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TRANSACTIONS

OF THE

INTERNATIONAL MEDICAL CONGRESS.

NINTH SESSION.

EDITED FOR THE EXECUTIVE COMMITTEE

BY

JOHN B. HAMILTON, M. D.,
Secretary-general.

VOLUME I.

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WASHINGTON, D. C., U. S. A.
1887.

PUBLISHED BY ORDER OF THE EXECUTIVE COMMITTEE.

JOHN B. HAMILTON, M.D.,
Secretary General of the Congress.

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ERRATA—VOLUME I.

Page 9. In remarks of Professor Semmola, line 8, for "Vœux" read Vœu; line 20, for "Chéric" read Chéri; line 21, for "frappant" read frappants.

Page 60. In second paragraph, line 9, for "keilförmigen" read keilförmigen.

Page 63. In paragraph 3, line 4, for "diagnostirenden" read diagnosticirenden.

Page 64. Paragraph 1, line 2 from end, for "Maase" read Maasse.

Page 65. Paragraph 2, line 2, for "zeichnen" read ziehen.

Page 66. Paragraph 2, line 9, for "Maasnahmen" read Maassnahmen.

Page 84. In speech of Dr. Landolt, 3d paragraph, 3d line, for "bénits" read bénis.

Page 89. In list of Delegates, for Dr. "Poussé" read Poussié.

Page 161. German title, for "Pathogenie" read Pathogenese.

Page 168. German title, for "croupartigen" read croupösen.

Page 184. German title, for "natürlich" read natürliche.

Page 194. For the German title, substitute the following: Neue experimentelle und klinische Untersuchungen über die Pathogenese der Albuminurie.

Page 195. German title, for "Phthisie" read Phthisis.

Page 196. French title, for "Sur Diabètes" read Sur le Diabète.

Page 205. Title, for "De Phthisie" read De la Phthisie; for "Etiologie" read Ätiologie.

Page 224. French title, for "Diphtheria" read Diphthérite; for "observé" read observée; for "Annees" read Années. In line 12 of Dr. Grant's article, for "Lawrentian" read Laurentian.

Page 225. German title, for "neuer" read neues; for "verbesserter" read verbessertes.

Page 231. French title, for "Sur Délatation" read Sur la Dilatation.

Page 239. 4th paragraph, last line, for "Decenium" read Decennium. Make same change in next paragraph.

Page 433. French title, for "De L'Abdomen" read Dans L'Abdomen. In German title read mit den Schusswunden, and for "und praktische Auszüge daraus" read nebst praktischen Schlussfolgerungen.

Page 435. German title, for "experimentaler" read experimenteller; for "zur Behandlung" read auf die Behandlung; for "der Verstopfung der Eingeweide" read des Darmverschlusses.

Page 485. No. 20, for "Intussciplens" read Intussusceptions.

Page 486. French title, for "operations" read opérations.

Page 536. French title, change so as to read Rapport D'un Cas de blessure de fusil dans L'Abdomen, etc. German title, change so as to read Bericht über einen Fall von Schusswunde des Unterleibes mit Durchlöcherung des Dickdarmes nebst Anmerkungen.

Page 563. French title, after "L'Articulation" read de la hanche.

Page 565. French title, for "De Cheval" read A Cheval. German title, for "Mastdarmfistern" read Mastdarmfisteln.

Page 572. In speech of Prof. Durante, 1st paragraph, last line, for "nuissable" read nuisible.

Page 573. French title, for "egauttage" read egouttage.

Page 595. French title, for "fractures pres au" read fractures près de, ou. German title, for "Fussschenkelschulter" read Fussschulter.

Page 606. Paragraph 1, last line, for "oblitreante" read obliterante. French title, for "in" read en. German title, for "Calculus" read Harnstein.

Page 617. French title, for "Intracapsulière" read Intracapsulaire. German title, for "knöchene" read knöcherne.

Page 658. German title, for "Bruchband" read Bauchbinde.

THE TRANSACTIONS

OF

THE NINTH

INTERNATIONAL MEDICAL CONGRESS,

HELD IN WASHINGTON, D.C., UNITED STATES OF AMERICA,

SEPTEMBER 5TH TO SEPTEMBER 10TH, A. D. 1887,

AND ALL DAYS INCLUDED.

FIRST DAY'S SESSION.

The Congress met at Albaugh's Theatre, on Fifteenth street, at 11 A. M. There were present on the stage, The President of the United States, The Honorable the Secretary of State, the Honorable the Speaker of the House of Representatives, the Executive Committee, and others.

The Chairman of the Executive Committee, Professor HENRY HOLLINGSWORTH SMITH, advanced to the footlights and said :—

It is probably known to all present, as well as to physicians throughout the world, that in May, 1884, representative members of the Medical profession in the United States decided to send a fraternal greeting to the Eighth International Medical Congress—then about to assemble in the Capital of Denmark—and ask that the Ninth International Medical Congress might meet in the City of Washington.

This invitation being accepted, an executive committee was named to make the necessary arrangements, and the result of their labors is seen in this large assembly, that the register shows contains many of the most brilliant and distinguished medical minds of Europe, Asia and America.

To welcome these guests of our profession, and show his interest in a great humanitarian object, the President of the United States has consented to open the Congress for organization, and I now have the honor to announce the Honorable GROVER CLEVELAND, President of the United States of America.

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THE PRESIDENT then came forward amid great cheering. When the applause had subsided, he said:—

I feel that the country should be congratulated to-day upon the presence at our capital of so many of our own citizens and those representing foreign countries who have distinguished themselves in the science of medicine, and are devoted to its further progress. My duty in this connection is a very pleasant and a very brief one. It is simply to declare that the Ninth International Medical Congress is now open for organization and for the transaction of business.

The Chairman of the Executive Committee then said:—

It is now my duty to present for your approval the names of the officers of the Congress agreed upon by the Executive Committee. For the high office of President of the Congress the Committee unanimously nominate to you one widely known as a scientific practitioner, an able teacher and medical author, Dr. NATHAN SMITH DAVIS, of Chicago. All approving of this nomination will say aye.

The motion was carried, with applause. Dr. SMITH requested Dr. FRANCESCO DURANTE, a delegate from Italy, and Deputy Surgeon-general MARSTON, of Her Majesty's service, to conduct the President-elect to the chair. Professor DAVIS then took his seat as President of the Congress.

The Chairman of the Executive Committee then said:—

MR. PRESIDENT, the Committee name to you as Secretary-general of the Congress Dr. JOHN B. HAMILTON, of Washington, Supervising Surgeon-general of the United States Marine Hospital Service.

The question was taken, and the vote appearing unanimously in favor of the nominee, he was declared elected.

Secretary-general HAMILTON then proceeded to nominate as Vice-presidents of the Congress the following:—

Dr. Alvarado, Mexico.
Dr. McCall Anderson, Glasgow, Scotland.
Dr. Thos. Annandale, Edinburgh, Scotland.
Prof. Georges Assaky, Bucarest.
Dr. Dujardin Beaumetz, Paris, France.
Dr. Cuthbert H. G. Bird, London, England.
Prof. Carl Braun, Vienna, Austria.
Dr. Wm. Brodie, Detroit, Michigan.
Prof. A. Charpentier, Paris, France.
Prof. A. Chervin, Paris, France.
Mr. Jno. Chiene, Edinburgh, Scotland.
Dr. Sinclair Coghill, Ventnor, England.
Dr. Wm. Coler, Berlin, Germany.
Dr. Wm. Wirt Dawson, Cincinnati, Ohio.
Dr. Thos. Michael Dolan, Halifax, England.
Dr. F. Dumont, Berne, Switzerland.
Dr. Francesco Durante, Rome, Italy.
Dr. Friederich Esmarch, Kiel, Germany.
Dr. Geo. J. H. Evatt, East Indies.
Dr. Joseph Ewart, Brighton, England.
Dr. L. Farkas, Buda Pesth, Hungary.
Prof. Léon Le Fort, Paris, France.
Sir B. Walter Foster, Birmingham, England.
Dr. T. R. Fraser, Edinburgh, Scotland.
Dr. A. Y. P. Garnett, Washington, D. C.
Dr. A. Pearce Gould, London, England.
Dr. J. A. S. Grant Bey, Cairo, Egypt.
Dr. J. A. Grant, Ottawa, Canada.
Dr. E. H. Gregory, St. Louis, Mo.
Sir Wm. W. Gull, London, England.
Dr. A. L. Gusserow, Berlin, Germany.
Dr. Nicolás José Gutierrez, Havana, Cuba.
Sir Jas. Arthur Hanbury, London, England.
Mr. Ernest Hart, London, England.
Dr. Hans R. von Hebra, Vienna, Austria.
Dr. Geo. M. Humphrey, Cambridge, England.
Dr. Jonathan Hutchinson, London, England.
Sir Wm. Jenner, London, England.
Dr. Fred. B. Jessett, London, England.
Dr. Theo. Kocher, Berne, Switzerland.
Dr. Leon Lablé, Paris, France.
Dr. Wm. Harris Lloyd, London, England.
Sir Thomas Longmore, Netley, England.
Dr. John D. MacDonald, Surrey, England.

Dr. Th. H. MacGillavry, Leiden, Netherlands.
Dr. Morrell Mackenzie, London, England.
Dr. Wm. Alexander Mackinnon, London, England.
Sir Douglas Maclagan, Edinburgh, Scotland.
Dr. Thos. J. Maclagan, London, England.
Dr. Geo. H. B. Macleod, Glasgow, Scotland.
Dr. John Marshall, London, England.
Dr. Jno. S. McGrew, Honolulu, H. I.
Dr. Wilhelm Meyer, Copenhagen, Denmark.
Dr. Withers Moore, Brighton, England.
Dr. Edward M. Moore, Rochester, N. Y.
Dr. O. Morisani, Naples, Italy.
Dr. Jeffrey A. Marston, London, England.
Dr. Karl von Mosengsil, Bonn, Germany.
Dr. Wm. Murrell, London, England.
Dr. W. D. Müller, Berlin, Germany.
Dr. Neudörfer, Wien, Austria.
Dr. Charles D. F. Phillips, London, England.
Mr. Richard Quain, London, England.
Sir John W. Reid, K. C. B., London, England.
Dr. Charles Reyher, St. Petersburg, Russia.
Dr. Tobias Richardson, New Orleans, La.
Sir Wm. Roberts, Manchester, England.
Dr. Saiga, Imperial Navy, Japan.
Dr. John B. Sanderson, Oxford, England.
Mr. Wm. H. Savory, London, England.
Dr. Lewis A. Sayre, New York City.
Dr. Mariano Semmola, Naples, Italy.
Dr. Leopold Servais, Antwerp, Belgium.
Sir Edward H. Sieveking, London, England.
Dr. Jos. R. Smith, U. S. Army.
Sir Wm. Stokes, Dublin, Ireland.
The Surgeon-general of the U. S. Army, Washington, D. C.
The Surgeon-general of the U. S. Navy, Washington, D. C.
Dr. Lawson Tait, F. R. C. S., Birmingham, England.
Sir Henry Thompson, London, England.
Dr. Tillman, Halmstadt, Sweden.
Sir John Tomes, Surrey, England.
Dr. J. M. Toner, Washington, D. C.
Dr. Trélat, Paris, France.
Sir Wm. Turner, Edinburgh, Scotland.
Dr. John Tweedy, London, England.
Dr. P. G. Unna, Hamburg, Germany.
Dr. Vallin, Paris, France.
Dr. J. E. de Vrij, Hague, Holland.
Dr. Waldeyer, Berlin, Germany.
Prof. F. Winckel, Munich, Bavaria.

THE PRESIDENT. Is the Congress ready to vote on nominations for Vice-presidents?

Dr. COHEN, of Philadelphia. Mr. President, I protest against having names of gentlemen placed on the list who are not present.

THE PRESIDENT. It is impossible at the present moment to say who are present. The gentlemen named have been communicated with, and the proceedings cannot, therefore, be interrupted. As many as are in favor of the election of the gentlemen named to be Vice-presidents of the Congress will say aye.

Aye, aye.

THE PRESIDENT. The motion prevails, and the Vice-presidents present will please take their seats on the stage.

THE SECRETARY-GENERAL. Mr. President, the Executive Committee have named as Associate Secretaries, Dr. WM. B. ATKINSON, of Philadelphia; GEORGE BYRD HARRISON, of Washington; and HENRY BANGA, of Chicago. For Treasurer, Dr. E. S. F. ARNOLD, of Newport. For Chairman of the Finance Committee, Dr. RICHARD J. DUNGLISON, of Philadelphia. For Chairman of Executive Committee, Dr. H. H. SMITH, of Philadelphia; and for Chairman of the Local Committee of Arrangements, Dr. A. Y. P. GARNETT, of Washington.

The question being taken, the gentlemen nominated were unanimously elected.

The Secretary-general then placed in nomination the gentlemen selected as Presidents of the Sections, as follows:—

SECTIONS.	PRESIDENTS.
I. GENERAL MEDICINE	A. B. ARNOLD, M.D.
II. GENERAL SURGERY	W. T. BRIGGS, M.D.
III. MILITARY AND NAVAL SURGERY	HENRY H. SMITH, M.D., LL.D.
IV. OBSTETRICS	DE LASKIE MILLER, M.D., PH.D.
V. GYNÆCOLOGY	HENRY O. MARCY, M.D.
VI. THERAPEUTICS AND MATERIA MEDICA	TRAILL GREEN, M.D., LL.D.
VII. ANATOMY	W. H. PANCOAST, M.D.
VIII. PHYSIOLOGY	J. H. CALLENDER, M.D.
IX. PATHOLOGY	A. B. PALMER, M.D., LL.D.
X. DISEASES OF CHILDREN	J. LEWIS SMITH, M.D.
XI. OPHTHALMOLOGY	J. J. CHISHOLM, M.D.
XII. OTOLOGY	S. J. JONES, M.D., LL.D.
XIII. LARYNGOLOGY	W. H. DALY, M.D.
XIV. DERMATOLOGY AND SYPHILOGRAPHY	A. R. ROBINSON, M.D.
XV. PUBLIC AND INTERNATIONAL HYGIENE	JOSEPH JONES, M.D.
XVI. CLIMATOLOGY AND DEMOGRAPHY	A. L. GIHON, A.M., M D., U.S.N.
XVII. PSYCHOLOGICAL MEDICINE AND DISEASES OF THE NERVOUS SYSTEM	J. B. ANDREWS, A.M., M.D.
XVIII. DENTAL AND ORAL SURGERY	JONATHAN TAFT, M.D., D.D.S.

The question being taken, the nominees were declared elected.

The Secretary-general then placed in nomination the names of the gentlemen selected as Vice-presidents, Secretaries, and Members of Council of the several Sections.*

The question being taken they were declared unanimously elected, and the President announced that the organization of the Ninth International Medical Congress was complete.

THE PRESIDENT. The report of the Secretary-general is now in order.

The Secretary-general then submitted the following report :—

MR. PRESIDENT: According to the precedent set at former sessions of this body, the Secretary-general must make a report of the work performed since the session last preceding, but I shall, in so doing, only occupy the time of the Congress for the briefest possible space.

It is now a matter of history that in May, 1884, the American Medical Association met in this Capital and passed a resolution inviting the Congress to honor America by holding its next session in the United States. At the meeting in Copenhagen, in August, 1884, when the question came up for disposition, Washington was selected.

The Committee having borne the invitation and secured its acceptance returned home and immediately began the work of organization. Shortly before the meeting of the American Medical Association in New Orleans, in May, 1885, the preliminary organization was completed. But it transpired that this Committee was unable to form an organization satisfactory to the majority of the members of the Association, and after some discussion a resolution was adopted which authorized the appointment of additional members of the Committee, so as to include, in accordance with our American system of representation, one member from each State and Territory, and to these were added one representative from each of the three public medical services. The enlarged committee met in Chicago, and a majority of the first committee was present and acted harmoniously with the new committee. In a short time, however, the members of the original committee withdrew, and the management was thus deprived of their valued services and experience.

* This list will be found in the volumes at the head of the respective Sections.

The Committee had, therefore, to contend against more than the ordinary difficulties attending so great an undertaking, and its success is due entirely to the zeal and energy of its Chairman, Prof. H. H. Smith, of Philadelphia, and the unflagging interest and industry of the remaining members. The time is too short to adequately speak of the multifarious labors attached to the office of the Secretary-general, but I have to report that at the present moment there remains no unfinished business on the Secretary-general's table. (Applause.) The work of organization is now complete, and I submit the programme and volume of abstracts. In regard to the programme, the rule requiring titles and abstracts to be furnished the Secretary-general on or before the 30th of April, was complied with in scarcely a single instance. The titles were, therefore, received too late to ensure entire accuracy either in translation or in proof-reading. It is impossible to perform such work so hurriedly without error. As to the abstracts, much of the manuscript did not reach me until within four days of the session!

A contract has been made for the printing of the "Transactions," and the work will be done as speedily as practicable.

For the social programme of this meeting I have to call on the Chairman of the Local Committee of Arrangements, who will announce it in detail.

The Chairman of the Local Committee of Arrangements (Dr. A. Y. P. GARNETT) then made the following report:—

MR. PRESIDENT: The Committee of Arrangements to whom was assigned the duty of formulating a programme for the social entertainment of the members of the Ninth International Medical Congress, beg leave to submit the following report:—

ON MONDAY EVENING, the 5th inst., the members of the Congress will assemble at the United States Pension Building, on G street between 4th and 5th. This informal meeting, beginning at 8.30 and ending at 11 P. M., will be held for the purpose of giving the members an opportunity of becoming acquainted with each other.

ON TUESDAY EVENING, the 6th inst., an informal reception will be held by the President of the United States, at the Executive Mansion, from 8 to 9 o'clock, for the members and their families, after which they will proceed to visit the Corcoran Art Gallery, at the corner of 17th and Pennsylvania avenue, which will be opened by order of the trustees for that purpose.

ON WEDNESDAY AFTERNOON, from four to six, there will be a lawn party at the residence of the Honorable Josiah Dent, Road street, Washington, to which the ladies are particularly invited. In the evening several private entertainments will be given by the citizens of Washington in honor of our foreign visitors, to be attended only by those who receive card invitations.

ON THURSDAY EVENING, the 8th, there will be a formal reception and banquet for all the members of the Congress and their families, at the U. S. Pension Building, between the hours of 8 and 11, when it is hoped that we shall be honored by the President of the United States and his Cabinet.

To those members who are accompanied by their families a cordial invitation, including the ladies, is extended for Friday afternoon, the 9th, to a reception at "Grasslands," the country residence of the Hon. Mr. Whitney, Secretary of the Navy. Carriages will be in readiness on G street, in front of the Riggs House, at 4.30 P. M., to convey them to that place.

ON SATURDAY, the 10th, there will be an excursion to Mt. Vernon, for the foreign members and their families only, on board the United States Steamer "Dispatch," which has been kindly placed at the disposition of the Committee by the Secretary of the Navy. Should the accommodations on the "Dispatch" prove insufficient for our guests, the use of the U. S. Revenue Steamer "Ewing" has been granted by the Hon. Secretary of the Treasury.

The Executive Committee of the Congress are respectfully invited to attend on this occasion as an escort to our guests. The steamer will leave the Navy Yard wharf at 10.30 A. M. An additional number of street cars will be placed in front of Willard's Hotel for the purpose of conveying the party to the Navy Yard.

An opportunity to visit Mt. Vernon will be afforded the American members each day during the week, at the reduced rate of seventy-five cents the round trip. The time of leaving will be published in the city papers.

In the AFTERNOON OF SATURDAY there will be an excursion train composed of sleepers, arranged to leave at 6.45, for the accommodation of our foreign guests and their families free of all expense, and those American members with their families who desire to avail themselves of this opportunity to visit Niagara and Watkin's Glen at the moderate sum of $14.00 the round trip, including all necessary personal expenses. A schedule of the hours of starting and arriving at various points on the route, together with a description of any points of interest, will be furnished on the train.

In order to avoid the possibility of any member being neglected through lack of personal identity, all are particularly requested to wear their badges during the entire session of the Congress.

It has been a source of profound regret and embarrassment to the Committee that the month of September should have been selected for this meeting, and we feel it due to ourselves and to the citizens of Washington to explain to you why there has not been a more general and spontaneous manifestation of that bountiful hospitality for which our city has at all times been so justly distinguished.

We therefore trust that our guests will not fail to understand and appreciate the unfortunate fact that they have come to a city absolutely and entirely deserted for the time by the larger portion of its inhabitants, and that those whose official and social positions entitled them to the privilege of extending to you a cordial welcome to their homes are necessarily deprived of that pleasure by absence. I will only add, gentlemen, that the labors of this committee have been truly herculean, and I embrace this occasion to return my thanks to the chairmen of the various sub-committees for the fidelity, zeal and patience displayed in the execution of their respective duties.

A. Y. P. GARNETT,
Chairman of Local Committee of Arrangements.

THE PRESIDENT. I now have the honor to introduce to you the Honorable THOMAS F. BAYARD, Secretary of State of the United States. He was greeted with great applause. He said:—

GENTLEMEN OF THE INTERNATIONAL MEDICAL CONGRESS:

The pleasing duty has been assigned to me of giving expression, in the name of my fellow countrymen, to the gratification felt by us all that you should have selected this Capital to be the scene of your Ninth Congress, and cordially bid you welcome.

The world is becoming better acquainted; social assimilation has progressed; small provinces and minor kingdoms are federalizing into great empires; international intimacy suffers less obstruction; the broad and powerful current of literature is silently wearing away the banks of geographical prejudice, and a spirit of a common brotherhood, of mutuality and independence, is expanding itself irresistibly over the barriers of mountain and sea; and these new and beneficent conditions give promise that the word "stranger" shall soon be obliterated from the vocabulary of civilization.

You, gentlemen, will not, I hope, feel—and I am sure you will not be considered by us—as strangers in the United States; for not only has the fame of many of your number—whom to name might seem invidious—long since surpassed the limits of

your own lands and been recorded in the world's annals of scientific attainment, but I take leave to say that here especially will your claims for public respect and grateful acknowledgment, due to your enlightened services, find prompt and hearty allowance by the populations who dwell amid the blessings of civil and religious liberty beneath the broad banner of these United States.

If letters be a republic, science is surely a democracy, whose domain is penetrated and traversed by no royal road, but is open on all sides and equally to all who with humility and intelligence shall watch and wait for light as it is gradually disclosed by Divine Providence for the amelioration of mankind.

In this democratic Republic the brotherhood of science can best realize its universality; for here you will find institutions for the promotion of science in every department,—and in none more conspicuously than in that of medicine and surgery;—the most important of which are the voluntary gifts of private citizens, men who, in the majority of cases, were painfully limited in their associations with science and letters, who began life at the lowest round of fortune's ladder; but, thanks to the noble equities of our political system, rose without "invidious bar" to the highest level of material success and public usefulness.

To the public spirit and benevolence of such individuals is due the endowment, on a scale that princes may envy but have never surpassed, of schools of science, colleges and universities, open for the intellectual training and advancement of all who desire to share and are competent to receive such benefits.

Your Congress is held, gentlemen, in the closing year of the first century of our national existence, and what has been here accomplished in the line of scientific edification and equipment owes comparatively little to official or governmental assistance. To no system of prescriptive privilege, but to individual energy, enterprise and generosity we owe what, under God, we now possess of such things, and non-interference by the Government has proved a promotion and not a hindrance in our advancement. Busy in every department of industrial pursuit, engrossed with diversified occupations, and hurrying with a breathless energy that has left its traces upon the physiognomy of our people, yet, believe me, we are not deaf to the calls of humanity nor lacking in appreciation and grateful respect for the votaries of science.

We welcome this Congress as guardians of the sanitation of the nations. In your profession we recognize the noblest school of human usefulness, and in the progress of the development of the laws of cure, the mitigation of suffering, the prolongation of human existence, and the efforts to discover the true principles and conditions by which life can be made "worth living," we have learned to appreciate our debt to those whose highest reward is the "still small voice of gratitude" and consciousness of benefaction to the human race. Gentlemen, I confidently promise your Convention a worthy audience—not alone the members of your profession here assembled nor the limited number whom this building can contain, but that vaster audience to whom, upon the wings of electrical force, your message will be daily borne far and wide to the listening ear of more than sixty millions of American citizens.

Sure am I that your message will be worthy, and equally that your thoughtful deliverances will be welcomed by a continent.

The closer relations of mankind which modern invention has induced has been necessarily accompanied by an increased dissemination of disease, and the need is obvious of frequent international conference, that, in the grand sweep of scientific observation, new discoveries in the healing art may be promptly tested and applied in counteraction.

Forgive me if, as one of the great army of patients, I humbly petition the profes-

sion, that in your deliberations Nature may be allowed a hearing when remedies are proposed; that her *vis medicatrix* may not be omitted in computing the forces of cure, and that Science may be restricted as often as possible to sounding the alarm for Nature to hasten, as she surely will, if permitted, to the defense of the point assailed.

My duty is very simple, and I fear I have already overstepped its limit, for there was indeed little more for me to say than to repeat the words of an ancient dame whose cottage was close to the battlefield of Waterloo, and being somewhat deaf, and hearing the sound of the artillery when the famous "pounding" was hardest, thought she heard some one knocking at her door, and simply said, "Come in!" This may seem an unscientific illustration of auscultation and percussion, but you need not make half the noise of Wellington or Bonaparte, and I can assure you the American people will hear you and heartily say to you, as I do for them, "*Come in!*" (*Great applause.*)

After the Honorable Mr. Bayard had concluded, Professor PAVY, of London, was called for, but owing to his not having received previous notice, he was absent from the stage.

Inspector-general LLOYD, of H. M. Royal Navy, was then called for, and responded as follows:—

MR. PRESIDENT: I rise to perform the agreeable task which has been allotted to me of returning thanks on behalf of the medical profession of Great Britain and Ireland, as represented by my professional brethren and myself now present at this Congress, for the warm and eloquent welcome we have just heard from the Honorable Secretary of State of the United States.

It is with great diffidence I rise to perform this task in the presence of the eminent men of world-wide reputation in medical science who are now present among the English members of the Congress, and I could not have felt justified in undertaking it did I not know that my selection for the task was due to my official position as representing one of the public services of Great Britain.

I have now to express our warm thanks and appreciation of the kind and cordial welcome we have received from the Congress, and the honor conferred by the presence and approval of the high officers of State of this truly great country.

Professor LÉON LE FORT said:—

M. LE PRÉSIDENT, M. LE MINISTRE:

Je viens au nom de mes compatriotes vous remercier de vos bienveillantes paroles:—

Nous avons voulu en traversant l'Atlantique apporter à nos collègues Américains le témoignage de notre sympathie. La réception qu'ils nous ont faite à Philadelphie nous a déjà procuré que nous pourions compter sur la leur.

En désignant Washington pour le siège du 9e congrès international, les médecins Européens ont voulu affirmer leur haute estime pour la médecine Américaine. Ce n'est pas le lieu de rappeler les progrès dont nous sommes redevables; mais nous pouvons rappeler que c'est à l'Amérique que nous devons une des plus grandes découvertes de la science moderne: l'anesthésie. Avec elle, on n'a pas seulement supprimé la douleur, on a rendu possibles des opérations qui sans elle seraient impraticables. Le congrès de Washington sera digne de ceux qui l'ont précédé et contribuera aussi au progrès de la science.

Daignez, M. le Président, M. le Ministre agréer les respects et les remerciements des médecins Français présents au Congrès.

Professor UNNA said :—

HERR PRÄSIDENT, MEINE HERREN !

Ich danke Ihnen herzlich im Namen meiner deutschen Collegen für die überaus freundlichen Worte, mit denen Sie uns begrüsst haben. Die deutschen Aerzte, welche die Vereinigten Staaten besuchen, haben das angenehme Gefühl, dass sie sich hier vollständig heimisch fühlen können. Nicht nur, dass wir durch das ganze Land verbreitet eine grosse Anzahl deutscher und in Deutschland gebildeter Collegen in allen Zweigen der Medizin thätig finden, wir wissen und wir fühlen es mit Stolz, dass hier deutsche Wissenschaft getrieben wird, dass deutsche Wissenschaft hier Wurzeln geschlagen und reiche Früchte getragen hat. Indem ich Sie versichere, dass wir Deutschen, die wir hierher gekommen sind, mit Freude unser Bestes zu diesem grossen internationalen Congress beisteuern werden, danke ich Ihnen noch einmal für die schönen gastlichen Worte, mit denen Sie uns empfangen haben.

Professor SEMMOLA said :—

Je suis heureux, M. le Secrétaire d'État d'avoir l'honneur de vous répondre au nom d'Italie pour vous apporter le salut de cette jeune et grande nation qui suit avec le plus vif intérêt les merveilles croissantes de ce grandiose édifice de l'indépendance des États Unis d'Amérique.—Et lorsque je dis les merveilles, j'entends parler non seulement du travail, de l'industrie, et de la politique, je veux à mon point de vue, parler aussi des progrès considérables que la médecine et la chirurgie ont su réaliser depuis plus de quarante ans avec des découvertes et des méthodes très utiles à l'humanité. Je vous avoue que le plus ardent vœux de mon cœur aurait été de pouvoir vous apporter avec le salut de mon Gouvernement la douceur de la langue de ma patrie ; mais je comprends que cela aurait beaucoup trop augmenté les difficultés de nos debats scientifiques.—Je sens le devoir de vous remercier de l'accueil si aimable et si bienveillant que vous nous avez fait aux confrères d'Italie et à moi. Cet accueil ne pouvait pas manquer de la part d'un peuple qui à côté des trésors de l'intelligence et du travail a toujours donné le plus libre essor à l'hospitalité comme apanage naturel de la noblesse et de la générosité de son cœur.—Je crois que les congrès scientifiques internationaux sont incontestablement un des moyens pour affirmer et souder la liberté, l'égalité et la fraternité des peuples. Mais parmi tous les congrès internationaux auxquels j'ai eu l'honneur de représenter l'Italie, celui de Washington, je pense, pourra exercer une des influences les plus bienfaisantes, parceque il a lieu sur le sol chérie de l'indépendance et il pourra, je veux bien me flatter, donner un des exemples plus frappant de la puissance invincible de l'humanité, lorsque elle marche avec la science alliée à la liberté, pour en arriver à cette grande œuvre de l'*Union de la paix sociale* qui fut inspirée dans les États-Unis et qui sera toujours le plus éclatant miroir du patriotisme de ce grand peuple.

Dr. CHAS. REYHER, of St. Petersburg, said :—

MR. PRESIDENT AND GENTLEMEN : I would not dare to speak here if I had not been invited to do it. If I should speak Russian I would not be understood ; and if I attempt to speak English, not knowing the English language, I am fearful I should be found in the same position. Therefore, allow me to speak only few words. I am convinced that, like me, all who came here expected to see and to learn much ; but not less am I convinced that all have seen and learned much more than we hoped. Allow me to thank you for it ; and if I were authorized by my Government I do not doubt that I should express their thanks also. Let these be my few words of thanks.

President NATHAN SMITH DAVIS then called Vice-president LEWIS A. SAYRE to the chair and delivered the following inaugural address :—

GENTLEMEN : It is my first sad duty to remind you that death has removed from among us one to whom, more than to any other, we are indebted for the privilege of having the Ninth International Medical Congress in America. One whose urbanity, erudition, valuable contributions to medical literature and eminence as a teacher, caused him not only to be universally regarded the most influential leader in all the preparatory work, but also the one unanimously designated to preside over your deliberations on this occasion. That one was the late Professor Austin Flint, of New York, who was taken suddenly from his earthly labors, early in 1886, before the work of preparation for this Congress had been half completed. The true nobility of his private and professional character, his eminent ability as a teacher, and, above all, the extent and value of his contributions to the literature and art of medicine, had caused him to be known and esteemed by the profession in all countries. And, as you all remember, while the shock of his death was fresh upon us, our loss seemed well-nigh irreparable. But, though he has taken his departure ripe in years and full of honors, yet the influence of his excellent example and his contributions to medical science remain, and will continue to exert their beneficent influence through all the generations to come.

With a full consciousness of my own deficiencies and still with a heart overflowing with gratitude, I thank you for the honor you have bestowed in selecting me to preside over the deliberations of this great and learned assembly. It is an honor that I appreciate as second to no other of a temporal nature because it has been bestowed, neither by conquest nor hereditary influence, nor yet by partisan strife, but by the free expression of your own choice.

Addressing myself now more directly to those here assembled, who have left home and loved ones in other lands and encountered the fatigue and dangers of traveling by sea and by land, in the name of the Medical Profession of this country I welcome you, not only to this beautiful city and the hospitality of its citizens, as has been so admirably done already by the honorable representative of the Government who has just taken his seat, but I cordially welcome you to the *whole country*, in whose name you were invited here three years since, and whose representatives are now here, side by side with you, gathered from the East, the West, the North, the South, as well as from the rugged mountains and fertile valleys of the Centre, to make good the promise implied by that invitation.

If they do not cause you to feel at home and happy, not only in the social circles and halls devoted to the advancement of science, literature and art in this city of our nation's pride, but wherever you may choose to roam, from the rocky coast of New England on the Atlantic to the Golden Gate of the Pacific, it will be from no want of earnest disposition to do so.

And now, I not only thus welcome you from other lands, but I take great pleasure in greeting you one and all as leading representatives of a profession whose paramount object is the lessening of human suffering, by preventing, alleviating, or curing diseases wherever found, and in whatever class or grade of the human family. Nay, more, with profound reverence I greet you as a noble brotherhood, who in the practical pursuit of that one grand object, recognize no distinction of country, race or creed, but bind up the wounds and assuage the pains of the rich and poor, ruler and ruled, Christian and pagan, friend and foe alike.

Not that every medical man does not love and defend his own country and fire-

side with as fervid a patriotism as the members of any other class of men. But as disease and pain are limited to no class or country, so is the application of his beneficent art limited only by the number of those suffering within his reach.

With a common object so beneficent in its nature, and opportunities for its practical pursuit so universal, it is but natural that you should be found searching for the most effectual means for the accomplishment of the one object of lessening human suffering, in every field of nature and every department of human knowledge.

The living human body—the chief object of your solicitude, not only combines in itself the greatest number of elementary substances and the most numerous organs and varied functions, so attuned to harmonious action as to illustrate the operation of every law of physics, every known force in nature, and every step in the development of living matter, from the simple aggregation of protoplasm constituting the germinal cell to the full-grown man, but it is placed in appreciable and important relations with the material and immaterial forces existing in the world in which he lives.

Hence a complete study of the living man, in health and disease, involves a thorough study, not only of his structure and functions, but more or less of every element and force entering into the earth, the air and the water with which he stands in constant relation.

The Medical Science of to-day, therefore, embraces not only a knowledge of the living man, but also of such facts, principles and materials gathered from every other department of human knowledge as may increase your resources for preventing or alleviating his suffering or prolonging his life.

The time has been, when medical studies embraced little else than the fanciful theories and arbitrary dogmas of a few leading minds, each of which became for the time the founder of a sect or so-called school of medicine, with his disciples more or less numerous. But with the development of general and analytical chemistry, of the several departments of Natural Science, of a more practical knowledge of physics, and the adoption of inductive processes of reasoning, the age of theoretical dogmas and of medical sects blindly following some more plausible leader passed away, leaving but an *infinitesimal* shadow yet visible on the medical horizon.

So true is this, that in casting our mental vision, to-day, over the broad domain of medicine we see its votaries engaged, some searching for new facts and new materials; some studying new applications and better uses of facts and materials already known; some of them are in the dead house with the scalpel and microscope, not only studying the position and relations of every part, from the obvious bones and muscles to the smallest leucocyte, in health; but also every deviation caused by morbid action or disease. Some are searching the fields, the forests, the earth and the air, both for more knowledge concerning the causes of disease and for additional remedial agents; some are in laboratories with crucible, test glass and microscope, analyzing every morbid product and every remedial agent, separating the active principles from the crude materials and demonstrating their action on living animals, while far the greater number are at the bedside of the sick and wounded, applying the knowledge gained by all other workers to the relief of human suffering. A more active, earnest, ceaseless and beneficent field of labor is not open to your vision in any other direction or occupied by any other profession or class of men. And thus has the Science of Medicine become a vast aggregation of observed facts, many of them so related to each other as to permit practical deductions of permanent value, while many others remain isolated through incompleteness of investigations, and therefore liable to prompt, hasty or even erroneous conclusions.

Indeed, the most defective and embarrassing feature in the Science and Art of Medicine, at this time, is the rapid accumulation of facts furnished by the vast number of individual workers, each pushing investigations in some special direction without concert with his fellows, and without any adequate conception of the coincident lines of observation necessary to enable him to see the true bearing of the facts he evolves. Hence he is constantly mistaking mere coincidences for the relation of cause and effect, and the pages of our medical literature are being filled with hastily formed conclusions and rules of practice, from inadequate data.

This results, in part at least, from the extent and variety of the fields of inquiry and the complexity of the problems presented for solution. For nowhere else within the realms of human thought does the mind encounter problems requiring for their correct solution the consideration of a greater number of data, than in the study of etiology and pathology. To determine the appreciable conditions of the earth, air and water of any country before, during and after the invasion of an epidemic disease long enough to include several consecutive visits of the same, is not possible for a single individual, nor for any number of observers acting separately or without concert.

Yet just this complete knowledge is necessary to enable us to separate the conditions that are merely coincident or accidental from those that are such constant accompaniments of the disease as to prove a necessary relation between them. And it is only by such persistent, coincident, systematic observations of many individuals, each having a definite part, and the results carefully compared analytically and synthetically at proper intervals, that the real conditions and laws controlling the prevalence and severity of epidemics can be clearly demonstrated. It is not enough to discover the primary infection, or the *contagium vivum*, whether it be the bacillus of cholera, yellow fever, or tuberculosis, for abundant experience has shown that not one of these will extend its ravages into any community or country unless it finds there a soil or pabulum congenial for its support and propagation.

It is on the development and diffusion of knowledge concerning the local conditions necessary for receiving and propagating the specific infections of disease that nearly all the important sanitary measures of modern times have been based. And it is on a further development of knowledge in the same direction, gained by more systematic, continuous and coincident investigation, that we shall most successfully protect our race from the pestilences that have hitherto "walked in darkness and wasted at noonday."

It was an extensive and ever extending field of medical science, the complexity of the problems pressing for solution, and still more the individual responsibility of applying the resources at command to the direct treatment of disease, that early disposed medical men to seek each other's counsel, to form groups or clubs for comparison of views and mutual improvement. The manifest advantages of these soon prompted more extended social gatherings, until at the present time a large proportion of the more active members of the profession in every civilized country are participating in municipal, district, National and International medical organizations.

The aggregate benefit derived from all this active intercourse is beyond easy expression in words. In the more frequent and familiar comparison of cases and views on all professional subjects in the local societies, closer habits of observation and a wider range of thought are induced, while narrow prejudices and bigotry give place to generous rivalry and personal friendships. In the larger gatherings, the formal preparation of papers and reports on a great variety of subjects impels their

authors to a wider range of study and greater mental discipline, while the collision with other minds in discussion brings all aspects of the subject to view, enlarging the scope of mental vision, starting new trains of thought, and begetting a broader and stronger mental grasp with purer and nobler aims in life.

I think I am justified in saying that no other one influence operative in human society during the present century has done as much to develop and diffuse medical knowledge, to stimulate its practical and successful application, both in sanitary measures for preventing disease and in the direct alleviation of suffering at the bedside, and in unifying and ennobling the profession itself, as has been accomplished by the aggregate medical society organizations of the world. Yet their capacity for conferring other and, perhaps, still greater benefits, under proper management, will have become manifest in the near future. And that I may accomplish the chief object of this address, I must ask your indulgence while I indicate some of the more important additional benefits in advancing medical science and saving human life through the instrumentality of our medical society organizations, and the methods by which they may be accomplished.

Every experienced and intelligent practitioner of the healing art is familiar with the fact that all acute general diseases are influenced in their prevalence and severity by seasons of the year, topographical and other conditions of the earth, meteorological conditions of the atmosphere, and the social conditions and habits of the people themselves. The most familiar endemics vary annually in the same localities, while the great epidemics that have for ages broken over the comparatively limited boundaries of their habitats only at intervals of years, and extended their ravages from country to country and receded again to the source from which they apparently originated, differ widely in the different periods of their prevalence. But in studying the essential causes of any one of these general diseases and the laws and conditions under which such causes operate, he soon finds certain factors, essential for the solution of his problems, wanting.

For instance, if he wishes to identify the date of the first attack of epidemic cholera in a given locality, and the character of bowel affections immediately preceding, the ordinary statistics of mortality will give him only the date of death, which may have been from one to seven days later, or it may have been preceded by one or more cases that recovered. If he is anxious to determine the reason why the disease, on entering one community, develops with such rapidity that in a few days its victims are found in every grade of the population and in almost every street, while in another it develops slowly, adhering persistently to particular classes or localities, he may find in the ordinary meteorological records the thermometric, barometric and hygrometric conditions of the atmosphere, with the direction and the velocity of the winds, but he finds nothing regarding those important though variable elements known as ozone and hydrogen peroxide, active oxidizers; or those nitrogenous products called free and albuminoid ammonia. Neither do the sanitary records give the desired information concerning the composition and impregnations of the soil, or of the organic and inorganic emanations that may arise therefrom.

An adequate knowledge of these absent factors relating to the condition of the earth, air and water over districts large enough to embrace localities subject to invasions of the epidemics and others known to be exempt, through a sufficient length of time to cover several periods of prevalence and periods of absence alike, is essential for enabling us to comprehend the causes that make one district amenable to the prevalence of a disease and another not, as well as the marked differences in the severity and mode of progress of the same disease at different periods in the same

localities and same classes of the people. The same additional knowledge would also furnish the basis for further sanitary measures of the greatest practical value.

And yet it must be obvious that the coöperation of numbers of medical men directly engaged in the field of general practice, with others possessed of more practical facilities for chemical and microscopical research, is necessary for successfully prosecuting such coincident and continuous investigations as would be likely to secure the desired results. Only well-trained general practitioners in every locality chosen for observation could observe and record the date of the initial symptoms of acute general diseases coming under their notice, and at stated intervals collate and report them to a central committee. The daily observations concerning the presence and relative proportion of active oxidizers and of nitrogenous organic elements in the atmosphere and the water, would require the selection of one or two experts in chemical and microscopical research for each locality; all making their observations coincidently in time and by uniform methods.

There are included in the organized medical association of each country the men and materials necessary for prosecuting every well-defined line of inquiry; and these associations, by their stated meetings and their facilities for inter-communication and concert of action, present the entire machinery needed and are only waiting for well planned and systematic use.

The tendency to make the permanent medical organizations available for prosecuting work in the directions I have indicated has already been manifested to a limited extent, as may be seen in the formation of the Collective Investigation Committee of the British Medical Association and of the International Collective Investigation Committee, organized during the sitting of the Eighth International Congress at Copenhagen.

An earlier movement, more fully of the character I have been endeavoring to explain, was made by the American Medical Association in 1875* when a standing committee was appointed to establish in a sufficient number of localities regular coincident daily observations and records concerning all appreciable meteorological conditions, including organic and inorganic elements found in the atmosphere, and the date of beginning of acute general diseases, and report the result at each annual meeting of the Association.

The Committee made reports embodying facts of interest and permanent value in 1877†, in 1879‡, in 1881§, in 1882‖, and in 1883¶. The latter report contains, among other items, a complete tabulated statement of the free and albuminoid ammonia in the atmosphere for every day in the year ending Aug. 31, 1883, as determined for the Committee by Prof. J. H. Long in connection with the laboratory of the Chicago Medical College. The Committee is still prosecuting its work, with material in hand for a still more important report at an early day. The greatest difficulty encountered has been to enlist a sufficient number of active practitioners in each locality who would faithfully record the desired clinical facts and report the results to the Committee. But this and all other obstacles can be overcome by persevering and well-directed work.

I trust no apology is needed for having embraced this occasion to attract your attention to the very important question how to make all our Medical Associations more useful in promoting the science of medicine by more complete methods of

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* See *Trans. American Medical Association*, Vol. 26, p. 125. † *Ibid.*, Vol. 28, p. 153.
‡ *Ibid.*, Vol. 30, pp. 38-147. § *Ibid.*, Vol. 32, p. 481. ‖ *Ibid.*, Vol. 33, p. 43.
¶ See *Journal of American Medical Association*, Vol. 2, pp. 85 and 169.

investigation, especially in directions where the coincident action of several persons in different places is essential for success.

I fully appreciate the great benefit resulting from the simple mingling of large numbers of medical men in social contact, where each is made to hear constantly, whether on the street, in the hotel or the assembly room, new suggestions, new modes of expression, and to observe the physical and mental effects of the various habits and customs of the different peoples, until each one leaves the general gathering with largely increased mental activity and resources, as was so happily expressed by Sir James Paget in his address to the Congress of 1881, in London. And I appreciate in a still higher degree the benefits derived from the preparation and reading of papers by individuals and the discussion of important questions, in all our assemblies.

But for reasons I have already briefly stated, I hope to see added in every permanent general medical society two standing committees; one to whom should be referred for critical examination every communication claiming to embody a new discovery in either the Science or Art of Medicine; and the other should be charged with the work of devising such lines of investigation for developing additional knowledge as require the coöperation of different individuals, and perhaps societies, and of superintending their efficient execution until crowned with success.

If ten or twenty per cent. of the money paid for initiation and membership dues by the members of each society were appropriated and judiciously expended in the prosecution of such systematic and continuous investigations from year to year, it would accomplish more in advancing medical science directly, and indirectly in benefiting the human race, than ten times that amount would accomplish if expended in any other direction.

For it must be remembered that when money is expended for material objects, even for food, clothing or medicine, such materials feed, clothe or relieve but one set of needy individuals, and are themselves consumed; but the expenditure of money and time in such a way as to develop a new fact capable of practical application either in preventing, alleviating or curing disease, that *fact* does not, like the food or medicine, perish with the using, but it becomes literally imperishable. Neither are its benefits limited to one set of individuals, but it is transmitted with the speed of the lightning, over the land and under the sea, to every civilized people; and whatever benefits it is capable of conferring are as capable of being applied to a million as to one, and of being repeated with increasing efficiency from generation to generation.

It has been tersely and correctly stated that associated action constitutes the characteristic and predominating power of the age in which we live.

It is by associated action that education in its broadest sense, religion and civilization, have been more rapidly diffused among the masses of mankind during the present century, than during any other period of the world's history.

It is by the association of capital, wielded by the associated intellects of the nineteenth century, that highways of commerce have been opened over the valleys, through the mountains, across the deserts, and on the oceans, over some of which the material productions of the nations are borne by the resistless power of steam, and along others the products of mental action are moved with the speed of electric currents, until both time and space are so far nullified that the most distant nations have become neighbors, and the inhabitants hold daily converse with each other from opposite sides of the globe.

Indeed, it is only by means of such of these highways as have been constructed

within the memory of him who addresses you, that you have been gathered in this hall from the four quarters of the earth, and through which an account of your doings may be daily transmitted to your most distant homes.

I congratulate you on the fact that the profession you represent has taken the lead of all other professions or classes of men, in rendering available these grand material achievements of the age, for cultivating fraternal relations, developing and interchanging knowledge, and planning concerted action for rendering human life everywhere healthier, happier, and of longer duration.

This is the Ninth Grand International Congress in the regular series, within little more than two decades, and let us hope that all its work will not only be done in harmony and good order, but with such results as will add much to the aggregate of human happiness through all the coming generations.

Without trespassing further on your patience, I must ask your forbearance with my own imperfect qualifications, and your generous assistance in the discharge of the responsible duties you have devolved upon me.

The general session then adjourned.

The Sections met at 3 P. M., and in the evening a *conversazione* was held at the Pension Hall.

SECOND DAY.

The Congress again met in general session at 10 A. M., Tuesday, September 6th, 1887. The President announced as the first business in order the reading of the address of Professor AUSTIN FLINT, on the subject of "Fever: its Cause, Mechanism and Rational Treatment."

FEVER: ITS CAUSE, MECHANISM AND RATIONAL TREATMENT.

BY PROF. AUSTIN FLINT, M. D., LL. D.,

Of New York.

In the classical monograph on inanition, by Chossat, published in 1843, is the following sentence: "Inanition is a cause of death which marches in front and in silence in every disease in which alimentation is not in a normal condition."*

A few years later, Graves, of Dublin, insisting upon the importance of alimentation in the management of continued fever, said that if he had met with more success than others in the treatment of the disease, it was owing, in a great degree, to the counsel of a country physician of great shrewdness, who advised him never to let his patients die of starvation.

Nearly half a century has elapsed since Chossat, comparing the results of a series of elaborate experiments on the lower animals with pathological phenomena in the human subject, recognized inanition as a cause of death in diseases which were then treated by depletory and so-called antiphlogistic measures, and since the "shrewd country physician" advised Dr. Graves never to let his patients die of starvation. Within this half century the ideas embodied in the two quotations I have made have taken a permanent place among the accepted principles of the science of medicine. The researches of the physiologist enabled him to recognize the spectre of inanition, marching "in front and in silence" with disease, and the great clinical observer "fed fevers;" the natural history of many diseases, undisturbed by active therapeutical measures, has been studied, and the self-limited character of a large number of these diseases has been established; and now, in the treatment of certain cases, abortive measures having been found ineffectual, the resisting and recuperative powers of patients are sustained.

An important result of the studies of physiologists with reference to animal heat, and of pathologists, with reference especially to the essential fevers, is that, important organs being protected against serious complications and accidents, and the nutrition of the body being measurably supported, a fever may run its course, leaving the patient in a physical condition in which speedy and complete convalescence is almost certain. The life of an acute disease usually is short; and a self-limited disease, such as typhoid fever, is a morbid force which calls for resistance on the part of the system for but a certain time. In cases of acute disease, as a rule, there is an inherent

* Chossat: "Recherches expérimentales sur l'inanition," Paris, 1843, p. 194.

tendency to recovery. A disease which involves in its course a rapid and abnormal consumption of matter within the body can rationally be met by the introduction and assimilation, if possible, of nutritive material to save or repair the destruction of tissue. That there is abnormal destruction of tissue in fevers is rendered certain by the progressive loss in body weight and the marked increase in the elimination of carbonic acid and nitrogenized excrementitious matters; and it is the province of the physician to keep this loss within the lowest limits, and to repair it as speedily as possible.

The principal object of this address will be to show how the metamorphosis of matter involved in the normal production of animal heat is accomplished, how the abnormal production of heat in fever, involving, as it does, abnormal activity in the metamorphosis of tissue, may be restricted, and how abnormal destruction of tissue may be limited and repaired.

It is well known to physiologists that the production of animal heat is one of the phenomena attendant upon the general processes of nutrition. It is also well known that the process with which the production of heat is most intimately connected is oxidation of certain matters which are either contained in food or form a part of the tissues of the body. This fact, a knowledge of which dates from the researches of Lavoisier, in the latter part of the last century, has now become firmly established; and the relations between the consumption of oxygen—with the production of certain excrementitious matters and the generation of heat within the body—have, in late years, been the subject of much physiological experimentation. Attempts have been made by Senator and others to measure directly the quantity of heat produced in the body, with the result of showing that in mammals there are produced about four heat units per hour, per pound weight of the body.* According to this estimate, a man weighing one hundred and forty pounds would produce 13,440 heat units in twenty-four hours.

While the direct method of estimating the heat produced by the body has some elements of uncertainty, it has the advantage, at least, of being similar to the method by which the heat value of food has been ascertained. On the other hand, the indirect method, employed by some observers, which is said by Dr. Foster to be "as trustworthy as any," seems to me to involve such possibly enormous errors as to be of little value. This method consists in "simply subtracting the normal daily mechanical expenditure from the normal daily income. Thus, 150,000 kilogramme-metres subtracted from 1,000,000 kilogramme-metres gives 850,000 kilogramme-metres as the daily expenditure in the form of heat." † In this method the only reasonably accurate element in the calculation is the "normal daily income," which is ascertained by estimating the heat value of food in a normal diet. The calculation of the "normal daily mechanical expenditure," is inaccurate almost to the

* The English heat unit represents the heat required to raise the temperature of one pound of water one degree Fahrenheit. The Continental heat unit represents the heat required to raise the temperature of one litre of water one degree Centigrade. One heat unit, Continental, equals about four (3.9628) heat units, English. Four heat units per pound per hour (English) would equal about one heat unit (Continental) per pound per hour, or 2.2 heat units (Continental) per kilogramme per hour. In what is to follow, I shall adopt the English standard for the heat unit.

† Foster: "Text-book of Physiology." London, 1883, p. 459. A kilogramme-metre, or a kilogramme lifted a metre, is equal to 7.232 foot pounds, or pounds lifted a foot. (Pavy: "Food and Dietetics." Philadelphia, 1874.)

degree of absurdity. The force expended in the circulation and respiration is estimated, the force in locomotion and in other muscular work is guessed at, and all of these estimates of expenditure of energy are calculated in foot pounds, or kilogrammemetres, which are afterward reduced to heat units. Taking the estimate of the work of the heart alone, if Dr. Haughton's calculation of the quantity of blood discharged by each ventricular systole, which is three ounces, be accepted, a certain result is arrived at; while, if we accept—and on equally good authority—the view that the quantity discharged is between five and six ounces, the figures are nearly doubled, and the error is multiplied by about 100,000 beats in twenty-four hours.* The estimates, also, of the force used in respiratory movements, locomotion, etc., are not more reliable; and while it is admitted that one pound weight falling 772 feet will turn a wheel the friction of which will raise the temperature of one pound of water one degree Fahrenheit, no one has been able to raise one pound weight to the height of 772 feet, or 772 pounds to the height of one foot, by using the heat which will raise the temperature of one pound of water one degree. In the calculations applied to physiological processes, it is always the heat produced which is converted into force; and in fixing the formula for such calculations, it is always the force which is converted into heat. It seems to me, therefore, that the most inaccurate of the direct methods of estimating the heat produced in the body is more useful than any calculations which involve such enormous sources of error as must exist in the estimates of daily mechanical expenditure. However, in the reflections that are to follow, I shall have nothing to do with the conversion of heat into force, but shall confine myself to the questions involved in the normal and abnormal production of heat in the human body.

The late Dr. John C. Draper, following the experiments by Senator and others on the inferior animals, made a series of observations on his own person, in which he showed that his body, which he found was equal in bulk to three cubic feet, was capable of raising the temperature of three cubic feet of water five degrees Fah. in an hour. During the observation, the temperature under the tongue was reduced one degree.† Making the correction — which was not made by Dr. Draper — of one degree lost by the body and imparted to the water,‡ I estimated that the body produces four heat units per pound weight per hour, which is nearly the result obtained by Senator, and that, consequently, a man weighing 140 pounds would produce 13,440 heat units in twenty-four hours in a condition of absolute repose. This quantity would, of course, be increased by muscular exercise.

In a series of experiments made on my own person for twenty-four hours, under a liberal diet, I calculated the heat value of the food ingested as equal to 14,979.15 heat units. At that time (1878) I weighed 186½ pounds, and, according to my estimate, I produced 17,880 heat units in twenty-four hours. There was no difference in the body weight at the beginning and at the end of the observation.§ This observation showed that nearly one-sixth of the heat estimated as actually produced by the

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* Flint: "The Source of Muscular Power," N. Y., 1878, p. 70.

† Draper: "The Heat Produced by the Body, and the Effects of Exposure to Cold," *American Journal of Science and Arts*, New Haven, December, 1872. Dr. Draper's experiments were made under conditions which possibly involved serious inaccuracies; and they are useful and interesting chiefly from the correspondence of the results with those obtained by Senator and others, in which corrections were carefully made.

‡ Flint: "Experiments and Reflections upon Animal Heat," *American Journal of the Medical Sciences*, Philadelphia, April, 1879, p. 343.

§ *Ibid.*, p. 355.

body was not accounted for by the heat value of the food taken. There can be little question with regard to the accuracy of the accepted methods for estimating the heat value of articles of food, and it follows, logically, that there must either be a grave error in the estimate of the heat produced by the body, or that there are certain processes going on within the body, not taken into account by physiologists, which involve a considerable production of animal heat. It is to be remembered, also, that I made no allowance for the conversion of a certain proportion of the heat produced in the body into force expended in circulation, locomotion, etc.

Indirect observations have shown that, out of the daily quantity of hydrogen introduced in organic combinations in the food, a large proportion (about eighty-five per cent.) cannot be accounted for by the organic ingredients of the excretions; and it has also been shown that there is generally an excess of water discharged from the body over that introduced with the food and drink.* In another experiment made on my own person, in 1878, I fasted for thirty three hours, beginning my observations on the discharge of water, nine hours after the beginning of the fast, in order to allow the digestion of the last meal to be completed. In twenty-four hours the discharge exceeded the introduction of water by forty-six ounces. I calculated the quantity of water discharged by deducting from the total loss of body weight the loss of solid matters in the urine and the estimated loss of carbon by the lungs. The quantity remaining represented the loss of water in excess of the water introduced. The quantity of water introduced was twenty ounces. No fæces were passed during this observation.

In another observation, the total discharge of water for a period of five days was estimated by the following method:—

The weight of the ingesta for five days was added to the body weight at the beginning of the observation. From this were deducted the weight of the urine and fæces for the five days, the estimated weight of carbon eliminated, and the body weight at the end of the observation. The result represented the total discharge of water by the lungs and skin, which, added to the water of the urine and fæces, gave the total discharge of water. From this was subtracted the water introduced in food and drink. The total excess of water discharged for the five days was 62.78 ounces.† The subject of this experiment weighed 119.2 pounds at the beginning of the observation; at the end of the five days, having walked 317½ miles, he weighed 115.75 pounds.

The observations thus briefly described seem to show that under certain circumstances, at least, water is actually formed in the body by the union of oxygen with hydrogen. In the observation made fasting for twenty-four hours, the quantity thus formed was very large. If it can be assumed that water is formed in this way, the heat value of hydrogen being very great, there is little difficulty in accounting for the heat which has been estimated by direct observation to be produced in the body, as well as for a considerable surplus of heat expended in the form of muscular force. The theory, however, that the oxidation of hydrogen in the body is an important factor in the production of animal heat leads to very interesting and somewhat novel reflections with regard to the physiological relations of water to the general processes of nutrition.

Water Regarded as a Product of Excretion.—There are two substances that result from the physiological wear of the tissues which may be taken as typical products

* Dalton: "Human Physiology," Philadelphia, 1882, p. 37.

† Flint: "Human Physiology," N. Y., 1884, pp. 516, 517.

of excretion. One of these, carbonic acid, is a non-nitrogenized principle, and the other, urea, is nitrogenized. Both of these principles are produced in the tissues and are carried by the blood to eliminating organs. The excretion of both is influenced by the activity of molecular changes in the body. It is with these principles that I shall compare the water which is, in all probability, formed in the body and discharged, under certain circumstances at least, in excess of the water of food and drink.

An excrementitious substance is a principle discharged from the body and produced by physiological wear, chiefly in the form of oxidation, of the organism. It is probable that the organism, under normal conditions as regards alimentation, use of parts, etc., produces a quantity of excrementitious matter which is fixed between certain physiological limits. When alimentation is excessive, a certain proportion of nutritive matter is represented in an increased discharge of excretions. This is marked as regards both carbonic acid and urea, particularly in the increased discharge of urea under an alimentation containing an excess of nitrogenized alimentary matters. Starch, sugar, fats and albuminoids are not discharged from the body in health, but are eliminated in the form of carbonic acid, urea, urates, etc. Muscular work increases immensely the discharge of carbonic acid, in a less degree the discharge of urea, and it notably increases the discharge of water.* Muscular work also increases the production of animal heat, but the temperature of the body is regulated within normal limits by evaporation from the general surface. When an excess of food is taken habitually and for a long time, there generally results an abnormal accumulation of fat, it being impossible for the elimination of carbonic acid to keep pace with the introduction and assimilation of food, unless there be a large expenditure of heat and force in muscular work. The excess of nitrogenized food is disposed of largely in the form of urea; but it is probable that a certain part of this excess is converted into fat, and the muscular substance cannot be increased in bulk except by exercise, and then only within certain restricted limits.

As compared with carbonic acid and urea, the water produced in the body seems, to a great extent, to be subject to the same laws. It is a product of oxidation within the organism; its production may be influenced by alimentation independently of the quantity of water introduced, and by muscular work; it is discharged through organs recognized as organs of elimination, such as the lungs, skin, and kidneys; but its chief point of similarity with the matters generally recognized as excrementitious is in its mode of production.

It has been thought that excrementitious principles, if their elimination be interrupted, are of necessity poisonous when retained in the system. This certainly is not true of water; but the opinion upon this point, with regard to principles commonly regarded as excrementitious, now is open to serious question. It is probable that carbonic acid is not in itself poisonous, and that its retention in the blood produces death by interfering with the absorption of oxygen. There are few conditions under which an animal can be placed in which carbonic acid can be made to accumulate in great quantity in the blood without interfering with the supply of oxygen; but in the well-known experiments of Regnault and Reiset dogs and rabbits were

* Pettenkofer and Voit: *Journal of Anatomy and Physiology*, Cambridge and London, 1868, vol. ii, p. 181. "The elimination of water is very much increased by work, and the increase continues during the ensuing hours of sleep." This was one of the conclusions arrived at from experiments upon a man, twenty-eight years of age, kept for twenty-four hours in a large "respiration apparatus."

exposed for many hours to an atmosphere containing twenty-three per cent. of carbonic acid artificially introduced, with between thirty and forty per cent. of oxygen, without any ill effects.* It is now thought by some pathologists that the so-called uræmic convulsions are not due purely and simply to the poisonous effects of the retention of urea in the blood, although this is still an open question.

If it be assumed that water is produced *de novo* in the economy, in its method of production it closely resembles carbonic acid, but differs from carbonic acid in having certain uses so important as to lead to its frequent introduction with food and drink. Its chief use, however, as regards nutrition, is as a solvent; but in this it is aided by carbonic acid, the presence of which, especially in the urine, increases the solvent properties of the organism. It is evident, also, that water, as a constituent part of the tissues, tends to preserve their proper consistence.

The water of food and drink has important indirect uses connected with nutrition, both as a solvent of nutritive and excrementitious matters and as a constituent part of the tissues; but the water produced within the body by the union of oxygen with hydrogen behaves, in the manner of its production and elimination, like an excrementitious matter.

If this view be accepted, it is evident that the two excrementitious principles with the production of which the generation of animal heat is most closely connected are water and carbonic acid. Under ordinary conditions of alimentation, the production of carbonic acid probably has the greater relative importance; but in starvation, while the excretion of carbonic acid is diminished, the production of water, as shown in my starvation experiment, is probably very largely increased. It is evident that the production of carbonic acid is a much more important factor in the generation of animal heat than is the formation of the urea. In the experiment referred to I calculated the heat value of the urinary nitrogen as equal to 1677.70 heat units, and the heat value of the carbon eliminated as equal to 10,759.09 heat units. The deficiency, as regards the heat actually produced, must have been represented by the excess of water discharged, which amounted to forty-six ounces, the hydrogen of which has a heat value equal to 19,751.75 heat units.† Persons exposed to intense cold, as in the Arctic regions, are known to require enormous quantities of food rich in fatty matters;‡ and the production of carbonic acid is probably very greatly increased, although direct observations on this point are wanting.

The foregoing remarks on the physiology of animal heat have been simply preliminary to a discussion of fever, and the influence of the nervous system upon calorification, which is so important in disease, will be considered only in its pathological relations.

The Mechanism of Fever.—The phenomena and mechanism of fever in all its varieties constitute much too large a subject for full discussion within the proper limits of this address. It has been rendered probable, by recent bacteriological studies, that all of the essential fevers are due primarily to the presence of micro-organisms, those producing typhoid fever, especially, having been isolated, cultivated and accurately described. Fevers symptomatic of local inflammations will

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* *Annales de chimie et de physique*, Paris, 1849, 3me série, tome xxvi.

† The temperature under the tongue at the beginning of the twenty-four hours of the observation was 99° Fah. At the end of the twenty-four hours it was 97¾°.

‡ The late Dr. Hayes stated that on one occasion he saw an Esquimau consume ten pounds of walrus flesh and blubber at a single meal. (*American Journal of the Medical Sciences*, Philadelphia, July, 1859.)

form no part of the question now under consideration; and I do not propose to take up the question of pyrexia due to exposure to external heat, as in insolation. The condition which I shall consider as the type of fever, is the pyrexia in typhoid fever, produced by a definite microörganism and having a duration restricted within certain limits which do not present very wide variations. Typhoid fever is strictly an essential fever, producing, like other fevers, certain parenchymatous degenerations and certain secondary effects upon the nervous system; but it is only the nature, mechanism and rational treatment of the fever itself which I propose to discuss.

The cause of the pyrexia in typhoid fever is twofold. The more important factor is an exaggeration of the chemical changes taking place in the organism, which generate the animal heat within normal limits. A less important factor is a disturbance of the processes of equalization of the heat of the body, mainly by the action of the skin. That an exaggeration of heat-producing processes within the body is an important element in the production of fever, is rendered certain by the excessive consumption of oxygen and discharge of carbonic acid and urea. In health, the discharge of carbonic acid and urea is compensated by the introduction of food, which also has a certain influence upon the quantities of these substances eliminated. In health, there are important influences, also, which depend upon muscular work and activity. In an essential fever, the heat-producing processes seem to be for the time removed from normal regulating influences. Even when no food is taken, the fever continues and the excessive discharge of carbonic acid and urea progresses. The regulating action of the skin is enfeebled or absent. In a fever in which no attempt is made to support the system by alimentation, the phenomena of inanition are added to those of the pyrexia. Simple inanition in a healthy subject is marked by a diminution in the excretion of carbonic acid and urea, with a lowering of the animal temperature, and there is usually but little muscular work. In an essential fever, it seems as if the body were at work, producing an excessive discharge of excretions, without adequate compensation by sufficient alimentation, and without a proper regulation of the heat of the body by cutaneous transpiration. Assuming the failure of measures employed to abort the disease, the excessive waste of tissue pursues its course until the morbid processes are arrested by self-limitation, or until the patient dies, either from secondary effects referable to the persistence of very high temperature, or from simple asthenia. In cases of death from uncomplicated typhoid fever, there usually is very great emaciation.

The waste of the organism usually is most marked as regards the adipose tissue, the destruction of which is probably represented in greatest part by the excessive discharge of carbonic acid. The muscles, also, undergo degeneration, which is at first of the variety called parenchymatous. The destruction of the muscular tissue is probably represented in greatest part by the excessive discharge of urea.

If we can logically view water formed in the body in the light of an excrementitious product, the formation of which in health is closely connected with the process of calorification, the changes noted in most of the cases of typhoid fever, as regards the production and discharge of water, become very important. In nearly all essential fevers there is thirst, but the discharge of water by the skin and kidneys is notably diminished, especially the discharge by the skin, which is dry and hot. In health, the formation of water, with its inevitable generation of heat, seems to carry with it the conditions for equalization of the animal temperature by cutaneous transpiration. In simple inanition, the tissues of the body are economized by the excessive formation of water and the increased prominence of this process in calorifi-

cation. In health, when there is excessive muscular exertion, there is an increased discharge of water, as was noted by Pettenkofer and Voit. In fever, however, the fats and solid tissues undergo destruction and certain degenerations. The formation of water seems to be diminished, and certainly there is a diminished discharge of water from the body attending the increase in the discharge of carbonic acid and urea. Whatever be the essential cause of the pyrexia, the consumption of matter in the production of the excessive heat is chiefly of fat and muscular tissue, presenting a striking contrast to the process of calorification in simple inanition and in violent muscular exertion, which latter condition would actually raise the temperature of the body, were it not for the increased formation of water and the cutaneous transpiration. It would seem that in health, when there occurs any unusual demand for heat to be used in muscular work, the production of water, as well as of carbonic acid, is increased; while in fever the pyrexia is fed, so to speak, by the fatty and solid tissues of the body alone. In health, muscular work confined within normal limits induces nutritive activity and improved assimilation of food. In fevers, the activity is mainly degenerative and the assimilative processes are seriously impaired.

These considerations lead naturally to some modification of accepted views with regard to the theories of fever, and render our ideas on these questions more positive and definite than heretofore. I shall express these ideas in the form of propositions, some of which are, to a certain extent, novel.

1. It is probable that the original cause of most, if not of all, of the essential fevers is a microörganism, different in character in different forms of fever.

This proposition is based upon bacteriological researches of recent date, especially with regard to typhoid fever.

2. Defining fever as an abnormal elevation in the general temperature of the body, the pyrexia is due to the following modifications in the normal heat-producing processes:—

A. Oxidation of certain constituents of the tissues, probably by reason of the presence of microörganisms in the blood, is exaggerated independently of increased muscular work, and without being compensated by a corresponding increase in the appropriation of nutritive material. This increased waste of tissue is represented by the excess of carbonic acid and urea excreted.

B. The part which the formation of water within the body plays in the production of heat is either suppressed or is greatly diminished in prominence, together with the equalizing action of cutaneous transpiration.

This proposition is based upon clinical facts, which show an increased excretion of carbonic acid and urea and a diminished excretion of water in fevers, and upon experiments which show that muscular work, while it increases heat production, increases the production of water.

3. Fever produces abnormal consumption of fat, with parenchymatous degenerations, for the following reasons:—

A. The fat is consumed because it feeds the pyrexia more readily than do the other tissues of the body, and its consumption is the most important source of carbonic acid.

B. Parenchymatous degenerations of muscular tissue and of the solid organs occur, chiefly because the abnormal transformations of these parts, which result in an excess of urea, and which probably, also, contribute to the excess of carbonic acid, are not compensated by the appropriation of nutritive matters from the blood.

C. It is well known that patients with unusual adipose or muscular development

are likely to present a more intense pyrexia in fevers than are those whose adipose and muscular development is smaller.

Finally. *An essential fever is an excessive production of heat in the body, induced by a special morbific agent or agents, and due to excessive oxidation, with destruction of the tissues of the body, and either a suppression or a considerable diminution in the production of water.*

Suppression or great diminution of cutaneous transpiration in the essential fevers, while it contributes, in a measure, to the rise in temperature, is not itself a cause of fever.

I do not propose to discuss at length the influence of the nervous system on the normal production of heat or upon fevers. It is well known that the nervous system is capable of modifying the local circulations and of producing local changes in temperature. Some physiologists have endeavored to locate a heat centre as well as a vasomotor centre, and some varieties of fever are regarded as due to morbid action of nerve centres, either direct or reflex. A consideration of these questions, except in so far as the nervous system is secondarily affected in fevers, would extend this address beyond its proper limits. I shall, however, allude to certain conditions of the nervous system in fevers in connection with what I shall have to say on the subject of treatment.

Rational Treatment of Fever.—Symptoms referable to the nervous system are nearly always more or less prominent in essential fevers of a grave character. In the great majority of cases, at least, the disturbances of the nervous system are secondary and are due to the pyrexia, being intense generally in proportion to the intensity of the fever itself. While the special morbific cause of typhoid fever is, of course, the cause of the delirium, coma vigil, hebetude, etc., observed in grave cases, it is rational to suppose that it acts as a secondary cause of these phenomena, by virtue of changes induced directly by the prolonged elevation of body temperature; and the same may be said of the pulse, which is high usually in proportion to the intensity of the pyrexia. Certain it is, that a mere reduction of the temperature, by means which cannot be presumed to affect the special cause of the disease, is nearly always attended with an amelioration of the nervous symptoms and a reduction in the rate of the pulse.

The parenchymatous degenerations and the alterations in the structure of the muscles and of the secreting cells of glands are unquestionably due to modifications in nutrition produced by the action of microörganisms, and it is well known that in typhoid fever and in pneumonic fever these microörganisms are deposited in special parts, as the intestinal glands and the lungs. It is certainly a rational object of treatment to confine these degenerations within the narrowest possible limits.

While it is not possible to exactly limit different measures of treatment to particular phenomena, there are certain therapeutical indications specially called for by morbid processes which relate to different systems and organs of the body. These measures may be classified as follows:—

1. Reduction of the general temperature by the external application of cold.
2. Reduction of temperature by the internal administration of antipyretics.
3. Promotion of general nutrition by alimentation.
4. Measures to supply to the system matters that can be consumed in the excessive production of heat, thereby retarding destruction of tissue.

The application of cold to the surface by means of cold baths, sponging, etc., is now almost universal in the treatment of the essential fevers. While the value of

this therapeutical measure is undoubted, and while its employment of late years has unquestionably diminished the fatality and abridged the duration of typhoid fever, writers are not agreed upon an exact explanation of its mode of action.

If the proposition that fever is due to the excessive production of heat be accepted as true, the explanation of the beneficial effects of refrigeration of the surface in fevers seems to me to be very simple and entirely satisfactory.

In health, when the body is subjected to excessive cold, the normal temperature is maintained not only by retarding the radiation of heat from the surface by appropriate clothing, but by an actual increase in the production of heat. External cold increases the consumption of oxygen and the production of carbonic acid. The increased production of heat is promoted by muscular exercise, and the material necessarily consumed is supplied by what, under ordinary conditions, would be an excessive assimilation of food, particularly of fatty matters, which have a high heat value. The enormous consumption of fats in excessively cold climates is an evidence of this fact.

In fevers there is an excessive production of heat, which raises the temperature of the body, partly for the reason that the equalizing action of cutaneous transpiration is impaired. If we remove part of this excessive heat by the application of cold to the surface, the temperature of the body is necessarily reduced, and it only remains for clinical observation to determine whether or not this reduction of temperature be beneficial. Its beneficial effects, however, are unquestionable.

Physiological and pathological conditions are thus brought into striking contrast; and the pathological phenomena are readily explained in accordance with physiological principles.

In the healthy body exposed to excessive cold we have given the condition of cold. This is met by a physiological increase in the processes of calorification, and by protection of the surface against loss of heat.

In the organism affected with fever, we have given, as a fixed condition, an increase in the process of calorification. This is to be met by artificial external conditions in which the excess of heat is abstracted from the body.

The clinical thermometer, the general condition, and the sensations of patients afford a sure guide with regard to the extent to which external cold should be applied in any individual case, and the application of cold to the surface is certainly a rational measure of treatment in fever.

The amelioration of the nervous symptoms and the reduction of the pulse rate, which usually follow reduction of temperature by external refrigeration, are arguments in favor of the view that these symptoms are mainly due to the pyrexia itself, and not to the direct action of the special morbific agent which produces the disease.

Analogous effects are produced, although in a different way, by internal antipyretic remedies, of which antipyrin and antifebrin are now extensively used in this country in the treatment of fevers. The mode of action of antipyrin is not well understood, but its efficacy and value in reducing temperature are universally acknowledged. Extended and complete observations on the influence of this drug upon the elimination of excrementitious principles are as yet wanting, but the recent experiments of Umbach, in Berne, show that antipyrin has an important action in diminishing the excretion of nitrogen.*

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* *Archiv für experimentelle Pathologie und Pharmakologie*, Leipzig, 1886, Bd. xxi, Nos. 2 u. 3. According to these observations: "Antipyrin, like quinine and other antipyretics, materially lessens the elimination of nitrogenous matters, and can therefore be said to cause a

Alimentation in Fever.—It is in accordance with accepted views concerning the physiological production of animal heat and its exaggeration in pyrexia, to supply food in fever, in such quantity as can be readily digested and assimilated, with a twofold object. The only possible objection to alimentation within the limits of assimilation would be clinical experience showing an increase in pyrexia or in other morbid symptoms; but clinical experience shows, on the contrary, that innutrition adds to the intensity of all the morbid phenomena characteristic of the disease. There is no disease in which the spectre of inanition is more prominently "a cause of death which marches in front and in silence" than in typhoid fever. An unusual physiological demand for heat is met by increased alimentation. The pathological increase in the production of heat in fever is attended eventually by destruction of tissue and results in degenerations.

The more prominent and important object of alimentation in fever is to supply or retard waste of tissue and degenerations. In so far as this end is attained, the ravages of fever are restrained. The disease having pursued its course, in proportion as its effects upon nutrition are restrained, the system is better prepared for rapid and complete convalescence.

A second object in alimentation, less prominent and important merely because more difficult and uncertain in practice, is to supply material for consumption, and thus far save destruction and degeneration of tissue.

The extent to which alimentation, therefore, is to be carried is limited only by the powers of the digestive system. Unfortunately, however, the degenerations and disturbances of function which occur in fever are prominent in the digestive organs. It is seldom, if ever, possible for a patient to assimilate food in sufficient quantity to repair the waste of tissue; but it is rational to endeavor to secure as much assimilation as is practicable. The difficulties in the way of efficient alimentation are due to degenerations of the glands of the stomach, to which are frequently added degenerations of the secreting cells of the salivary glands and pancreas. The practical skill of the physician is taxed to the utmost, in individual cases, to overcome these difficulties; but the judicious administration of milk, eggs, farinaceous articles, meat broths, meat essences, etc., is always productive of good results. A part of the nutritive constituents of these articles goes to repair the waste of tissue, and it is logical to conclude that a part supplies matter consumed in the production of heat. It is also rational to assume that, the repair of tissue being carried to the greatest possible extent, the hydrocarbons and fats must be useful in supplying material for those processes which are represented by the excessive elimination of carbonic acid. In no case of fever is it possible actually to accumulate fat in the system during the progress of the disease; and hydrocarbons and fats introduced must contribute to the formation of heat and thus restrict parenchymatous degenerations.

The disturbances which follow an alimentation carried to a degree beyond the powers of the digestive organs afford a reliable guide as regards the extent to which food should be introduced in fevers; and, in my judgment, too little attention and care have been given to the administration of varieties of food which have a high heat value, such as fatty and farinaceous articles.

As a digression, bearing, however, upon the question of value of fats in conditions involving excessive productions of heat, I may allude to their use in pulmonary phthisis.

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decrease in the tissue changes of the respiratory and alimentary systems." *Therapeutic Gazette*, Detroit, September 15th, 1866.

I assume it to be settled that phthisis is produced by a special microörganism. One of the most prominent general phenomena of phthisis is a constant elevation in the body temperature. When the disease is progressive there is an increase in the heat of the body. When the disease is non-progressive, as indicated by physical signs, arrest of loss, and perhaps increase of weight and absence of bacilli from the sputum, the temperature of the body becomes nearly or quite normal. The pyrexia in phthisis is ordinarily not sufficiently intense to induce, of itself, serious disorders of digestion or much general disturbance; and the difficulties in the way of the assimilation of fats are usually not great. Leaving out of consideration, for the present, the effects of alcohol, there is no measure in the treatment of phthisis of greater recognized therapeutical value than the administration of fats.

The Use of Alcohol in Fever.—Alcohol is a substance the toxic effects of which, taken in excess, are quickly and distinctly manifested; and there are few agents more prompt and decided in their influence on cases of disease. Clinically, the effects of alcohol in diseases in which there is a tendency to death by asthenia are so marked, that it is often used indiscriminately and injudiciously. In the treatment of fever the immense benefit which follows the use of alcohol in certain cases has led, at one time, to its use under all circumstances; and its indiscriminate administration, on the other hand, has produced, from time to time, a reaction of opinion, leading to its suppression in cases in which it would be of great service. Many of the moral arguments against the use of alcohol in disease are entirely illogical, and could, with equal want of propriety, be applied to a number of important articles of the materia medica. Alcohol is a potent agent in the treatment of fever; and the clinical guides which should direct its administration are easily recognizable.

In no case of disease, except, perhaps, in certain instances of poisoning by animal venoms, should alcohol be administered to a point where the slightest degree of alcoholic intoxication is apparent.

Alcoholic intoxication is due to certain peculiar effects of alcohol upon nerve centres; and in order to produce these effects, the alcohol must circulate in the blood. As these effects pass off, the alcohol is either oxidized or is eliminated by the skin, lungs and kidneys. Under normal conditions of nutrition, the effects of alcohol are so rapid and transitory, and are followed by such decided reaction, that it contributes little or nothing toward a prolonged resistance to cold. Experience has shown that it cannot take the place of an abundant and a highly fatty alimentation in excessive cold, as in the Arctic regions; and under these conditions its constant use has been found to be positively injurious. The same remark is in a measure applicable to all conditions of healthy nutrition. In a continued fever, however, the conditions are radically changed. In accordance with the views which I have presented, the excessive production of heat in fever is a fixed condition, continuing for a certain period, which is limited by the duration of the disease. The phenomena referable to the pulse, to the nervous system, etc., are secondary to the pyrexia. The parenchymatous degenerations are the more remote changes of tissue which follow and result from transformations involved in the long-continued excessive production of heat. If these views be accepted as correct, any readily oxidizable substance artificially introduced, will, if it be oxidized, mitigate the secondary effects of the fever upon the pulse and nervous system and retard degenerations, provided, always, that it does not increase the intensity of the pyrexia. Experience is not wanting to show that these results follow the judicious administration of alcohol in fever.

Inanition is also a constant element in a fever long continued. In health, the formation of water in considerable quantity, in the production of heat, occurs in the

first part of a period of deprivation of food, and this saves, to a certain extent, destruction of the solid tissues. One of the most marked and constant conditions in fever is a disturbance of the heat-producing processes, in which the solid tissues are consumed and the production of water is greatly diminished. It is a rational object of treatment to endeavor to restore the normal equilibrium between the consumption of the so-called solids and the formation of water, as factors in the production of heat. If it were possible to introduce farinaceous and fatty articles of food in sufficient quantity in fever, it might be necessary or desirable to use alcohol; but the condition of the digestive organs is such that these articles are slowly and imperfectly prepared for absorption. Alcohol, however, requires no preparation by digestion. It is promptly taken up by the blood, and is oxidized even more readily in fever than in health.

It is well known that saccharine and starchy articles of food, as well as the liver sugar, rapidly disappear, and that starch is converted into sugar in digestion. In a remarkable paper by Dr. William Hutson Ford, a number of interesting experiments are published, showing the presence of alcohol in small quantity in the normal blood, resulting, according to Dr. Ford, from the decomposition of sugar. In this paper, Dr. Ford makes the following statement:—

"The destination of alcohol, whose presence in the economy I have thus demonstrated, must be to a hæmal oxidation or 'combustion,' as a main source of animal heat. This combustion is maintained, not only by glucose derived from amylaceous food, but likewise from the proximate products of change in the nitrogenous tissues."*

The thermal phenomena observed in diabetes mellitus are of much interest in connection with the theory that hydrocarbons are converted into alcohol, the oxidation of which is an important factor in the production of animal heat. In nearly all cases of diabetes there is a constant and persistent depression of the animal temperature. The principal pathological condition in this disease is manifested by a discharge from the body, in the form of sugar, of the hydrocarbonaceous elements of food; and, as a consequence, these substances are not used in the production of heat. The result is a constant depression in temperature, with a loss of weight which is often very rapid,† due to a consumption of the fatty and nitrogenized parts of the tissues, and certain parenchymatous degenerations which are observed particularly in the cells of the kidneys. It may seem paradoxical to say that diabetes is attended with a fever in which the temperature of the body is depressed. In progressive phthisis there is actually a fever, marked by elevation of temperature. In diabetes the fatty and nitrogenized parts are consumed to an abnormal extent, as in fever; but the heat-producing action of the hydrocarbons being suppressed, even this excessive consumption of fats and albuminoids is inadequate to maintain the normal standard of the bodily temperature. The theory that the hydrocarbons are converted into alcohol which is oxidized in the body is entirely in accordance with my view that

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* Ford: "The Normal Presence of Alcohol in the Blood." *New York Medical Journal*, June, 1872, p. 594.

† The history of a diabetic patient who consulted me in November, 1865, showed a loss of weight amounting in three years to 145 pounds. The patient was six feet three inches tall, and weighed, before the disease was recognized, 375 pounds. Under treatment for five days, the quantity of urine was reduced from 100 to 40 fluidounces, and the proportion of sugar from 27 to 2 grains per ounce. The patient then passed from under my direct observation, and the discharge of sugar was increased by indiscretions in diet. Ten months after, the patient wrote to me that he "felt perfectly well," and had gained forty pounds in weight.

the production of water is an important factor in the generation of animal heat. If alcohol be oxidized in the body, as it is in certain quantity, the production of water is inevitable. The heat value of the hydrogen in alcohol being very great, and if—the hydrocarbons being discharged in the form of sugar in diabetes—the normal heat of the body be not maintained, the final oxidation of these hydrocarbons, with the consequent production of water, is an important factor in the production of animal heat. The excessive quantity of water discharged in diabetes probably comes in great part directly from the blood and the watery parts of the tissues, which accounts for the intense thirst always observed in grave cases of this disease.

In the administration of alcohol in the treatment of fever, we are really using the hydrocarbons in a form in which they may be immediately oxidized and do not require preparation by digestion. Thus we easily supply material to meet the excessive waste involved in the pyrexia, in much the same way as we administer peptonized albuminoids to meet the excessive waste of the nitrogenized parts of the tissues when the digestive powers are impaired.

It is a matter of universal clinical observation that there is great tolerance of alcohol in fevers and in pulmonary phthisis. This tolerance of an agent which is probably never useful in perfect health is strong evidence of a demand on the part of the system for the class of alimentary principles, the hydrocarbons, which alcohol represents; and it affords an absolute guide as regards the quantity that should be employed. The quantity which will be useful in individual cases may be small or it may be great. In certain exceptional cases, one or two ounces of spirit may be administered hourly for a day or two, with the best results; and this quantity may be taken without the slightest manifestation of alcoholic intoxication. With an alarmingly high temperature, a rapid and feeble pulse, and grave ataxic symptoms indicating impending death, alcohol may be given largely, but never to the extent of producing its characteristic toxic effects. In fever only such quantity of alcohol as is readily oxidized is useful; and any excess, which will certainly produce some degree of alcoholic intoxication and which must be eliminated as alcohol, will be productive of harm. In ordinary cases of continued fever it is seldom necessary or desirable to give more than eight or ten ounces of spirit daily.

I do not wish to be understood as advocating an indiscriminate use of alcohol in all cases of fever. Alcohol is indicated by an excessively high temperature, with the ataxic and other symptoms to which I have referred. In ordinary cases of typhoid fever, particularly in the early stages, it should be administered sparingly, cautiously and tentatively. Its quantity should be reduced, or it should be omitted at the first indication of alcoholic intoxication. Nevertheless, alcohol, judiciously administered, so that all that is introduced is promptly and completely oxidized, as it contributes material for consumption in the production of excessive heat, exactly in that degree does it retard destruction and degeneration of tissue, and it should be employed to supplement the use of matters that are regarded as nutritive.

In a paper published in 1879, I made a calculation of the heat value of the ordinary brandy of the Pharmacopœia, which I venture to quote:—

"According to Brande,* Cognac brandy contains 46 per cent. of absolute alcohol. With a specific gravity of 0.930, one ounce of brandy weighs 406.875 grains and contains 187.1625 grains of alcohol. The alcohol, with a composition of $C_4H_6O_2$, contains 12.9 per cent. of hydrogen, or 24.14 grains, and 56.65 per cent. of carbon, or 98.54 grains. The heat value of 24.14 grains of hydrogen equals 214.77 heat units.

* Brande and Taylor: "Chemistry," Philadelphia, 1867, p. 583.

The heat value of 98.54 grains of carbon equals 182.44 heat units.* Taking, then, the total heat value of the hydrogen and carbon contained in one ounce of brandy, and taking no account of the oxygen contained, the heat value amounts to 397.21 heat units.† If we assume that a man produces four heat units per pound weight of the body per hour, the amount of heat normally produced in twenty-four hours by a man weighing 140 pounds is equal to 13,440 heat units. The quantity of brandy required to supply this amount of heat, according to the calculations I have just made, would be a little less than 34 ounces. Theoretically, then, it is easy to see how alcohol may furnish material to supply heat and save waste of tissue in fevers. It is not very unusual, in certain stages of fever, to administer from 16 to 32 ounces of brandy in twenty-four hours."‡

I am deeply sensible of the great honor of an invitation to address this Congress. This invitation I felt bound to accept, although with a full appreciation of the responsibility which it involved, and with much doubt and timidity with regard to my ability to do even a small measure of justice to the occasion. I have selected a topic of great present interest, which has been the subject of much fruitful study within the last few years. In discussing this subject, I have endeavored to apply the physiological methods of study which have lately contributed so largely to the advancement of pathology and therapeutics. I have been led by my reflections upon animal heat and fevers to present certain views which I venture, in conclusion, to summarize in the following propositions:—

1. Fevers, especially those belonging to the class of acute diseases, are self-limited in their duration, and are due each one to a special cause, a microörganism, the operation of which ceases after the lapse of a certain time.

2. We are as yet unable to destroy directly the morbific organisms which give rise to continued fevers; and we must be content, for the present, to moderate their action and sustain the powers of resistance of patients.

3. The production of animal heat involves oxidation of parts of the organism or of articles of food, represented in the formation and discharge of nitrogenized excrementitious matters, carbonic acid and water.

4. As regards its relations to general nutrition and the production of animal heat, water formed in the body by a process of oxidation is to be counted as an excrementitious principle.

5. Fever, as observed in the so-called essential fevers, may be defined as a condition of excessive production of heat, involving defective nutrition or inanition, an excessive production and discharge of nitrogenized excrementitious matters and carbonic acid, with waste and degeneration of the tissues, and partial or complete suppression of the production and discharge of water.

6. Aside from the influence of complications and accidents, the ataxic symptoms in fevers, the intensity and persistence of which endanger life, are secondary to the fever and are usually proportionate to the elevation of temperature. These symp-

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* Mayer: "Celestial Dynamics, Correlation and Conservation of Forces," N. Y., 1868, p. 261.

† It is well known that the heat value of certain elements in combination is less than of the same elements in a free state, as was shown by the experiments of Favre and Silbermann, made many years ago; but with regard to alcohol, the difference is only about one-third of one per cent. Favre et Silbermann: *Annales de chimie et de physique*, 3me série, Paris, 1842, tome xxxiv, p. 357, and Milne Edwards: "Leçons de physiologie," Paris, 1863, tome viii, p. 25.

‡ Flint: "Experiments and Reflections upon Animal Heat." *American Journal of the Medical Sciences*, Philadelphia, April, 1879, p. 362.

toms are ameliorated by measures of treatment directed to a reduction of the general temperature of the body.

7. The abstraction of heat by external cold and the reduction of temperature by antipyretics administered internally, without affecting the special cause of the fever, improve the symptoms which are secondary to the pyrexia.

8. In health, during a period of inanition, the consumption of the tissues in the production of animal heat is in a measure saved by an increased production and excretion of water.

9. In fever, the effects of inanition, manifested by destruction and degeneration of tissues, are intensified by a deficient formation and excretion of water.

10. Alimentation in fever, the object of which is to retard and repair the destruction and degeneration of tissues and organs, is difficult mainly on account of derangements of the digestive organs; and this difficulty is to be met by the administration of articles of food easily digested or of articles in which the processes of digestion have been begun or are partly accomplished.

11. In the introduction of the hydrocarbons, which are important factors in the production of animal heat, alcohol presents a form of hydrocarbon which is promptly oxidized, and in which absorption can take place without preparation by digestion.

12. Precisely in so far as it is oxidized in the body, alcohol furnishes matter which is consumed in the excessive production of heat in fever, and saves destruction and degeneration of tissue.

13. The introduction of matters consumed in the production of heat in fever diminishes rather than increases the intensity of the pyrexia.

14. As the oxidation of alcohol necessarily involves the formation of water and limits the destruction of tissue, its action in fever tends to restore the normal processes of heat production, in which the formation of water plays an important part.

15. The great objects in the treatment of fever itself are to limit and reduce the pyrexia by direct and indirect means; to limit and repair destruction and degeneration of tissues and organs by alimentation; to provide matters for consumption in the abnormal production of heat, and thus to place the system in the most favorable condition for recuperation after the disease shall have run its course.

The reading of the address was listened to with marked attention, and at its close there was great applause.

The general session then adjourned.

In the evening the President of the United States, the Honorable GROVER CLEVELAND, and MRS. CLEVELAND, received the members of the Congress and the ladies accompanying them, after which the members visited the Corcoran Art Gallery, which was opened for that purpose by the courtesy of the Trustees.

THIRD DAY.

The Congress again met in general session on Wednesday, September 7th. The President announced as the first business the reading of an address by Professor MARIANO SEMMOLA, of Naples, Italy, and Vice-president FRANCESCO DURANTE, of Rome, was called to the chair.

Professor SEMMOLA then delivered in French the following address. As the manuscript is in Italian, an English translation is used :—*

SCIENTIFIC MEDICINE AND BACTERIOLOGY IN THEIR RELATIONS TO THE EXPERIMENTAL METHOD.

BY MARIANO SEMMOLA, M.D.,

Director of the Therapeutical Clinic of the University of Naples; Delegate of the Italian Government to the Congress.

"Nec ab antiquis sum nec a novis; utrosque ubi veritatem colunt sequor."—BAGLIVI.

"En science, de même qu'en politique, il faut se défendre également et des préjugés conservateurs et des préjugés novateurs. Nos contemporains prennent souvent la nouveauté d'une idée pour un gage de sa vérité. La règle de nos pensées ne doit être ni le vieux ni le neuf, mais le vrai."—BERNARD.

GENTLEMEN: Invited by the Executive Committee to the high honor of giving before you an address on the subject of general medicine, I feel it, on the one hand, my duty to express to you my thanks for this high distinction, far above my modest merits, and, on the other hand, I feel the need of appealing to your indulgence, which I hope will come to my aid in this difficult arena that I have entered, animated by the desire to bring you the greetings which the Italian Government has confided to me to convey as a tribute to this medical festival, with faith in me that I may worthily respond to the call.

In the name of Italian medicine I greet you, most illustrious colleagues of the United States, and representatives of medical progress in the New World. I feel it my duty to thank you before all else for the exquisite courtesy with which I have been received in this land, which includes all the greatness of all future free nations. A humble son of Italy, but a passionate admirer of the grand titles of nobility that this alma mater has acquired by right in the history of thought and wisdom, it has ever been the dream of my life to see her honored as she deserves; therefore, invited by your benevolence, I have thought that the best way for me, of all the sons who so revere their country (as you do yours), to merit it, and to fulfill toward my Italy the duty of a son, was to talk to you of the value of one of her most glorious discoveries in relation to the progress of modern medicine: I mean the experimental method which was born in Italy with Galileo, and which has always

* From the *New York Medical Journal.*

been the only guiding compass of scientific progress, without which the most daring and expert pilot would most surely be shipwrecked.

On the flag of medicine—as old, or about as old, as human suffering—there has always been written this device: "Preserve health and cure all ills." This flag was, and will always be, that which must guide us to the effective end of every study, and which must be for the physician, even the one most passionately fond of his science, a true amulet against those temptations that medical studies can inspire in him in the way of mere scientific curiosity. It is self-evident that if there had never been any sick there would never have been any doctors, and it is for this reason that the Platonic philosophy in the progress of medicine seems ridiculous to society, and the old saying, "*Medice cura te ipsum,*" very well expresses the irony and sarcasm which the world flings at the physician who is impotent to overcome his own ills. I can easily conceive of a physicist or a chemist who is not a mechanic or a merchant, or of a botanist who is not an agriculturist, and so on, but I cannot conceive that there can be a physician who does not occupy himself with observing and taking care of the sick, because it is his study, and it is unquestionably at the same time the only means and only intent of his real mission.

Thus, for me, as in the case of great physicians, the measure of real progress in medicine must not, and should not, be estimated in any other way than the greater or lesser number of patients which the physician can conscientiously say he has seized out of the jaws of death. This estimate may appear to some to be too prosaic, but, unfortunately, it is the stern reality; and it is this reality that renders the evolution of medicine one of the most important social functions connected with civilization, for, wherever man is associated with man in a mutual cause, medicine corresponds to a collective and social interest. "*Mens sana in corpore sano,*" says the poet, is the apogee of human happiness.

No matter in what position in life a man may be, his greatest object should be to preserve health and life, because illness and death represent disorder, affliction and the desolation of his domestic home. The solidarity that unites the members of a family among themselves extends to society in general, and sometimes the death of a single man may be such a public calamity as to compromise and change the destiny of a nation, and this is the very reason why there is nowhere in the whole range of human investigation a more ardent desire to solve the problems encountered than in medicine. Still, it is very natural that the hope of arriving at the whole truth, constantly misled and constantly being born again, has upheld through so many hundreds of years, upholds, and will uphold forever, all the generations in their passionate ardor of study to discover the mysteries of the phenomena of life in health and in disease.

Beginning with the great Greek epoch, that of Phidias and of Plato, of which the immortal Hippocratic pages constituted the first scientific expressions, up to Koch and Pasteur—that is to say, beginning with the time when man, allowing himself to be guided solely by sentiment, thought that he was in possession of absolute knowledge, up to the modern epoch, in which reason and experience would completely suffocate these aspirations—medicine, except during the long silence of the middle ages, has developed by observation alone, and made precious acquisitions, although often agitated in the most opposite directions, but still living every day upon the discoveries of the day before, and never disowning its past, in which even its bitterest adversaries have ever found an indispensable support and an inseparable counsellor for the new directions of the work.

Thus more than two thousand years have gone by, during which period, first by

the force of instinct purified even to sentiment, and afterward with the abuse of reason prostituted more or less by the scholastic, the intimate study remained a dead letter for the progress of medicine, except here and there, where all those rare geniuses who have built up the true medical tradition have in all times found themselves forced to conduct medicine back again to the knowledge of human nature acquired by observation and experience. But these true naturalistic geniuses were wanting in a solid base, and could find their indispensable foundation only at the beginning of the seventeenth century, which epoch made such admirable conquests in the dominion of science. And it was then only that the incessant progress of the science of physics and chemistry allowed, if it did not really compel, biology to take its place among the experimental sciences, thus promising to save medicine from subsequent shipwreck. So, after having traversed so many centuries of superstition and errors, medicine at last saw rise in the heavens the true cynosure which was to direct the steps of its most daring ministers. This star, gentlemen, was experiment—that is to say, the study of the objective reality of natural phenomena—and lastly the evolution of the human mind, which had to teach man that the truth of the external world was not to be found formulated either by sentiment or by reason; or, in other words, according to the happy expression of Berthelot, that the world was not made by guessing at it, but by observing. And, in fact, man with this guide has done wonders in all branches of human knowledge, and arrived at an infinite superiority over his most perfect forefathers of the best Greek and Alexandrian epochs; and the story of experimental science is a sublime and infinite poem, of which the substance is always humanity at war with nature to subjugate and conquer. It is easy to imagine with what an impulse physicians with this prospect in view will go on, with the noble hope of conquering the most terrible enemies of mankind—that is, his ills. But the revelation of truth, which is the natural fruit of science, has need, like fruits of the cultivation of the soil, of the sowing of the seed, of the development of the plant, of the flowers to assure the harvest; but woe to the farmer who sows in sand even good seed, trusting that its goodness is enough to insure him a harvest—he would destroy it forever. And the physician who should imitate him, even under cover of the experimental method, would not be less to be pitied than his predecessors, who, armed with the finest logic, always arrived at error. I understand that the incessant curiosity of the scientist, and, above all, of the physician, is impatient, and that curiosity, as the saying has been during many centuries, is the mother of science, for in truth nature only reveals her mysteries to the inquisitive. I understand that the love of novelty is a most natural thing in turbulent times, and that great commotions in any order of ideas awaken the impatient activity of revolutionary spirits. I also understand that when evolution languishes it is by revolution that progress takes place, and that at such times it is even allowable to tolerate the excesses of these profound commotions, even if they take place in social or scientific order. But the moment will always come in which the inquisitive spirit will stop to remember that nature does not proceed by jumps, and that, therefore, it is the duty of the true scientist, although recognizing that revolutions are an historical necessity of humanity, to keep them within the limits which the law of evolution defines.

This harmony between evolution and revolution in the march of a science is commensurate with the benefits that its development brings to society; and this may be said of the physical as well as of the moral, because the effective balance of the ills and advantages of life represents unquestionably the only true measure of the progress of humanity. When this balance shows gaps and delusions, and, above all,

when the road that has been taken to gain it is undoubtedly proclaimed the only and indisputable one, we must come to the conclusion that there are some broken teeth in the wheel, and that therefore the interlocking is deranged. The result then is far worse than if the wrong road had been taken in the beginning. I beg you, gentlemen, to follow me.

Medicine presumes to-day more than ever to regenerate itself, and it is its right, if not its duty, as was the case with the other sciences that preceded it in this noble aspiration. Medicine has arrived at that period of evolution in which to-day it invades every other science. It feels the need to penetrate into the inmost of nature's phenomena, which for it are only the facts of a healthy or diseased life. Medicine would, with the experimental method as its guide, aspire to achieve a mathematical precision and impose itself more and more on all the other sciences, and thus it is to be hoped that at last it will have its definite code, which should be the breviary of its ministers for curing diseases. What a new and glorious age of light will this be for humanity! Perhaps we may then be able to say that the final aim of our studies is not only the "*ars medendi*," but the "*ars semper sanandi*."

This point does not call for the least discussion. Therapeutics has always been the aim of the greatest physicians. Even clinical medicine, without therapeutics, would be the meditation of death. If, dazzled by the wonderful progress made by science, it had been forced to forget this fundamental truth, it would feel the ground falling under its feet, and would walk in the darkness of emptiness.

This is what history teaches. At present, when we think that in all this chaos of errors tradition has transmitted to us curative treasures that even skeptics are obliged to respect, and which still form the greatest, the clearest, and, perhaps, the only demonstrations of therapeutics—mercury, quinine, iodine, etc. On the other hand, all the splendid scientific successes have not been able as yet to furnish anything that can truly rival these poor little foundlings of empiricism. In a very serious disease a doubt must always arise in the mind of a really learned, honest physician, but, rather than yield to the illusion of new remedies that are proclaimed to-day on the altars only to be buried in the dust to-morrow, he would shut himself up and meditate on the phantasmagory of the present day, which certainly cannot constitute the real balance of scientific progress.

Not otherwise, gentlemen, would the most daring and confident traveler do, who, tired and footsore from his journey, unable to near his haven, and often discovering new horizons, which make him see it further away, feels the need of rest for a little, so as to regain his strength, and make sure that he has not lost the right road. Please be so kind as to follow me, gentlemen.

The experimental method has for its aim to search for the determining or proximate causes of the phenomena of nature. The principle on which this rests is the certainty that determining causes exist; its way of proceeding in the search is the philosophic doubt; its criterion and judge is experience. In other words, the scientist firmly believes in the existence of the determining causes that he is searching for, but always doubts having discovered them until experience peremptorily demonstrates that he is in the right.

The experimental method is, in fact, nothing else than the expression of the natural march of the human mind in the investigation of scientific truth, of reason and experience surrounded with the aid of well-balanced judgment. It does not admit dogmatic personal authority, and in the most absolute way it thrusts from it hypothetical systems and doctrines; nor does it do this out of pride or for bravado; on the contrary, the true scientist is always humble, denying individual authority,

doubtful even of himself, and submitting his opinions to the authority of experience and the laws of nature. Goethe said that the only mediator that could exist between the scientist and the phenomena by which he was surrounded must be experience. The rigorous observation of truth is the first study; next comes the coördination of the truth, which is an entirely different thing from the fact itself, and is the formulated law which is the exponent of the fact. Lastly, the need is felt of searching for the *causes* of the fact, and here begins the most arduous work of the experimental method. The search for these causes means the ascertaining of the conditions under the influence of which the fact or phenomenon manifests itself, and it is only when that has been done that the scientist can formulate the laws that regulate the appearance or disappearance of the phenomenon. Without such knowledge, simple causal notions cannot logically be fecundated; and why? because the causes of phenomena are not laws, and the laws are not causes, notwithstanding what some absent-minded philosophers have said.

It is evident that this last research is the most complicated and difficult, because after the simple observation of the fact, and in the order in which it is unfolded, there is always a need of an hypothesis that prepares for the discovery of the truth; this hypothesis, as was said by Newton, is a kind of daybreak that just begins to disclose the truth in a vague way, which must therefore be illuminated little by little by experience until it shines with brilliancy. Then only can the hypothesis be upheld as true, and the honest and eager searcher must be ready to forget his hypothesis forever, if it is not confirmed by the interrogations which he has made of nature. The honest scientist must always forget himself, never be satisfied by his intuitions, even if they are those of a genius, and proceed in all ways to verify hypothesis by experience before proclaiming it as a truth. The experimental method, therefore, is composed of three elements—observation, supposition, and verification. These three elements are distinct, but inseparable. The hypothesis occurs in the observation and in the verification. The observation occurs also in the hypothesis, of which it is the starting-point; and in the verification, of which it is the final substance. Finally, the verification is inseparable from the observation, which is its instrument, and from the hypothesis which it is to destroy or confirm. The experimenter who, after having made a new and splendid observation, and conceived a daring hypothesis to explain it, instead of wholly giving himself up to completing the experiment so as to establish an everlasting truth, prefers to exalt himself for his hypothesis without trying to verify it, and communicate it to all mankind, affirming it to be the definite truth, torturing nature to justify his enthusiasm, is in reality a traitor to science, and loves himself better than he does the discovery of truth, because, trusting to the credulity and servility of the ignorant masses, he sacrifices *real* progress to the vain paternity of a new system. In fact, he allows himself to be carried away by a weak though dominating spirit; perhaps, though a learned man, he does not deserve the name of a scientist, because he does not possess the sentiment which Pascal expressed paradoxically when he said: "*We never search for* things, but we investigate things." The experimenter who loves true progress does not suppress anything in his researches, nor is he disturbed by results contrary to his expectations; the immortal Bernard used to repeat, "Redouble your ardor to arrive at the temple of truth," because he well knew that to shut one's eyes to unfavorable results was not equivalent to suppressing them. He who does this simply deludes himself, like the ostrich, which, when it has hidden its head in the sand, thinks it has escaped the danger.

This, therefore, is the road upon which medicine must travel if it wishes to regen-

erate itself. We can imagine a science in its infancy, but we cannot admit the construction of a science that denies at every step the only code of its origin. The problem of scientific medicine appears most simple, to determine the conditions of the existence of the vital phenomena in their healthy or morbid state. The first step was in physiology, and I certainly need not remind you of how much the experimental method in physiology has taught physicians, or that it is exactly the fact of their having applied it so well in the discovery of many secret mechanical functions that has made it light up clinical medicine like a bright sun. To arrive at this degree of certainty it needed more than half a century of study, without taking into consideration the precious foundations which biology had prepared since the beginning of the last century; and the "*errata corrigenda*" which had to be made at so many points of the science, after long periods in which it was thought that certain definite truths were already on the throne; without counting, finally, that, while the patient and honest physiologist was hunting with an undaunted ardor that the search of truth alone can cause, the object of his quest never escaped him, and the failure of to-day was softened by the certainty that on each to-morrow he would be able again to interrogate nature, because humanity continued to live quietly on without occupying itself as to how the stomach digested, or what were the functions of the liver, etc. And it is thus that unconsciously it presented to the scientist each day, each hour, each minute the same problems, without ever dreaming that around it there were so many honest spies that were each day presuming to lift another corner of the mysteries of its enjoyment, because good health is enjoyment. Still, after so long a time, and with such favorable conditions of study, who would dare to assert to-day that what we know in human physiology is more or less than that which remains for us to learn? It is enough to remember that of the biological chemistry of the blood we know little or nothing, and that hæmatological notions which were the most accredited, are threatened by a revolution since the late researches of an illustrious scientist, Angelo Mosso, an honor to Italy, but who will most likely be obliged to stop and begin over again.

Logically, the physician who proclaimed the experimental method should have been disheartened by the colossal difficulties of this prologue, and of those infinitely greater ones that were easy to foresee when entering upon a field so much more complicated—the field of pathology and therapeutics. But it was not only the difficulty of conquering; it had also to be considered that the imperfect conclusions at which one might arrive were not a matter of indifference to humanity, and that the correction of the error of to-day would always be too late to remedy the unfortunate consequences that had already taken place, because the sick man does not wait; moreover, he cannot and *must* not wait.

These difficulties and these reflections, which did not escape some immortal physiologists, among others Bernard, escaped many physicians, and Bernard, after thirty years of hard research, came to the conclusion that scientific medicine was not yet ready to be constituted. The sign of the shop was enough for these physicians, who had no clear idea of the vastness of the problems that they would have to solve in the experimental method. This innocence constituted in itself an unfavorable sign for the result, because in nature all is harmony. The force must be in proportion to the resistance that is to be overcome, and it is only in this way that we must prepare to observe and study nature. Otherwise we must supply the deficiency of force from fancy, and then we return to the perfectly scholastic without knowing it. This mode of procedure is certainly damaging to the effective progress of pathology and of clinical medicine, but becomes downright ruin in therapeutics. A bold hypothe-

sis more or less true or more or less false, which is limited simply to satisfying the physician's curiosity to explain obscure morbid facts plausibly, is an innocent illusion which may change every day according to the taste, without any one having the right to complain; but when this hypothesis is not yet judged true—for, according to the laws of the experimental method, it must conduct straight to the cure of the patient—then it becomes another matter. The physician then commits a crime and cannot be excused, not even by pleading the feverish desire to cure his patient, because it is not allowable for an honest man to conceive even praiseworthy ideas when conscience makes him feel that he cannot do so without harming another man, and the fact of this impossibility is sure to be felt with the knowledge that this or that rational remedy was not the result of the true experimental method. If we were to proceed in this way in physics and chemistry, all the manufacturers who have opened factories founded on the rigid laws obtained from these sciences would lose their capital, and would necessarily become bankrupts. The poor sick must bear everything. I do not really know why such a cry is raised against the empirical medicine of former times, while the application of the new remedies based on a bad experimental method is nothing more nor less than another kind of empiricism, not less to be deplored than the empiricism of the past. In traditional empiricism medicine cured or killed the patient without knowing *why*—that is to say, despising the exact idea of the remedy—and scientific medicine does the same when, instead of religiously discovering the truth, it abandons itself to be judged by *how;* they both belong to blind medicine most assuredly—one is blind with the appearance of ignorance, and the other is blind under the mask of science. Unfortunately, this is the naked reality of the facts.

But very different are the exigencies of the experimental method when it wants to proceed to the laying of the corner stone of true scientific therapeutics. What has physiology done and what is it doing? Physics and chemistry study the conditions of the existence of phenomena to formulate laws so as to direct the appearance or disappearance of said phenomena. Pathology and therapeutics must do the same, as a healthy organism is not a passive field of action in which disease and the remedy which is to be applied as a cure come to do battle. The method of search must, therefore, be alike, in the same way that there are not two workmen, one to build a house and one for the house which is falling.

In the mechanical phenomena of the organism nothing can be seen to distinguish them from the phenomena of general mechanics. Likewise in the infinite series of physico-chemical phenomena there is nothing that distinguishes the physico-chemical phenomena of a living being from those of dead matter, except the law of matter, different from that which regulates the manifestations of the first.

Thus there are not two different sorts of mechanics, nor of physics, nor of chemistry, but there are surely in living beings certain special characteristic conditions of matter, which are regulated without doubt by the laws of physical chemistry, but different from the *ordinary* laws which physics and chemistry have formulated in relation to conditions of matter entirely opposite. Under this relation life is only a modality of the general phenomena of nature—that is to say, while there is a specialty of substance and form, which characterizes the manifestations, and which in its origin is inaccessible to our researches, because life signifies *creation*, in reality it has a communion of laws which confounds it with all the other cosmical phenomena.

Scientific and experimental medicine proposes to discover the conditions of the phenomena proper to life, or perhaps to decide their determining causes, and these are, or rather should be, its pillars of Hercules. It, like all the rest of the sciences,

does not delude itself by hunting for the Why, well knowing that the first legislative and directive causes of creation are inaccessible; and that it is enough for each scientist to know under what physico-chemical conditions such or such a phenomenon manifests itself, that he may be able to control and modify its occurrence.

What are the phenomena that the physician studies? The functional disturbances, or the symptoms of the disease. What are the physico-chemical conditions of these phenomena? The internal causes of the disease. This is the simple formula of the pathological problem. To arrive logically at the third part of the solution, which is the most serious, how is it possible to artificially modify these diseased physico-chemical conditions so as to restore them to their normal physico-chemical condition—that is to say, to make the morbid phenomena disappear and the normal functions return?

It is enough, gentlemen, to announce this series of problems to frighten and amaze, not *one*, but many and many generations of explorers. This honest confession would appear to me to be the best preface of future scientific medicine. There is no remedy. If scientific medicine is to be constituted, this is the logical progression of its steps. Outside of this orbit all is empiricism and ignorance, because there do not exist half-sciences or conjectural sciences. Following this road, the physician (like the physico-chemist, who first properly knew the conditions of the existence of natural phenomena, and then modified them so as to turn them to his profit)—the physician, I say, will learn how stupid is the phrase which to-day is so often repeated, and in the use of which he often abuses the name of a misunderstood progress—that *man commands nature*. No, gentlemen, in reality the scientist, as also the physician, if he wishes to be and not appear such, instead of commanding, obeys, and *must* obey nature, a truth as old as Hippocrates: because, if he wishes to profit by them, he must completely study the laws that preside over phenomena, and, while interrogating nature, must jealously respect these laws. Otherwise, nature without exception will rebel, and the result which he desires will certainly not take place.

Ask the modern prodigies of industry, who are the legitimate sons of scientific progress; interrogate Franklin, Stephenson, Daguerre, Edison, and many other benefactors of humanity, if it is true that they have bridled the thunderbolt. They have allowed man to nearly suppress distances; they have given to us the honor of having for our painter the light-house of the world, and placed most fearful lightning in our parks and houses as a rival to the sun. But they were always true and faithful ministers of science; they were quiet, and never made pompous promises until they had discovered all the secrets of the phenomena which they had undertaken to study. If but one link of the long chain of their researches had not been properly forged and welded, none of us can doubt that the chain would most certainly have been broken at the trial, and that the miracles prematurely announced would have been lost forever. This, then, is what unquestionably distinguishes real scientific or experimental progress. In the biological sciences, and above all in the progress of pathology and of therapeutics, this fundamental principle has too often been forgotten, and this forgetfulness appears to me the real cause which has paralyzed, up to the present, the useful results in proportion to the immense mass of researches made in the field of medical sciences. It is repeated on all sides that there has been discovered thousands and thousands of facts. You continually hear of these facts, and of a thousand new experiences to excuse the forgetfulness of the experimental method. But, as De Candolle, Chevreul, Bernard, and many others kept continually saying, in real science it is digested facts, and not crude ones, which nourish thought, and that nutrition may be perfect, it is indispensable that a just proportion between the quantity

of nourishment and the power of the digestive organs be established (see "*Histoire des sciences et des savants depuis deux siècles*"). This boast, which many modern scientists make, of not wishing to discover any but new facts, really is nothing more than a reaction of the natural philosophy that ruled about the beginning of this century, especially in Germany, and which gave to the mind an exaggerated preponderance in the interpretation of the phenomena of the external world. But if the excesses of reasoning in the progress of experimental science opened the doors to the present apparition of scientists and of experimenters purely empirical and skeptical, the contrary excess, that is the complete absence of all reasoning on the facts which are observed, leads to the complete loss of the great benefits of the experimental method. In the experimental sciences isolated facts are a luxury, vain and perilous, when they are not discovered under a common directing principle, or at least from time to time coördinated, put in connection, and lighted by a connected logic, that is afterward proved true by later experiences. And thus, also, in medicine, riches isolated and divided cannot by themselves produce useful progress to clinical medicine, because the physician at the bedside, after a minute analysis, has need of a great synthesis to formulate wise advice, and without synthesis even an encyclopedic physician is ruin for the patient. I beg you, illustrious colleagues, to fix your kind attention on these ideas, as it appears to me that the future will not be different if the true lovers of progress do not counsel the rising generation to proceed with the same rigor, so that the experimental method may conduct to prodigious results of the physico-chemical sciences, and of physiology, because there cannot exist two different experimental methods; the experimental method is one, and is that which was born with Galileo, and its code is always the same. Undoubtedly, with few exceptions, the feverish desire to *do fast* has interfered with doing well, and on this account it is that, while for over half a century the absolute reign of the experimental method in medicine has been proclaimed as a principle (or dogma) of progress, in reality we are spectators and actors of real barbaric onslaughts, which are the systematic invasions that are the negation of the experimental method. It matters little if a system be constructed in the name of an hypothesis, and another in the name of a new and true fact, and it matters little whether these systems are called vitalism or contra-stimulism instead of cellular pathology or bacteriology. These differences in names only imply that, instead of having a strong passion for the fantastic hypothesis of the old school of medicine, we first stop on the discovery of a new fact, and then think we see beyond the fact, constructing instead hypotheses; but this is exactly what constitutes the system, *an edge of truth which it is sought to proclaim and impose as the total and absolute truth, with the effect of dethroning the rest.*

This progress is anti-scientific, and at a given moment must from necessity come to a stop. To-day, unfortunately, medicine continues to be the victim of systems, and the system of the day is bacteriology. For those who sincerely love the progress of medicine it appears to me that hiding from us this dangerous reality is little scientific charity. It would undoubtedly be childish, if not dishonest, to ridicule the great teachings which are included in the discovery of a real microcosm incessantly at war with mankind. It is true that it is written in the pages of the chosen masters (Brieger, Hayem, Klebs, Sternberg, *et al.*) that the limit which we must dream of for the present in this new era of pathology and therapeutics is clearly given; but the current of mediocrity suffocates all, conquers the masses, carries with it the less reserved, and fills with enthusiasm those who have no scientific faith and who are ready to cry Hosanna to-day to Christ, and to-morrow to Mahomet. The only fact that has allowed of this systematic invasion, a hundred times more powerful and

incomparable in its dominion than that of cellular pathology, is the complete forgetfulness of the laws of the experimental method in the progress of medicine.

Without pretending to search for the prophets of bacteriology in Lucretius' poem, "De natura rerum," and in the *contagium animatum* of the middle ages, I prefer to tell you that the idea that living microscopic germs, penetrating insidiously into us by our lungs, stomach, and skin, are capable of developing certain determined diseases, is not new, and has in other periods presented itself to the minds of physicians under another name, and it would be enough to mention the universal panacea of camphor that checkmated, less than half a century ago, the greater part of the medical faculty. But these attempts did not contribute to start micro-biology, of which the first light is undoubtedly due to Cagnard Latour, who had formally announced that if yeast made from beer caused sugar to ferment, it was on account of *some effect of its generation and of its life.* No one could then have believed (in 1825) that in these words were included the germ of one of the most fruitful naturalistic discoveries of the nineteenth century. Nor do I pretend here to trace to you by what rigorous and scientific ways this great discovery has made its way, and what obstacles it has had to overcome. But I must recall the memorable researches of Rayer and Davaine in 1851 on the bacteria of carbuncle, and those of Pasteur on the transformation of lactic acid into butyric acid (1861), and on the diseases of silkworms; these were the starting-points of the present scientific researches, so much the more remarkable inasmuch as they show that when science has followed the bed of experience without overflowing, it has always come to indestructible and lasting results, things continued to go on in the right way for many years, and everything seemed as if it would proceed with the greatest experimental rigor.

Sufficient would be the works of Raulin, who was the first to present to us the vast horizons that were opening before the scientific physician regarding the physico-chemical conditions necessary for the development of the bacilli, studying in a given space the conditions favorable and necessary for the development of the mold commonly known under the name of *Aspergillus niger;* he demonstrated that the smallest trace of nitrate of silver in a liquid specially prepared was enough to stop the development of this mold. This should have infused into the physician great reserve and great prudence, but for more than ten years micro-biology, instead of proceeding by measured and sure steps, assumed to have become all pathology. It was a real whirlwind that enveloped everything in it; and alongside of precious discoveries, such, for example, as that of the bacilli of carbuncle and of tuberculous and some other diseases, which were really an honor to science, sprang up on all sides microscopic researches on the existence of new microbes in all diseases, and it appeared as if each disease had found its real cause. For the malarial infection alone, after the *palmella* of Salisbury, six or seven microbes were discovered, up to the new *Plasmodium malariæ*, destined perhaps to die before it is registered, because authoritative researches (Tommasi Crudeli) have demonstrated that its presence in the corpuscles of the blood was illusory, the alteration of which is due instead to a retrograde metamorphosis of the red corpuscles (Mosso). For the last five or six years it has been impossible to open a newspaper without finding registered the discovery of one or more new pathogenetic microbes, and it must be said that, while attention was turned away from so many unsolved problems of pathology, the easiest way to enter upon the road to celebrity was, and is still, to announce the discovery of some new micrococcus or bacillus in such or such a disease. It has been a general blindness; pathology has almost been proclaimed by some to be a mere corollary of bacteriology. Every fashionable clinic in vogue found it indispensable to

have next to the sick-ward a cabinet for microbe cultivation, and so far no harm has been done, for such researches may prepare for the future, but should always remain in their places. But what may appear to some of this auditory incredible is that, as some of the clinics were not rich enough to maintain the laboratories of bacteriology, they opened a laboratory for the cultivation of bacteria and closed a sick-ward; and it happened that in some of the great charity hospitals, during the same day and same hour, the director, on the one hand, begged the medical corps to moderate the prescribing of meat for the sick, threatening otherwise to diminish the number of patients, and, on the other hand, proposed spending a new sum for meat necessary for the cultivation of bacteria in a special institute. If these accounts were not capable of proof, they might be believed to be tales of the middle ages, and, in fact, in some of the present dissertations on this subject, we find romantic descriptions of the lives of the microbes, of the battles and slaughter in the phalanxes of the different microbes which exist in our organism, enough to make us believe that we have returned with scientific form—that is, with a new *mise en scène*—to the battles of the acids, of vital spirits, and of the archeri which rendered Sylvius and Van Helmont celebrated. The innocent public, only desirous of knowing that a sure remedy would be found for every disease, and that even epidemics would be checked, and above all that of cholera (as the great clamor which surrounded the discovery of the comma bacillus was the furnace of this hope), enthusiastically applauded this discovery, which was proclaimed on all sides the true philosopher's stone of pathology, and the physicians, on the other hand, not willing to lose these praises, although so premature, hastened with unheard-of zeal to proclaim that the only intent in the cure of disease must be that of killing the microbes, or at least of making a sharp war against them, so that they might not devour the organism, while this latter, unfortunately, most of the time succumbed to the action of the anti-parasitics. It is impossible to say, and, moreover, it would be a superfluous work to state it here, with what criminal audacity and with what ridiculous childishness, at the same time, the brains of those physicians found it easier not to follow the thorny way of the experimental method, but grasped thirstily at every new idea, and magnified and exaggerated it, believing that this was enough to stamp them as progressive men. A learned and honest cultivator of medicine who would like to amuse himself need only observe the most absurd cures which have been proposed for the most serious diseases during the last ten years; and, for the honor of progress, it must be said that it could not believe its eyes.

And all these curative propositions were made, and are still repeated, in the name of scientific medicine and of rational therapeutics. It is enough to remember the unfortunate attempts at many different clinics, with the most poisonous parasites, to kill the bacillus of tuberculosis in consumptives, but which contributed, instead, to aggravate their miserable destiny. Sufficient, also, is it to remember that carbolic acid and salicylic acid were proclaimed as remedies sure to abort typhoid fever and cholera. But, on the other hand, we must not be unjust to the honesty of the progressive men, for a certain remorse made itself felt in these physicians, who kept putting the organism between the door and the wall, playing lightly with terrible poisons. So some concluded to come to a shrewd compromise, using certain dangerous medicines in such doses as to render them harmless, but at the same time ineffective, according to the teachings of the laboratory. Undoubtedly in this way the life of the patient was guaranteed against the dangers of the cure, but undoubtedly none the less was the physician who pretended to be a scientist a charlatan. But the honor of progress at least was saved before the public, which in reality, ignorant of how much carbolic acid was necessary to kill this or that bacillus, and seeing the

patient cured, cried out, *Eureka*, and blessed in every way this new kind of medicine. Nor did they hesitate to refer to Darwinism applied to therapeutics in some diseases. "Struggle *for life*," gentlemen—this is the formula, the fight for existence; and they thought to kill the bacilli of tuberculosis by introducing into the respiratory organs other bacilli believed to be innocuous, but in reality only making the efforts of the former worse, which fact has since been demonstrated in important trials in laboratories and clinics.

I might still continue the enumeration of the new methods of cure proposed for all the diseases which were believed to originate from parasites, but it would be time lost. That which I am anxious to impress upon you is the systematic blindness of similar currents of ideas not able to reduce themselves even before the most overwhelming death statistics. The physician, when he is blinded by a system, always finds a good reason to console himself for his non-success, and continues in this headstrong way to travel along the road into which a preconceived idea has made him enter. Still, for the calm observer—really the friend of progress—who has arrived by the experimental method, it would have been so easy and so natural to foresee those curative failures, since that immense mass of microscopic studies, applied first to pathology and then to therapeutics, with few exceptions, were built on sand, completely trampling under foot the laws of the experimental method. It is true that man lives continually besieged by myriads of microscopic beings that are always ready to invade his organism, and to contend with him for the materials of which he has need in order to live; and it is not less true that sometimes in this combat for existence some of these infinitely small beings, that have the same right to live as he, are capable of causing in our body revolution and death. Behold, one of the greatest conceptions, an indisputable conquest of modern biology, and perhaps one of the most eloquent teachings for man, who, superior in physical force to the greater number of animals by which he is surrounded, accustomed to pitch his tent from one pole to the other, to substitute for the immense virgin forests the luxurious fields, and destroy all with iron and fire, preserving only what he sees fit, and who had proclaimed himself King of Nature, whose motto for his own use was *vœ parvis*, it did not seem possible that he in his greatness should humbly submit to the inexorable laws of circulation of the matter and force—and instead the microscope has placed under our eyes a new world, and he has been obliged to remain stupefied by the untold activity which therein takes place.

There are, without doubt, pathological microbes; this point of the science was proved by the researches of Davaine and of other illustrious microscopists. After these ideas the problem which naturally arose was that of discovering with what mechanism these enemies of man, so infinitely small, dared to attack and sometimes destroy a fortress so far superior to their invisible power. This grand problem which modern biology has placed before us was worthy of occurring in this great century, but on account of its immense difficulty it was necessary to impose the most scrupulous experimental rigor, and to profit with success by this revolution of the science there was need of the measured step of many and many generations, and to hurry things would only lead to those illusory exaggerations which we to-day must deplore. This is in reality the only reason by which the bacteriology of to-day, instead of constituting a gradual progress, constitutes a real systematic invasion, which is greatly favored by the inborn tendency of man to wish to explain everything at once. It is this disposition of man which has led experimenters to generalize beyond what they have seen, and decoyed them too far off from their starting-point. Instead of this, they should, before all else, have been able with great rigor to distinguish the true

from the false enemies. If it is unfortunately true that some of these microbes are capable at times of killing a human being in a few hours, it is none the less true that ordinarily we can take in with impunity thousands of them in the water which we drink and the air which we inhale, and many millions during every twenty-four hours we live (Miguel's analysis of the air of the rue de Rivoli), still preserving the best of health. Naturally from this sprung the doubt whether the small fraction, so terrible, confronted by the immense majority of innocuous cases, did not have some connection with exceptional and invisible diseased conditions of those organisms which were attacked and overcome.

If, therefore, there was need of minute study of so many microbes, it was absolutely indispensable to investigate the special qualities of the soil in which they flourished, one of the most fundamental researches, beyond doubt, but at the same time nearly impracticable, when we reflect upon the intra-organic sphere of the microbes. This research was completely suppressed. No one has dared to face it, and let this be said in honor of biological chemistry. But, then, how was it possible to undertake the experimental solution of a problem of which one of the factors was unknown? Is it enough to observe in the blood of a patient a microbe to say that it has been the real cause of his disease? What is the limit between the microbes that are perhaps indispensable to normal life and those which produce some specific disease? Is it indeed enough to determine that such a microbe belonging to a pathogenetic type is really capable of causing a disease when it penetrates into the organism? It is allowable to doubt, according to the researches of Klebs, who affirms that in the same manner that there exist vegetable species that are poisonous, there are other species belonging to the same group and same family, and closely allied to the preceding ones, which have not this poisonous quality and can in no way acquire it—just as there are pathogenetic microörganisms existing that cannot be distinguished from others perfectly inoffensive, the latter preserving their innocuousness in any condition and during the whole of their existence. And I will further state that in the same way many plants, poisonous in their wild state, become harmless and even edible upon being cultivated, still preserving, intact and unchanged, all their morphology. Does this occur in the same way with the microbes, and does their morphology become inoffensive? The experiments made by their cultivation in other spheres authorizes us to conclude nothing, if we wish to obey the laws of the experimental method. Which, after rigorous researches (up to the present day) of the two, three, or four pretended microbes that have been discovered in the same disease, is the true pathogenetic microbe? The authors of these observations have not been able to decide for themselves, and the colossal studies which are multiplying on every side with the cultivation of these microbes, we must honestly confess, have given results which are very questionable, if not altogether negative.

In general, the experimenters have shown themselves easily contented, and have always hastened to declare that from their inoculated cultivations such and such an *identical* disease was produced. But I appeal to the good faith of all these famous colleagues, who, if they are lovers of the discovery of truth, will tell me which are the artificial diseases produced by the inoculation of this cultivation, and if the natural primitive disease from which the germs were taken has really been reproduced; truly, I only know from human pathology of the existence of the bacillus of carbuncle, and it might be also said that of tuberculosis, but who can say he has seen reproduced a true paroxysm of malarial fever, or real diphtheria, or other diseases attributed to microbes? The infinite numbers of microbes to which successively have been attributed many of these diseases testify against the presumed results announced

by different experimenters. But they will confute me by saying that the reason of this want of success was the difference of the field, because the pathogenetic bacteria, like all parasites, prefer a certain kind of animal—for example, the bacilli of carbuncle live prosperously on graminivorous animals (especially ruminants), whereas they prosper very little in the organism of the carnivorous kind, and have no effect whatever on dogs, therefore, the same predilection must be admitted for the bacilli of the diseases of mankind. Likewise the objection can be made that the virulence of some bacteria is considerably modified during the time which has passed since they were cultivated outside of the body of the animal, and that no less influence has been exercised by the aliment that served for the cultivation, and that to many other microbes the same thing occurs as happens to that of Davaine's septicæmia of rabbits, that cannot be inoculated in guinea pigs, and to that of Koch's *virus*, that, likewise, cannot be inoculated in guinea pigs. But, then, of what use are the pure cultivations? What serious demonstration can they furnish of the affinity between such a microbe and such a disease, if the physico-chemical conditions of the soil are an indispensable element for the success; and if up to the present moment we absolutely know nothing, scientifically speaking, of the physico-chemical conditions which the blood of such or such an animal presents as a favorable field of cultivation for this or that microbe? This, as has already been said in the beginning, is the real scientific notion that explains the inoculation or the non-inoculation of pathogenetic microbes. And on this proposition I believe that there is not to-day a real scientist who has any illusions. These physico-chemical peculiarities of the blood, on account of its biological chemical conditions, will not be discovered for a long time to come. Inform yourselves of the nature of this problem (Raulin's liquid), and the culture of the *Aspergillus niger*, as likewise the present conditions of chemical hæmatology. What, then, is to be said of the receptive and non-receptive condition for the same pathogenetic inoculation in the same species of animals? The problem always becomes more inaccessible to our researches, and he that sustains the contrary cannot speak in good faith without showing that he is no scientist. I do not see in all this the least shadow of the true experimental method. Sometimes we obtain by the inoculation of the supposed pathogenetic microbe a morbid effect, but it does not reproduce the disease. Another one produces no effect whatever, and then the reason which is invoked is the badness of the subject. And then, again, the same pathogenetic microbe is shown to be capable of producing two different diseases, as, for example, the *Diplococcus pneumonicus* and the *meningococcus* (*P. Foa* and *G. Bordoni Uffreduzzi*), which last result would completely destroy the pathogenetic electivity of the microbes, which ought to be one of the principal attributes of parasitical etiology. But what, then, do they wish to demonstrate in this way? Experimental logic shows us that we cannot always demonstrate that which we desire. This, in fact, is the reality of the thing for one who wishes to understand it. And all this without even mentioning that, in certain very serious diseases, it has been as yet impossible to discover a microbe (hydrophobia), and that there are many and many others in which the etiological influence is so primitively chemical that it would be impossible to abdicate to the most ordinary good sense. We should have to suppress physiology! Let us take, as an example, a being who, in a perspiration, goes into a grotto, and who after a few hours is seized with a violent attack of rheumatism that continues two months or more. In this case, would you want me to search for the pathogenetic microbe without seeing at once that this profound functional cutaneous perturbation, which produced fever, was caused by a chemical mechanism to me unknown? Even when it is demonstrated that in the blood of this patient there

existed microbes, I should still be of the opinion that they were developed from pre-existing germs, and were one of the effects, and not the cause of the disease. This example can easily be multiplied in many serious diseases, due without doubt to grave functional disorders caused by errors of hygiene. The same is said of the pathogenetic influence which is attributed to some microbes, while in reality they alone are not capable of producing the disease attributed to them, for example, osteo-myelitis purulenta and acute endocarditis—invoking a demonstration that is the negation of the experimental method. It is known that when a pure cultivation of the *Staphylococcus aureus* is injected into the blood vessels of an animal (for example, a rabbit), probably it will die; but it does not die from osteo-myelitis, mark you, but from the so-called general infection, and none of the localizations in the bony marrow are observed (Weichselbaum, "Klinische Zeit- und Streitfragen," Vienna, 1887). But if, before or soon after the injection, a bone of that animal is bruised or broken, then an osteo-myelitis purulenta is produced. I would frankly ask of any honest scientist if, with the experimental logic at hand, it is allowable to conclude or give to the world as a demonstrated fact that the *Staphylococcus aureus* is really the cause of this terrible disease in man, and also in the poor rabbit? But how can we admit the electivity of this pathogenetic microbe for the marrow of a normal bone if there is need of the intervening of another cause of illness, and how can we, then, logically infer that this microbe is the true cause of the osteo-myelitis that the physician observes in nature?

The same is said of ulcerative endocarditis, that is never produced when we inject into the circulating torrent of an animal those bacteria that are to-day considered as the cause of this disease, while on the contrary, if after the injection a valvular injury is produced, for example, offending mechanically by the use of a sterilized sound introduced into the carotids, the bacteria circulating in the blood fasten themselves on the valve and produce an endocarditis with all its consequences (see former citation). The character of this experiment is complicated; there are so many different casual elements which take part in it, that I, for the honor of the experimental method, am chagrined to think that they can seriously be taken as a contribution to scientific pathology. In all these lightning studies and conclusions there is always a criminal infraction of the laws of the experimental method, because, as has been said above, it is impossible to follow the experimental method while ignoring one of the data of experience. Up to the present time it has been acknowledged that the experimental method was that which proceeded from the known to the unknown; instead of which, in all the experiments of bacteriology up to this time, there is always an x in the data of the experiments, and then the conclusion which is presumed to be accepted as a scientific truth, is the daughter of an unknown factor, merely an x. This x, which is the secret of the whole position, consists in the diseased physico-chemical conditions of the blood or tissues of this or that animal. I understand that, in comparison to its immense difficulty, it is easier to suppress this idea than to support it; but then this in reality would be a most excellent experimental method, *ad usum Delphini*, and will certainly never be the experimental method that was taught us by the grand masters, or be the experimental method that was invoked as the Palladium of salvation that was to regenerate medicine. Where are you, glorious memories of Galileo, of Torricelli, of Newton, of Volta, of Spallanzani, who taught the world the sacred code of naturalistic philosophy? Rise, for pity's sake, from your tombs to illumine once again with a ray of genius the daring pioneers of medicine; echo at least once more among us the magic voice of Magendie, of Liebig, of Claude Bernard, of Whewell, of Chevreul, and so many others that up to within

a few years had by their example preserved the real traditions of the true experimental method uncontaminated by any other passion, even though the excusable one of leading to a quick discovery of a truth not yet ripe, and then only can true scientific medicine be inaugurated. Bacteriology made its entrance into pathology as a hasty means of indicating new ways for the cure of diseases, as I have before hinted. But the attempts made or proposed (more than absurd) would merit ridicule, if, unfortunately, their consequences were not so tragic. Good God, I cannot conceive how in a general disease already developed it is possible to kill the microbes, admitting them to be the real cause, without killing the patient. Thus, to lessen this simple want of logic, the authors of these new methods have declared that they propose to kill the microbes in their primitive state, in the intestinal tube in cholera and in typhus, in the respiratory tree in tuberculosis, etc. In fact, they would do the same as is done in malignant pustule when at the right time the hot iron is applied. But the case is entirely different, as malignant pustule is seen as soon as it begins to manifest itself, and the expert surgeon recognizes it in time—that is to say, when the organism has not yet begun to participate in the infection; the patient is saved because, the point of departure being destroyed, all the cause of the disease is destroyed also. But in cholera, typhus, etc., this point of departure foreign to the organism is impossible to be seen in the required time, as in the former example, but the disease has already become general, and the destruction of the pathogenetic microbes at their point of entrance is no longer of any value. Let us not speak of tuberculosis, because, besides these most serious and peremptory objections, there is still another, and it is that the poor consumptive is doomed long before the bacillus can be observed in the expectoration, and the terrible condition of his general nutrition is certainly the most important part of the disease (notwithstanding what the pure parasitologists pretend), and precludes any hope of curing him, even if all the bacilli were to disappear as by enchantment from the respiratory tree. Nor no less absurd is the example which these anti-parasitical therapeutists invoke in their favor, citing the real miracles which Listerian surgery has wrought. It would seem to me almost childish to mention the difference which is apparent between an antiseptic preventive method and an antiseptic curative method. Without doubt it was due to the germ theory that Lister was inspired with the directing principle of that surgical revolution which will ever form a glorious halo to the English surgeon and one of the most precious conquests of the second half of the nineteenth century. But Lister—surrounding himself with those immense precautions of cleanliness, even scrupulousness, which constitute at least a good half of the importance of his method, and then purifying the medium in which are to be laid bare parts of the organism more or less profound—in reality did not propose any other idea than that of impeding the parasitic germs which beset us, so that they could not profit (as is their law of existence) by the open door that the surgeon of other times unconsciously presented to them, without ever dreaming that, like invisible vultures, they had thrown themselves on their prey, producing the terrible and deadly effects that are known to all. In reality, therefore, in all this the organism remains perfectly foreign, and it is absurd to invoke the successes of Lister's antiseptic method to justify the internal medication that is directed against those germs which have already taken possession of the organism. The same is to be said of the appeal that is made to the splendid microbiotic application which has been consummated to fight and limit the development of epidemic diseases, because the good effects of the destruction of the microbes as long as they are out of the organism have nothing to do with the cure of general diseases already developed. But I do not wish to lose

the occasion of observing that in reality the usual effects of the strongest disinfections still leave much to be desired, and that practically we are anything but near to possessing complete ideas about all the ways in which epidemic and contagious diseases are diffused. Therefore I do not hesitate, after a long clinical experience, to assert that even from this point of view bacteriology has exaggerated its presumptions.

Here, then, gentlemen, you have sketched out a guide to an experimental criticism of the true present state of bacteriology, and you see that its too hurried march does not merit the name of experimental progress. But up to the present I have not spoken to you of another colossal step which science to-day has made, and which demonstrates always more peremptorily that so many, many researches purely bacteriological, and so many improvised conclusions have done harm to the real scientific progress of pathology. To-day it is well known that it is not enough to explain the morbid symptoms of the different diseases, the purely mechanical action of the bacteria, nor the modification which they produce on the inter-organic sphere only by the alimentation of which they have need. At present the more important and terrible influence of the microbes consists in the complicated chemical actions which they determine in the materials of the blood, with the production of eminently poisonous substances of different nature; and on the production of a greater or smaller number, and by their more or less rapid elimination, depends the loss or the victory of the organism. This experimental discovery without any doubt will one day be fruitful of happy curative applications, much more than any direct cure against the microbes, because the absolutely chemical search is the only one that can make us conceive serious hopes regarding the discovery of the intimate mechanism of diseases—that is, of their nature. In fact, it is undoubted that the living animal organism is a vast and most complicated laboratory of chemistry, still dark in the greater parts of its intimate mechanisms in its healthy state, and much more mysterious in a state of disease; and it is likewise true that the general effect of drugs is a chemical action that modifies in various ways the inter-organic sphere deteriorated by disease. The intimate history of the most serious diseases, like that of the great remedies, was there to demonstrate even before these last experimental theories.

While we must presume that this new direction of medical studies, inaugurated so brilliantly by those pleiades of scientists, Nencki, Husemann, Gussenbauer, Gröbner, Kobert, Brieger, and others, will be more than fertile for the future of pathology and therapeutics, on the other hand, it alone would be enough to bring up again a discussion about the conclusions of parasitical etiology pure and simple, even if they were not prejudiced by the effect of the bad experimental method followed at present. With the idea of an innumerable series of poisonous alkaloids (of which, up to the present, we only know a few), it is easily understood that an exact notion of the physico-chemical conditions of the inter-organic sphere in each disease is the indispensable condition for constructing on the one side pathogeny, and on the other side therapeutics. The morphology of the microbes is relegated in the background of the picture. That which principally interests us to know is the method of rendering these inseparable companions of our existence inoffensive, inasmuch as they cannot disappear from creation; on the contrary, it is to be believed that they make a part of its harmony. Now, this preservation is purely physico-chemical. Or we must be contented to repeat with the hygienist of all times, *Keep yourself strong and robust*, preserve the health of the functions, etc., because the stronger and more resisting will be your organism, the more difficult will be the contracting of disease; or, if we wish to invoke the lights of science, to be mathematically sure

of what we should do, science must tell us plainly and openly what are precisely the physico-chemical conditions of the organism in which such and such a microbe will be able to grow and thrive, and produce its grave effects. Take, for example, pulmonary troubles. All ancients and moderns agree on the hereditary predisposition. Now, there is not a good clinician who has not observed that general characteristic decay of health which takes place in individuals as a prologue to tuberculosis. This is a decay of the so-called organic mixture, or, if you wish, of the change of material that is the prologue of pulmonary phthisis, and is precisely that which would allow the bacillus to reign in the lungs, and it is exactly for this that one hundred individuals can confront bronchitis over and over again, and mingle with consumptives, without ever becoming consumptive, while another poor unfortunate, who to all appearances is ruddy and strong, becomes consumptive after the first cold, because he has the misfortune to have phthisis in the blazon of his family. Now, frankly and in good faith, as honest scientists, what is the difference between these two types of organism—the difference of the inter-organic sphere or field of cultivation, as they mean to say? Why does the bacillus of tuberculosis not thrive in one, whereas in the other it lives and prospers? This is the Gordian knot. Science says that the second organism offers a good field for the bacillus and the first does not. But this idea is an *ignotum* for an *ignotum*. That it is called organic predisposition or field of cultivation matters little to me; on the contrary, the word predisposition is to be preferred, as it indicates the pure and simple fact without prejudicing the explanation with a phrase that, while it adds nothing to the concrete and useful, scientifically speaking, insinuates into the mind an hypothesis not demonstrated. I desire to know from science in what consists precisely this field of cultivation; that is to say, what are its physical and chemical conditions? And only when I know this with precision can I propose to myself to modify the field; that is to say, to make a rational preventive cure for tuberculosis. Up to the present I know of no other cure than the good hygienic conditions of high country air, good alimentation, etc.; in fact, whatever betters the physiological powers and augments the vitality of the cells is the only means that can increase the resistance of the organism and give hope in the beginning of a cure. But, unfortunately, in the greater number of cases we do not succeed, nor can we invoke the experience of the laboratory, which demonstrates that animals who have been inoculated with the bacillus of tuberculosis can sometimes fight against the power of the deadly germ when the nutritive processes are excited to the apogee of their power. But I should like to ask in good faith of any pathologist or clinician what similarity can exist between the diminished capacity of resistance (an indispensable condition) of an animal that pines away by degrees in a laboratory, on account of the bad conditions of the air and light, etc., and the long and unknown preparation that spontaneously takes place in the organism that has an hereditary predisposition to tuberculosis, *even when it is put into the most favorable medium?* Physiologically and clinically speaking, between these two conditions there is an abyss; science sees it and measures it, and is silent to my question; because real science does not venture beyond its depth. Real science, unfortunately, knows that the phenomena of the same order seen in the laboratory obliges us to defer certain aspirations to an undetermined time. Science knows that the smallest traces of nitrate of silver in Raulin's liquid—traces that the same chemical analysis can with difficulty discover—are enough to impede the growth of the *Aspergillus niger;* and from this it is deducible that for the present, and perhaps forever, the investigation of the physico-chemical conditions which make an organism a good field for the bacillus of tuberculosis, as opposed to a

refractory organism, is interdicted to us. What I have said about the bacillus of tuberculosis can be repeated about all other diseases to which to-day is attributed a parasitic origin. What can rationally be said about the receptivity or non-receptivity of individuals toward cholera, smallpox, diphtheria, scarlet fever, etc.? It is easy to say that it is a question of a good or bad field of cultivation, but these words signify nothing, and do not certainly constitute a scientific progress that bacteriology presumes to have realized. Nor is this all that peremptorily demonstrates the unfathomable abyss that surrounds the scientific knowledge of the predisposition of the different organisms, or fields of cultivation as they wish to call them. What is to be said of the immunity that a first attack of parasitic infection confers for a time more or less long, and sometimes forever, against a second attack of the same disease? Going always from hypothesis to hypothesis, we have invoked the excitement of the field of cultivation, no matter how powerful are the conditions of the budding of these germs in an inter-organic sphere; incomprehensible as it may seem, these conditions cannot reproduce themselves a second or a third time when a long period has elapsed after the cure of the first attack, and consequently the organism is completely restored and has absolutely returned to its normal state. What is the difference between a young man who, in his infancy, suffered from scarlet fever which has rendered him exempt, and another perfectly healthy man who, not having the disease, is likely to take it later in life? Absolute mystery! Please be kind enough to follow me still a little longer. Take ten organisms, all in a receptive condition for contracting scarlet fever, and which are taken with the infection as soon as an epidemic of scarlet fever begins. In one you will have the typical case that ends in recovery in eight days; in another, the scarlet fever becomes ataxic at the third or fourth day, and the patient dies, as dies also another who has the fever, in which the angina became diphtheritic; and, finally, a fourth, which, after the fever has run its course, develops into autopathic and adenopathic suppuration.

In this case we do not deal only with knowing in what consists the field of the healthy organism, favorable more or less to the cultivation of the microbe of scarlet fever; in this case the question is to determine the individual physico-chemical conditions by which the same infecting agent has radically modified the chemical productions of its fabrication, and from which, therefore, have been derived those fatal consequences that were unforeseeable on the first or second day of the disease. Let this be said only to humiliate those who pretend to proclaim bacteriology to be the philosopher's stone that is to enlighten the clinician, because, so far as regards the pure pathologists of the laboratory, I admire their innocence, and I excuse them, because they have never seen a patient taken with fulminant diphtheria or with hemorrhagical smallpox, and for this it is permissible for them to believe in good faith that pathology can be regenerated by the cultivation of the bacilli obtained in different broths, or on pieces of potato and other things.

If, then, you suppress the scientific idea—that is, the precise determination of the quality of this field of cultivation, an idea that, in fact, corresponds to the famous organic predisposition which the ancients and even the moderns admit—then good-by scientific medicine. We shall have to finish by resigning ourselves, as is allowable, to doubting pure and simple clinical observations. Nor need they come and tell me that we have made progress in this branch. Yes, it is true that we have acquired precious ideas, but they are very far from sufficient to construct the edifice, or to even serve as a useful foundation to therapeutics; thus they would have to construct with phantasy. Now, it is my opinion that a structure cannot exist half real and half false, half experimental and half hypothetical, because then we should

have to repeat with Victor Hugo, "*Je cherche un edifice et je trouve une ruine.*" I could have cited simpler examples referable only to external parasites of the organism. Take, for example, the experiments of Delafond and Bourguignon on the itch of domestic animals. Itch, as we know, is produced by an acarus nearly visible to the naked eye, that lives in the superficial stratum of the skin, and therefore it is not necessary to examine the morbid chemical changes of the inter-organic sphere, because the combat takes place on the surface of the body. Well, Delafond and Bourguignon have demonstrated that the acarus put on the skin of a well-fed and healthy sheep does not live, nor can it form colonies; but if, on the contrary, these sheep are first submitted to a bad alimentation and poorly stabled, the acarus easily finds a hold, and if the bad treatment is continued to these animals, the itch will extend considerably. If, at a given moment, the alimentation is bettered, together with the stabling and ventilation, then these simple hygienic agents, without any special cure against the acarus, are enough to make the itch disappear, and the animal recovers its primitive good health. Also the human subject presents examples of this kind of physiological therapeutics, due to the greater resistance. In what consists the physico-chemical change which has taken place in the organism of these animals, and in what way has this change been able to have such an effect on the skin that it would receive or refuse the parasite? To me it would appear that all, honestly, must recognize that to-day science cannot even propose the solution of these problems. Think, then, what is to be said of the physico-chemical conditions of the inter-organic sphere, and tell me if it is allowable to speak of therapeutics based on bacteriology. In fact, all the most learned and conservative cultivators recognize the fact, and all hope in a better future, limiting themselves for the present to consoling us for the therapeutic sterility of their studies with giving the clearest explanations of the mechanism with which powerful remedies—such as mercury, quinine, etc.—work the curative miracles that all the world knows. It can be said that these systematic progressionists envy the past, and that to demonstrate to humanity that they also are doing something to enrich or perfect the cure of disease, imitate the fly who settles on the back of the ox and cries out, fully satisfied, *aramus.* Naturally, their therapeutic explanation comes from their parasitical ideas, and therefore they teach youth that mercury cures syphilis, and that the salts of quinine cure malarial infection by killing the microbes which are the respective causes of these two pathological conditions.

Not wishing to further tax your patience, a few words will suffice me to demonstrate how the systematic domination of an idea can blind one, even to the point of presuming to turn to its profit assured empirical facts about which there can be no discussion. The parasitical experiences which they are always invoking are those which come from the laboratory, and which have always the fault of not taking into account the fact that in a living organism the factors which are encountered are very different from those which are found *in vitro.* Thus, for example, it is known that corrosive sublimate is one of the strongest parasite-killers, and it is used with great success to destroy the germs of infection under the form of washes, injections, etc., in the proportion of $\frac{1}{5000}$ to $\frac{1}{1000}$. This is a well-established fact, but it cannot be invoked to explain the mechanism of the anti-syphilitic virtues of mercurials, for two most simple and peremptory reasons, allowing as demonstrated (which it is not) that constitutional syphilis is a general disease determined by a special microbe (and I pray you to note that this first hypothesis, which is not yet demonstrated, would be enough to paralyze all future reasoning). But the hypothesis *crescit eundo.* If the salt of mercury were to circulate in an organism under the form of the

bichloride, it would still be easy to delude one's self. But it is the most elementary idea of experimental pharmacy that the salts of mercury are absorbed under the form of an albuminate, and that in contact with the excess of albumin, or perhaps with the chloride of sodium of the blood and of the tissues, the compound of albumin-mercurial is dissolved, and does its work. Now the solution of albuminate of mercury has not the antiseptic virtues of the sublimate, and this for a very simple reason; bichloride of mercury exercises its powerful antiseptic action because it seeks to combine with the albuminous materials of the pathogenetic germs; it (Hayem) could not have the same action after having already combined with great excesses of albumin. The microbes of syphilis might continue to live on quietly. Still, there are some who, not willing to give up, prefer to say that, after all, we do not know what composition or decomposition the salts of mercury are subjected to in the blood; denying, moreover, that which is proved in experimental pharmacy, and this only for the purpose of being able to start from the experience of a laboratory to arrive at an x for the pleasure of sustaining an hypothesis. But there are other, and not less peremptory, reasons demonstrating the error. It is admitted, for example, that corrosive sublimate, notwithstanding its transformations, kills the microbes of syphilis used even at minimum dilution of $\frac{1}{5000}$; one cannot go further. It is likewise admitted, which is erroneous, that the microbes of constitutional syphilis only circulate in the blood, and this makes necessary one of the most simple calculations of the proportions of sublimate required to sterilize them all. The quantity of blood which circulates in a medium organism is calculated at five kilogrammes. According to the parasiticals, the results *in vitro*, there would therefore be need that not less than one gramme of corrosive sublimate should be present and circulating at the same moment in the blood torrent. Renouncing the serious consideration that a gramme of corrosive sublimate present and circulating in the blood would cause fatal acute mercurialism, I will limit myself to observing that practice demonstrates every day that from sixty to eighty hypodermic injections are enough at the most to cure the most serious form of syphilis, using half a centigramme of bichloride of mercury for each injection, which amounts to thirty or forty centigrammes of corrosive sublimate in all—that is to say, the fifth or sixth part of that proportion, even the minimum, that, according to the laboratory, would be necessary! And if to this you will add that after the third or fourth hypodermic injection it can be demonstrated with certainty that the elimination of the mercury has already begun by means of the urine, it is an indisputable fact that the combined dose of albumin-mercurial circulating at the same moment is by far inferior to the entire dose of corrosive sublimate used in the whole cure. Nor can I help asking of these blinded followers of this singular experimental logic, why they have never—they who are so sure that corrosive sublimate and all other mercurials cure syphilis by killing its microbes—proclaimed that they have at last found the remedy for all chronic diseases that, according to their wisdom, are due exclusively to the existence of some kind of a bacillus or microbe? Why should not the use of sublimate, according to the same doctrine, cure pulmonary tuberculosis? There is no need that I recall here the deplorable effects of these unheard-of and audacious curatives. The same reasoning can be repeated for the mechanism by which the salts of quinine cure malarial infection, etc. The final conclusion is, therefore, this, gentlemen, that, going on with the everlasting guide of the experimental method, the present progress of pathology and of therapeutics founded on microscopic studies, we must, unfortunately, convince ourselves that, under the most seducing appearance of rational and experimental progress, they are nothing else than hypotheses and systematic ten-

dencies. It is, therefore, urgently to be hoped that experimental biology may once again recover that scientific rigor that is found consecrated in the immortal codes which our forefathers transmitted to us and by which only it is possible to prepare, little by little, the solid materials for the colossal edifice of scientific medicine. We shall be all the more largely recompensed for this immense work, inasmuch as it will be disinterested, because the edifice is so grand that, none of us being able to delude ourselves with the hope of seeing it finished, no one can be accused of ardently proclaiming progress for his own ends. We will work for the triumphs of future generations.

But do not let it be thought from this, gentlemen, that medicine is to continue in the way of traditional empiricism. This would be an easy accusation for those to make who in bad faith pretend not to understand the real sense of my discourse, and who, not wishing to conform to the rigor of the experimental method, prefer to have only progress on the lips and *ex cathedra*, while in practice they do not hesitate to become more empirical than their forefathers, with a humiliating polypharmacy for science and with a continual up and down of new remedies, first glorified as a miracle, and then forgotten without pity. No, gentlemen, empiricism is dead forever in every order of things. Modern civilization, conquering by its true scientific methods inorganic nature and organic nature, finds itself in conditions entirely new and unknown to old civilizations. The history of ancient peoples cannot always be logically invoked to calculate the destinies of modern people. Humanity understands, to-day, that its aim is no longer passive contemplation, but progress and action, and it is for this that the experimental method, passing from the physico-chemical sciences and physiology, is extending its influence over the historical sciences and over moral sciences. Humanity understands that it is not enough to remain an inactive spectator of good and bad, enjoying the one and cursing against the other; no, it searches for the cause it wishes to explain, and wishes to act on them, thus forcing itself to dominate good and evil; to develop the one and to battle with the other—to eradicate and destroy it. And it is you yourselves, Americans—great people of the United States—that are entitled to be admired by the whole universe as having applied these great principles of the experimental method to your political and social constitution. Those who, preferring unbridled license to healthy liberty, call you retrogrades, cannot be anything but rascals, that for their personal advantages sacrifice the limits of the just and honest, and aim at the triumph of disorder. Therefore, he who confounds empiricism with the just measure of the evolution of progress either lies or does not know what he says. The experimental method teaches us that its first step is the rigorous observation of facts, that is, the rigorous determination of this or that phenomenon. When this is well established, comes the research of the Why? which is the second step of the experimental method. Empiricism represents blind and confused observation, and, if the scientific physician begins by investigating the why of a phenomenon that is badly determined in his simple observation, then in reality the experimental method is wanting a base. This is the sense in which I say that the first duty of modern scientific or experimental medicine is that of transforming the empirical notion of facts into a scientific idea. The great treasures of clinical observations transmitted to us by tradition represent indisputably the everlasting basis of medicine. To-day the duty of the physician who wishes to be the real pioneer of scientific medicine is that of applying all the great truths of the past to the bright lamp of physics, of chemistry, of physiology, and of pathological anatomy. This illumination has already begun, and here is the first transformation of the empirical period into the scientific period. At the same

time, and accordingly as each clinical fact has been subjected to exact and rigorous tests, with so many new means of investigation comes the second period of research—that is, the research of the *why*, or the study of the conditions of existence of different pathological and therapeutical phenomena; and it is thus that this slow evolution of experimental progress begins that can, only in time unknown, conduct to the definitive construction of scientific medicine. But, if in this long journey the right path of experimental medicine is lost, the same thing will take place that has occurred for the last half century—that is, while the auxiliary science of medicine is throwing bright light on the many mysterious problems of pathology, physicians are allowing themselves to be conquered by impatience, and prefer to build the progress of therapeutics on hypotheses rather than on rigorous experience, and thus are always going further away from the definite scientific solution.

This, my oldest, my dearest, and most ardent vow for the future of medicine in general, and, above all, for *Italian* medicine, I am more than happy to repeat here among you all, worthy sons of the freest land, because, gentlemen, liberty and patriotism are the natural and indispensable allies of the progress of science, and one of the fundamental conditions for attaining the experimental method is the nationality of thought, or the abolition of all kinds of intellectual slavery, which is a hundred times more humiliating and damaging than the slavery of chains. It is true that science has no fatherland and much less confined boundaries, but it becomes universal only when it is already constituted; but while it finds itself in the state of evolution —that is, in a state of truth not yet demonstrated—each people has special dispositions and special ways of being and hearing, and for this each people or nation gives a special characteristic to its studies. To those people or nations that, having glorious traditions and therefore undeniable duties, prefer to imitate rather than to create, I would recall the words of Virchow in the congress of German naturalists at Hanover in 1866, which were that "science is unproductive when it has not a national character." And that Germany herself, with Oken at its head, cries to those who make their scientific progress consist in imitating, "Imitate no one, if you wish your sciences to revive and regain their ancient greatness." In no corner of the world can this voice be so powerfully echoed for the experimental method as on these shores of great modern liberty. Long live the alliance in medicine of the experimental method and scientific independence, an alliance which to conquer does not need bayonets, cannon, nor victims.

At the conclusion of Professor Semmola's address, which was many times interrupted by the applause of the audience, Vice-president LEWIS A. SAYRE, of New York, said:—

MR. PRESIDENT: I am sure that all of the members of this Congress have, like myself, been electrified with delight in listening to the eloquent address of our distinguished member, Professor Semmola, of Italy. But, sir, for his masterly exposition of the important subject of Bacteriology and its relation to therapeutics, and the wonderful influences which the experimental method should have in affecting the practice of medicine in the future, I feel, sir, that the whole profession are under deep obligations to him, and I therefore rise to offer him a unanimous vote of thanks.

Dr. W. H. HINGSTON, of Montreal, seconded the motion for a vote of thanks, which was unanimously carried.

Dr. ALBERT L. GIHON, U. S. N., then offered the following resolution, which, on being seconded, was put before the Congress:—

Resolved, That the President of the Congress be authorized to appoint a committee, to consist of an equal number of members from each nationality represented in the Congress, for the purpose of selecting the place of meeting of the Tenth International Medical Congress, to be held in the year 1890, which committee shall report on Friday morning, immediately before the address of Dr. Blandford.

The resolution was adopted and the general session adjourned for the day.

In the evening private receptions were given by Honorable JOHN M. GLOVER, M.C., and Mr. THOMAS WILSON.

FOURTH DAY.

The Congress again met in general session on Thursday, at 10 A.M. The President announced as the Committee to select the next place of meeting, by countries—

Dr. Servais, Antwerp, Belgium.
Dr. Freire, Rio de Janeiro, Brazil.
Dr. Boone, Shanghai, China.
Dr. Grant, Bey, Cairo, Egypt.
Dr. Landolt, Paris, France.
Dr. Martin, Berlin, German Empire.
Dr. Pavy, London, Great Britain.
Dr. Semmola, Naples, Italy.
Dr. Saiga, Imperial Japanese Navy, Japan.
Dr. Alvarado, Mexico, Mexico.
Dr. Gillavry, Leiden, Netherlands.
Dr. Charles Reyher, St. Petersburg, Russia.
Dr. Georges Assaky, Bucharest, Roumania.
Dr. Lalearda, Seville, Spain.
Dr. Post, Beirût, Turkey.
Dr. A. L. Gihon, U. S. Navy, United States.

The SECRETARY-GENERAL then read the following resolution and moved its adoption :—

WHEREAS, it is proposed to hold at the City of Washington, in 1892, an international celebration in honor of the 400th anniversary of the discovery of America by Christopher Columbus, and an exposition of the history, arts and industries of all nations ;

Resolved, That the International Medical Congress favors this patriotic movement, and commends it to the nations of the world.

Professor SEMMOLA rose to second the resolution. He said :—

GENTLEMEN : There are things so great in the history of humanity that it is not possible to glorify them as much as they deserve. The discovery of America by Christopher Columbus is one of these. I have been educated from my infancy to venerate the memory of that immortal Italian, but I confess that, now that I have crossed the ocean, with all the facilities that steam and nautical science afford, it is the first time that I have been able to conceive a correct idea of the genius of Columbus, and to appreciate his enterprise, his hardihood, and the foresight which never failed him the last day of his journey, so that now I prostrate myself before Columbus and worship him as of Divine emanation.

Therefore, gentlemen, I rejoice with you and congratulate you from the bottom of my heart for the noble sentiment that has inspired you in proposing an international testimony in honor of the immortal son of Genoa, and be it as an Italian, or as a member of the parliament of Italy, as delegate of my government to this Medical Congress, and even as one of its vice-presidents, with infinite delight do I associate myself with this proposition and thank you most sincerely.

The motion was then put and the resolution unanimously adopted.

The President then called Vice-president MARTIN to the chair, and Professor UNNA, of Hamburg, delivered the following address :—

DIE DERMATOLOGIE IN IHREM VERHÄLTNISS ZUR GESAMMTMEDICIN.

Wenn es einem Specialisten vergönnt ist, an dieser erhabenen Stelle zu den Vertretern des gesammten Aerztestandes der Welt zu reden, so giebt es — will er ganz auf seinem eigenen Boden bleiben — eigentlich nur *ein* Thema, welches diesem Ort und Zeitpunkt angemessen ist: *das Verhältniss seiner Specialität zur Gesammtmedicin.* Der wahre Specialist, derjenige, welcher aus reiner Begeisterung für sein Fach den gewöhnlichen Gang der ärztlichen Laufbahn verlassen hat, kann nur einen einzigen Weg gehen. Theoretisch und womöglich auch praktisch geschult in der gesammten ärztlichen Wissenschaft und Kunst, wird er sein ganzes Leben dem doppelten Zwecke widmen, Samenkörner aufzulesen auf dem unermesslichen Felde der Gesammtmedicin und in seinen Specialboden zu versenken, um die Ernte, welche ihm hier emporwächst, als reife Frucht dankbar der Gesammtmedicin zurückzugeben.

Von diesem Standpunkte, meine Herren, lassen Sie uns heute das Verhältniss der *Dermatologie zur Gesammtmedicin* betrachten. Lassen Sie uns ganz absehen von jenen kleinlichen äusseren Umständen, welche gewöhnlich zur Entschuldigung der Existenz von Specialfächern angeführt werden. Wir wollen nicht daran denken, dass der Specialist auf seinem kleinen Gebiete ein umfassenderes Wissen, eine grössere Geschicklichkeit erwerben kann und muss, als irgend ein anderer einzelner Mensch auf dem Gesammtgebiete der Medicin. Dieser Umstand, welcher mit eiserner Nothwendigkeit die Specialitäten geschaffen hat, ist ja auch leider der, welcher sie ewig von der Gesammtmedicin trennt.

Bedienen wir uns vielmehr heute der erfreulicheren Fiction, die Specialitäten seien geschaffen durch die wachsende und reifende Einsicht des auf der Höhe stehenden Arztes und Naturforschers, durch den idealen Wunsch nach Erweiterung der Grenzen unseres Naturerkennens. Dann wird sofort unser Auge geschärft für Dasjenige, was sie mit der Gesammtmedicin auf ewig verbindet, für das Interesse, welches die Gesammtmedicin ihrerseits hat, die getrennte, specielle Bearbeitung einzelner Wissensgebiete fortdauernd zu wünschen und zu begünstigen.

Ich betrachte meine heutige Aufgabe als gelöst, wenn es mir gelingen sollte, Sie davon zu überzeugen, dass die Gesammtmedicin, dass jeder praktische Arzt ein grosses, ideales Interesse daran hat, dass die *Dermatologie* in einer viel intensiveren und extensiveren Weise als bisher von möglichst zahlreichen Kräften gepflegt werde, dass diese junge Tochter der Medicin solch' besonderer Aufmerksamkeit und Fürsorge werth ist und ihrer Mutter die aufgewendete Mühe sehr bald und reichlich vergelten wird.

Die Dermatologie ist in der That noch eine junge Wissenschaft. Nicht, als ob nicht schon seit einem vollen Jahrhundert berufene und unberufene Kräfte an ihrem Ausbau arbeiteten; eine nur zu zahlreiche Menge einzelner Thatsachen, vorläufiger Systeme und endloser Namen giebt von dieser Arbeit Kunde. Aber als Wissenschaft betrachtet, ist die Dermatologie im Laufe dieses Jahrhunderts aus dem Stadium ununterbrochener Gährung, eines ewigen Umbaues nicht herausgekommen. Vergeblich sehnen wir Dermatologen von heute uns nach einem behaglichen Ruhepunkte, nach der friedlichen Entwickelung, welche, z. B., die Ophthalmologie seit den Epoche machenden Arbeiten eines Helmholtz, Donders und Graefe auszeichnet. Jede wichtige, neue Thatsache, welche auf dermatologischem Gebiete auftaucht, ist nur zu geeignet, das ganze bisherige Gebäude in's Wanken zu bringen und uns an seine Hinfälligkeit zu erinnern.

Und diese Wissenschaft, deren bisherige Signatur — trotz aller praktischen Erfolge — die theoretische Unzulänglichkeit und der ewige Wechsel war, sie soll der Gesammtmedicin die verheissenen werthvollen Früchte bringen? — so höre ich Ihre erstaunte Frage. Und doch muss ich antworten: Ja — und so paradox es klingen mag, sogar gerade deshalb. Die Ursachen, welche der Entwickelung der Dermatologie hindernd und verzögernd in den Weg treten, sind gerade in denselben Umständen begründet, welche sie dereinst für die Gesammtmedicin so werthvoll machen werden.

In keinem anderen Fache der Medicin sind Beobachtungen so leicht zu machen, aber auch nirgends so schwer zu definiren und zu erklären, als auf der Haut. Die verwirrende Vielgestaltigkeit der Vorgänge, die sich auf der äusseren Decke abspielen, genügt allein schon, um die langsamen Fortschritte der wissenschaftlichen Dermatologie zu erklären. Wie es unendlich viel leichter ist, die Eigenschaften einer Flüssigkeit zu erkennen, wenn sie langsam, tropfenweise, etwa aus einem Filter zu Boden fällt, als wenn sie in grosser Menge in einem offenen Kanale rasch dahinfliesst, so verschwindet auch leicht das einzelne Symptom einer Hautkrankheit dem von dem Gesammtbilde gefesselten Blicke des Beobachters. Die Erforschung der Nierenkrankheiten durch Beobachtung des Harns, der Lungenkrankheiten durch die des Sputums, der Krankheiten des Verdauungstractes durch die der Faeces und des Erbrochenen gleicht jener spärlichen aber genauen Beobachtung des Wassertropfens am Filter. Die Aufmerksamkeit ist lediglich auf die Variation eines einzelnen Symptomes gerichtet; die Kenntniss desselben ist bald erschöpft, ebenso die Schlussfolgerungen für den Zustand des inneren Organs; beide sind wenig umfangreich, aber sicher. Eine umfassendere Kenntniss des inneren Organs erlangen wir nur allmählich durch die successive Combination und Vergleichung einer grossen Reihe solcher Einzelbeobachtungen; sie wächst nur langsam, aber stetig und befriedigend. Und doch bleibt sie immer sehr lückenhaft. Welche Aufschlüsse würde z. B. die Beobachtung der Niere in allen ihren Theilen bei den verschiedenen Nierenentzündungen ergeben, wo wir bis jetzt allein auf die speculative Verbindung der Harnuntersuchungen mit den Ergebnissen der Leichenbefunde angewiesen sind? Wenn diese Möglichkeit einträte, würde sofort eine grosse Reihe jetzt noch schwebender Controversen geschlichtet sein; die einfache Beobachtung würde unter denselben entscheiden. Dieser hypothetische Fall setzt jedoch voraus, dass die Symptomatologie des Organs bereits den hohen Grad von Genauigkeit erhalten hat, wie eben die Harnuntersuchung. Wäre diese nicht vorausgegangen, würden die Fragen nicht bereits genau formulirt sein, so würde uns die überraschende Mannigfaltigkeit des Bildes ebenso verwirren, wie jetzt noch die wechselnden Symptome der Hautkrankheiten auf die meisten Aerzte das Gegentheil einer klaren Anschauung hervorrufen.

Wir kommen also zu dem Schlusse, dass es für die Lehre von den Hautkrankheiten besser gewesen wäre, wenn beim Studium einzelner Symptome — der Sekretionen und Exkretionen z. B. — nicht der ganze Symptomencomplex vor Augen gelegen hätte. Das Wenige wäre dann mit allen Hilfskräften der Physik und Chemie eifrigst durchforscht, viele Fragen wären auf speculativem Wege gestellt und beantwortet und die Hauptfragen schliesslich soweit zugespitzt, dass, wenn nun auf einmal der Schleier von sämmtlichen Vorgängen genommen würde, unser Blick für diejenigen Punkte geschärft wäre, auf welche die Fragen der Hautpathologie hinauslaufen, und die einfache Beobachtung sofort zwischen Ja und Nein entscheiden könnte.

Doch das ist leider ein Utopien. Wir müssen bei den Hautkrankheiten eben unter der erdrückenden Menge von Symptomen leiden und mit ihnen rechnen. Wir haben keinen mechanischen Filter, um uns die Einzelthatsachen bequem, tropfenweise vorzuführen; wir besitzen nur einen geistigen in der angestrengt auf den einzelnen Punkt gerichteten Aufmerksamkeit und in der immermehr in's Einzelne dringenden Kenntniss vom feineren Baue der Haut. So werden wir, langsam fortarbeitend, die *störende Mannigfaltigkeit* des äusseren Bildes überwinden lernen.

Diese Mannigfaltigkeit tritt uns am schroffsten in *topographischer* Beziehung entgegen. Kein Organ des menschlichen Körpers weist — ausgenommen das nervöse Centralorgan, das zu einem Theil ein Spiegelbild der äusseren Haut darstellt, — so grosse topographische Unterschiede auf. Ein Segment der Leber, z. B., gleicht, was den inneren Bau betrifft, dem andern, jedes Läppchen zeigt in gleichmässiger Vertheilung Gallengänge und Parenchymzellen, Bindegewebe, Blutgefässe und Nerven. Betrachten wir ein complicirter gebautes Organ wie die Niere, das Auge. Dort haben wir freilich Gegensätze von Rinde, Mark, Papillen, aber dieselben wiederholen sich bei jedem heilförmigen Segment in derselben Reihenfolge.

Selbst das Auge, dieses Wunder zweckmässiger Organik, wenn ich diesen Begriff dem der Mechanik gegenüberstellen darf, findet sich wie die Niere annähernd radiär symmetrisch angelegt. Die Haut dagegen zeigt, wie das Centralnervensystem, nur eine bilaterale Symmetrie und in jeder der symmetrischen Hälften haben wir eine grosse Reihe in ihrer Struktur völlig verschiedener Regionen zu unterscheiden, so behaarte und unbehaarte, muskelreiche und muskelarme, mit dickerem und dünnerem Panniculus, mit Nervenendapparaten reichlich und spärlich versehene, solche, die Talg- und Knäueldrüsen, und solche, die nur Knäueldrüsen besitzen, mit dicker und mit dünner Hornschicht bedeckte, mit vollausgebildetem Papillarkörper und ohne solchen, mit gleichförmiger und ungleichförmiger Spaltbarkeit, mit stark saurer und schwach saurer, resp. alkalischer Reaktion des Sekrets, die Haut der Streckseiten und die der Beugeseiten, freie und Contactflächen, die eigentliche Haut und ihre Anhangsgebilde. Eine grosse Reihe von Hautaffektionen wird durch solche topographische Verschiedenheiten derart umgeprägt, dass es eines langen Studiums bedurfte, um die Zusammengehörigkeit solcher Affektionen zu begreifen. Ein sehr altes Beispiel dafür ist die in Frankreich und England sich noch heute in den Namen Porrigo, Tinea, Teigne fortpflanzende, isolirte Bezeichnung der Krankheiten des behaarten Kopfes, die ursprünglich wirklich für ebensoviele eigenartige Krankheiten galten. Als ein ganz modernes Gegenstück seien die drei Krankheiten der Impetigo simplex (Wilson), der Sykosis simplex und der Furunkulose angeführt, welche sämmtlich nach den Untersuchungen von Bockhart durch die bekannten weissen und gelben Staphylokokken verursacht werden, je nachdem dieselben auf der glatten, mit Lanugo besetzten oder auf der mit dicken Haaren versehenen Haut oder endlich in den Drüsenapparaten einen geeigneten Nährboden finden. Das für die ganze Pathologie wichtigste Beispiel ist jedoch das der verschiedenen Tuberkuloseformen der Haut. Wir haben an Händen und Füssen die relativ unschädliche Species des papillomatösen Lupus, der als Leichentuberkel beginnt und im Centrum von selbst heilend, mit warzenartigen Excrescenzen unendlich langsam peripherisch fortschreitet; wir haben den meist im Gesichte, am Rumpfe und den oberen Theilen der Extremitäten vorkommenden gewöhnlichen tuberösen Lupus und den sehr davon verschiedenen sklerotischen Lupus, dann die tuberkulöse Lymphangitis der Haut mit den sogenannten tuberkulösen Gummen und Bubonulis, die zur Tuberkulose der benachbarten Lymphdrüsen führen, sodann

den ganz flachen Lupus oberhalb solcher käsiger Drüsen und deren Narben, und endlich am Schlusse des tuberkulösen Dramas in der vollständig erschöpften Haut die — in der Lunge und dem Darm schon so viel früher auftretenden — ominösen, tuberkulösen Geschwüre an den Eingängen der Körperhöhlen. Ebenso verschiedene Krankheitsbilder schafft der Lichenprozess, je nachdem sich die verschiedenen Bestandtheile der Haut an demselben in wechselndem Grade betheiligen. Bekannt sind auch die Besonderheiten, welche die Localisation am Unterschenkel den verschiedensten Hautkrankheiten ertheilt. Doch genug dieser Beispiele. Sie zeigen, dass wir stets die topographischen, makroskopischen und mikroskopischen Differenzen der normalen Haut im Geiste von dem mannigfaltigen Bilde einer vorliegenden Krankheit gleichsam abzufiltriren haben, wenn wir zu einer exacten Diagnose gelangen wollen. Ganz besonders wichtig ist dieses bei solchen Dermatosen, welche sich einmal in ungewöhnlicher Weise localisirt haben, z. B. bei einem extra genitalen Schanker, einem umschriebenen Lichen- oder Psoriasisfleck der Genitalien, einer Lykosis des behaarten Kopfes, bei spitzen Condylomen des Capillitiums.

Eine andere Art von Mannigfaltigkeit der Hautkrankheiten ist durch die *typische Wandlung der Symptome in ihrem Verlaufe* bedingt. Diese Ursache der Polymorphie hat historisch die meisten Verwirrungen herbeigeführt und ist auch fortwährend eine ergiebige Quelle von Missverständnissen. Die ersten grossen Dermatologen neuerer Zeit Plenck und Willan kämpften mit dieser Schwierigkeit, ohne sie zu überwinden. Ihre Systeme sind Versuche, in der klinischen Form des Exanthems die wahren Krankheitstypen zu fassen, und sie waren — wie wenigstens aus Bateman's Vorrede zu Willan's Handbuch hervorgeht — sich wohl bewusst, dass diese einseitige Hervorhebung der Gestalt der Efflorescenzen zum Zwecke der Classifikation dem wandelbaren Charakter der meisten derselben nicht sonderlich entspricht. Sie glaubten jedoch, aus praktischen Gründen diese Eintheilung, als auf das sinnfälligste Symptom gegründet, der bis dahin waltenden Gesetzlosigkeit vorziehen zu sollen, und darin hatten sie Recht. Allerdings ahnten sie nicht, wie sehr die Folgezeit im Einzelnen ihre "Ordnungen" umordnen und verschieben würde, theils indem sie feinere, anatomische Differenzen aufdeckte, welche eine Zusammenfassung unhaltbar machten, wie bei Lichen pilaris, Lichen urticatus und Lichen tropicus, theils indem sie weit entlegene Krankheitsbilder als besondere Formen einer einzigen erkannte, wie die Impetigo sparsa und Porrigo larvalis als Ekzeme. An diesem wunden Punkte der älteren Systeme setzte der ältere Hebra seinen kritischen Hebel an. Ein wesentlich *klinischer* Scharfblick liess ihn feste Krankheitstypen auch dort erblicken, wo eine ganze Reihe verschiedener Efflorescenzen im Verlaufe des Prozesses zu Tage traten. Der Begriff des Ekzems wurde durch Aufnahme anscheinend heterogener Krankheitsbilder ungemein erweitert, ja so sehr, dass man die ganze Periode seit Hebra nach der Lieblingsdiagnose der Zeit die Periode des Ekzems nennen könnte, wie es vordem eine Periode der Scabies und viel früher eine der Lepra gab.

Mit demselben ausgezeichneten klinischen Instinkte verengerte Hebra den Begriff des Lichen und der Impetigo und machte umgekehrt diese Krankheiten ausschliesslich zu Trägern eines bestimmten, anatomisch definirbaren Efflorescenztypus. So überwand er, ohne der Anatomie und Physiologie der Haut Meister zu sein, intentiv die Hauptschwierigkeiten, welche die Proteusnatur vieler Dermatosen seinen Vorgängern geschaffen hatte, hat uns aber bei neuen und altbekannten Hautkrankheiten genug derselben fortzuschaffen übrig gelassen.

Die zahlreichsten Variationen bringen bekanntlich *äussere Einflüsse* an den Haut-

krankheiten hervor; dieselben kommen naturgemäss auch bei keinem anderen Organe so sehr in Betracht wie bei der äusseren Decke. Während man früher diese immer sehr hoch angeschlagenen, äusseren schädlichen Einflüsse meist als mechanische, seltener als chemische betrachtete, treten nach unserer jetzigen Anschauung diese gewiss sehr wichtigen Momente noch zurück gegen jene, welche durch lebende Parasiten ausgeübt werden. Wenn wir jetzt mit Sicherheit wissen, dass schon die *normale* Oberhaut in ihren verhornten Schichten eine ganze Reihe von Saprophyten constant beherbergt, so ist es nach Allem, was wir sonst von den Bedingungen der Ansiedlung solcher Organismen wissen, eine nothwendige Consequenz, dass die kranke Oberhaut einen noch besseren Tummelplatz der gewöhnlichen Saprophyten und einen im allgemeinen günstigeren Boden für die Ansiedlung wahrer Parasiten abgiebt, besonders wenn die Oberhaut abnorm durchfeuchtet ist und abgestorbene Elemente, Eiterzellen u. dgl. enthält. Wenn schon die Mischinfektionen bei sämmtlichen inneren Organen eine grössere Rolle spielen als man bisher ahnte, und viele Abweichungen vom Typus innerer Krankheiten bei genauerer backteriologischer Untersuchung sich als secundäre bakterielle Invasionen erweisen, so muss man a priori annehmen, dass bei der wie kein Organ sonst exponirten Haut eine secundäre Einwanderung von Parasiten die Regel bildet. In der That bestätigt sich das auch, jemehr wir in dieser Richtung Erfahrungen sammeln. Bei den meisten Infektionskrankheiten z. B., welche die äussere Decke befallen, wie Variola, Varicellen, Morbilli und Scarlatina, haben wir bis in die neueste Zeit *nur* solche secundäre Eindringlinge, nicht aber die Träger des eigentlichen Giftes kennen gelernt. Vielleicht begrüssen wir jetzt erst in den Amoeben van der Loeff's und den Sporozoiden Pfeiffer's den wahren Parasiten der pockenartigen Erkrankungen. Alle bisherigen Kokkenfunde bei den Pocken waren nur geeignet, die eiterige, secundäre Schmelzung der Pocken zu erklären.

Dieselbe Erfahrung werden wir noch sicher bei sehr vielen Hauterkrankungen machen. Es unterliegt ja, z. B., keinem Zweifel, dass bei der Acne vulgaris die Bildung von Comedonen einerseits, die eiterige Entzündung der Follikel andererseits zwei nur äusserlich combinirte, innerlich durchaus fremde Prozesse darstellen, die sich allerdings häufig combiniren, aber ganz unabhängig von einander bestehen können. Wir erleben es oft genug, dass ein ursprünglich ganz rein nervöses Ekzem, dessen zosterähnliche Bläschengruppen auf einer bis dahin gesunden Haut plötzlich aufgeschossen sind, z. B. das Dentitionsekzem auf den Wangen eines Säuglings, nach längerem Bestande zu wandern anfängt und Herpes tonsurans-artig fortkriecht, anstatt seiner Natur nach auf den Bahnen nervösen Reflexes auf entfernte Hautpartien überzuspringen. Es unterliegt dann keinem Zweifel, dass auf dem feuchten Boden des nervösen Ekzems ein parasitäres — sagen wir z. B. seborrhoisches Ekzem — sich eingenistet hat, welches von jetzt ab die Führerschaft übernimmt und im Krankheitsbilde dominirt. Gewiss nicht anders ist auch das Hinzutreten eines Carcinoms zum Lupus zu erklären.

Selbst Vorgänge, welche wir gewöhnt sind, als ganz normale Phasen allbekannter Dermatosen aufzufassen, so z. B. die Eiterung der Zosterbläschen, müssen auf secundäre Infektionen zurückgeführt werden. Wenn man einen beginnenden Zoster sofort mit Iodoformleim oder Aehnlichem bepinselt, entwickeln sich die Bläschen vollständig wie gewöhnlich, bleiben aber klar, die so gut wie normale Eiterung bleibt aus und in dem Bläscheninhalt findet man die Pfeiffer'schen amoebenähnlichen Organismen. Wie sehr muss man nun erst dort, wo die Hautaffektionen uns nicht gerade unter ihrem typischen Bilde entgegentreten, auf solche Mischinfektionen gefasst sein.

Hier liegt die nächste, die dringendste Aufgabe der Zukunft; die bakteriologische Analyse der Hautkrankheiten ist vielleicht eine der schwierigsten aller bakteriologischen Aufgaben, aber sie ist auch eine der interessantesten, und es gehört gerade keine Sehergabe dazu, um aus diesem Studium den Keim zu einer ganz neuen Auffassung der Hautkrankheiten überhaupt sich entwickeln zu sehen. Denn klinische Gründe erfordern es, den parasitären Ursprung einer grösseren Anzahl von Dermatosen anzunehmen, als die älteren Praktiker und Fachgenossen von heute es zuzugestehen geneigt sein werden.

Nach den Mischinfektionen kommen nun aber geradezu zahllose äussere Schädlichkeiten mechanischer, physikalischer und chemischer Art für die Hautkrankheiten in Betracht. Man denke nur an die Einflüsse der Kälte und der Wärme, an die Verschiedenheiten der Exantheme an bedeckten und unbedeckten Körperstellen, an Orten, welche dem kratzenden Finger bequem und welche ihm fern liegen, an die mannigfaltigen, durch den Beruf des Patienten der Krankheit aufgedrückten Besonderheiten, welche sich kaum alle beschreiben lassen. Man erinnere sich der Complikationen, welche einschnürende, reibende und drückende Kleidungsstücke und Zierräthe auf die kranke Haut ausüben und, z. B., unschuldige Ekzeme in mit Oedem, Sugillationen und Blasenbildung einhergehenden Dermatitiden verwandeln können. Man lasse endlich die Unzahl innerer und äusserer Medikamente im Geiste an sich vorüberziehen, welche theils bei günstigem, theils bei unerwünschtem Erfolge der behandelten Haut ein ganz neues Ansehen verleihen können — von der Arnica hinauf bis zum Theer und Chrysarobin. Diese äusseren Einflüsse sind so gross und seit langer Zeit anerkannt, dass schon stets der Grundsatz befolgt wurde, unter solchen, die Diagnose verwirrenden Umständen die Haut unter einem beruhigenden Deckmittel sich auf sich selbst und ihre eigentliche Krankheit besinnen zu lassen.

Diametral gegenüber diesen unzähligen äusseren Einflüssen stehen viel seltenere, aber um so schwieriger zu beurtheilende Einflüsse innerer Art, welche der *Individualität* einzelner Patienten zuzuschreiben sind. Die Psoriasis, der Lupus vulgaris, gehören gewiss zu den am leichtesten zu diagnostirenden Hautkrankheiten, und doch giebt es Individuen, deren Haut auf diese Krankheitsursachen mit so eigenthümlichen Reizphänomenen antwortet, dass der Diagnose und Therapie in diesen Fällen ganz aussergewöhnliche Schwierigkeiten bereitet werden. Das grösste Contingent zu diesen individuellen Varianten liefern natürlich die unter Nerveneinfluss stehenden und entstehenden Erkrankungen: das rein nervöse Ekzem, die Dermatitis herpetiformis (Duhring), die Herpesarten, das Erythema multiforme, die Urticaria. Hier zeigen sich häufig Abweichungen vom Typus, welche die Charaktere der einzelnen Glieder dieser Gruppe unter sich zu verschieben und zu verwischen drohen und die exakte Diagnose erheblich erschweren. Schon die der arbeitenden Classe besonders eigenthümliche, mangelhafte Cirkulation und cyanotische Verfärbung der Hände kann daselbst etablirte Ekzeme so erheblich im Aussehen verändern, dass selbst erfahrene Aerzte in der Diagnose irren. Der Dermatologe muss es sich deshalb bei seltenen Erkrankungen zur Regel machen, die Gesammtconstitution der Haut, vor Allem die Cirkulation, die Funktion der Drüsenapparate und die Beschaffenheit der Oberhaut zu beurtheilen, ehe er an das Studium der erkrankten Hauptpartien geht.

Und doch sind hiermit noch lange nicht die Ursachen der Mannigfaltigkeit der Hautkrankheiten erschöpft. Wir haben noch Nichts gesagt von denjenigen Variationen, welche das Alter und Geschlecht, welche das Land, das Klima, die Jahreszeiten, welche die Rasse mit sich bringt. Die Sonderstellung, welche manche Hauterkrankungen des Kindesalters im Vergleich mit den entsprechenden Erkrankungen der Erwachsenen einnehmen, sind jedem Praktiker geläufig. Auch ist es

jedem amerikanischen Arzte genügsam bekannt, wie grosse Schwierigkeiten die verschiedene Pigmentation der Rassen in die Diagnostik der Hautkrankheiten bringt. Ich fürchte, Sie durch ein näheres Eingehen auf diese Punkte zu ermüden, und will nur noch mit wenigen Worten auf den verschiedenen Genius endemicus der Dermatosen in verschiedenen Ländern, Zonen und Erdtheilen, und auf den wechselnden Genius epidemicus an ein und demselben Orte hinweisen. Der Lichen ruber acuminatus, die Prurigo Hebra's sind hier in Amerika fast unbekannte Krankheiten, ebenso in England und Frankreich. Prurigokranke, vom europäischen Continent nach England und Amerika versetzt, verlieren hier wirklich ohne Arzt ihr Leiden vollständiger als sie es zu Hause in bester Pflege vermögen. In Hamburg trat vor einigen Jahren eine förmliche Epidemie von Lichen ruber acuminatus auf, um fast vollständig wieder zu verschwinden. Jetzt herrscht dort das Exzema seborrhoicum vor allen anderen Ekzemen, ja selbst vor der Psoriasis bedeutend vor. Dagegen kommen hier als Ersatz: der Lichen planus und ein der Prurigo nahestehendes Ekzem häufig vor. Die Sykosis parasitaria breitet sich zugleich mit dem Lupus tonsurans der Kinder langsam von England und Frankreich über Deutschland aus. Vor einigen Jahren war die erstere Form in Wien noch unbekannt. Der Einfluss der Tropen auf die Hauterkrankungen, so besonders auf die durch übermässige Hidrose entstehenden Ausschläge, ist noch lange nicht in erforderlichem Maase studirt. Auch hier werden wir wohl mehrfach alten Bekannten in fremdem Kleide und neuen Benennungen wieder begegnen.

Diese dürftigen Andeutungen mögen doch schon genügen, um zu beweisen, dass für die Haut in ganz besonders hohem Grade das Studium zugleich ein ethnologisches, klimatologisches und geographisches sein muss. Einerseits werden wir durch solche Vergleiche vor den Einseitigkeiten der in verschiedenen Ländern erwachsenen einzelnen Schulen bewahrt, andererseits gewinnen wir erst durch Zusammenfassen der dermatologischen Erfahrungen aller Länder und Zonen ein ausreichendes Bild von der gesammten pathologischen Leistungsfähigkeit der Haut.

In diesen Thatsachen, meine Herren, liegt die Erklärung — wenn Sie wollen, auch die Entschuldigung — dafür, dass die Dermatologie als Wissenschaft noch nicht denselben hohen Stand sicherer Erkenntniss erreicht hat, wie etwa die Augenheilkunde oder die Geburtshilfe. *Der complicirte Bau des Hautorgans* und *seine exponirte Lage*, diese beiden Umstände zusammen sind es, welche die Analyse des Beobachteten bisher so ungemein erschwert haben und auch in Zukunft erschweren werden. Es lassen sich aber noch weitere Folgerungen aus diesen Thatsachen ziehen. Erstens werden wir mit zwingender Nothwendigkeit auf die *minutiöseste Analyse der einzelnen Symptome der Hautkrankheiten hingewiesen*, als erste Vorbedingung zu einem besseren Verständniss der Hautkrankheiten selbst. Diese genaue Verfolgung eines einzelnen Symptoms, z. B. der Schweissfunktion, der Nervenfunktion, mit allen Hilfsmitteln der Physik und Chemie, der Histologie und experimentellen Physiologie, durch alle Regionen der Haut, alle Lebensalter, an der gesunden Haut des Menschen und möglichst vielen Thiergattungen, bei den verschiedensten krankhaften Prozessen — das ist es zunächst, dessen wir bedürfen und bisher so gut wie gänzlich entbehren mussten. Nicht die Beschreibung irgend einer einzelnen neuen, bisher unbekannten Hautkrankheit ist zur Zeit unser wichtigstes Problem, sondern das *eindringendste Verständniss der gemeinsten, alltäglichsten Affektionen*, wie der Ekzeme, der Acne, der Psoriasis. Die strenge Analyse der allergewöhnlichsten Symptome muss und wird uns endlich auf eine feste Basis der Thatsachen führen, die von allen Fachgenossen gleichmässig anerkannt wird und auf der sich dann erst auch eine gleichmässig anerkannte Pathologie der Haut von selbst erheben wird. Dann

sind wir nicht mehr, wie zu Hebra's Zeit, auf den klinischen Instinkt eines einzelnen Genius angewiesen, sondern Hunderte von arbeitsamen Bauleuten werden erfolgreich an demselben soliden Werke schaffen. Dann wird der Autoritätenglaube aufhören und wir werden den übrigen Fachgenossen nicht die Mysterien der Dermatologie zu deuten, sondern die Thatsachen derselben zu beweisen haben. Dann werden die getrennten nationalen Lager und Schulen unter den Dermatologen verschwinden und es wird unmöglich sein, dieselbe Krankheit in Europa als eine Pilzkrankheit, in Amerika oder Frankreich als eine constitutionelle Blutkrankheit anzusehen. Dann erst werden wir auch mit dem Jahrhunderte alten Schutt, dem Aberglauben des Volkes und dem Aberglauben der Mediciner aufräumen können, der trotz der ruhmreichen Anstrengungen der Wiener Schule noch immer wie ein Alp auf der unbefangenen Erkenntniss gerade der Hautkrankheiten lastet.

Aber diese strenge und minutiöse Analyse aller Symptome im weitesten Umfange wird nicht blos — und dieses ist die zweite Folgerung, welche ich zu zeichnen wünsche — der Dermatologie allein zu Gute kommen, sondern der gesammten Pathologie. Gerade die beiden Umstände, die Complicirtheit des Organs und seine äusserliche Lage, welche die Beobachtung auf der einen Seite erschweren, sind es auch wieder, welche auf der andern Seite das wirklich Beobachtete und Feststehende für die ganze Pathologie so wichtig machen. Die Haut wird dereinst der Probirstein für jede pathologische Theorie der Zukunft und weit mehr als heute das Versuchsorgan für die experimentelle Pathologie und Therapie werden. Nur das Auge wetteifert an Klarheit der Beobachtung für den Experimentator mit der Haut und ist für den Thierkörper in vielen Fällen vorzuziehen. Für das Experiment am Menschen sind wir aber fast allein auf die Haut angewiesen, und die wenigen bakteriologischen Versuche, welche in dieser Richtung vorliegen, von Fehleisen, Garré, Bockhart u. A., haben stets zu vollkommen befriedigenden, eindeutigen Resultaten geführt.

Wenn einmal die sämmtlichen Parasiten der Haut und deren Lebensbedingungen genau bekannt sind und wir daraus ein Verständniss gewonnen haben für die so ausserordentlich verschiedenen pathologischen Effekte derselben, wird von diesem kleinen, aber am Menschen selbst bearbeiteten Capitel experimenteller Pathologie und Therapie ein helles Licht auf die viel dunkleren analogen Vorgänge der Krankheiten und Behandlung innerer Organe geworfen werden. Ebenso wird naturgemäss der Kampf für und wider die *trophoneurotische* Theorie bestimmter Störungen, deren sichere und breite Basis erst durch die ausgezeichnete Arbeit der amerikanischen Aerzte Weir-Mitchell, Morehouse und Keen im Secessionskriege geschaffen wurde, auf der Haut zum Austrage gebracht werden. Am unersetzlichsten ist aber die äussere Decke zur Beobachtung der *circulatorischen* Phänomene und speciell für jene rasch auftretenden und ebenso rasch vorübergehenden, welche bislang nur von der Haut bekannt sind, wie die angioneurotischen, z. B. die Urticaria. Es ist durchaus möglich, ja sogar wahrscheinlich, dass es auch in anderen Organen, z. B. im Centralnervensystem, auf den Schleimhäuten analoge Störungen giebt, die, da sie niemals auf dem Leichentische erscheinen, gar keiner anderen Erforschung fähig sind als vermittelst der Analogie der ähnlichen Symptome an der Haut. Die wahre Natur dieser bislang von der experimentellen Forschung stiefmütterlich behandelten Hauterkrankungen hat daher für die allgemeine Pathologie gewiss einen sehr hohen Werth.

Ich glaube, diese Schlüsse sind so selbstverständlich, dass ich den Gedankengang nicht weiter auszuführen brauche. Ist doch die ganze Pathologie in ihren Hauptrichtungen, als Lehre von der Entzündung und Lehre von den Geschwülsten, wesentlich von Symptomen der Haut ausgegangen und bleibt es doch noch heute für

jede neue Definition der Entzündung der schliessliche Probirstein, ob sie die alten vier Cardinalsymptome Tumor, Calor, Rubor und Dolor erklären könne, welche man ursprünglich von der Entzündung der Haut aufstellte, wo man sie eben sah und tastete.

Ich möchte nur noch ganz kurz auch der experimentellen Therapie gedenken, dieser interessantesten Zukunftswissenschaft, deren Resultate allmählich diejenigen der empirischen Therapie zu ergänzen, zu ersetzen oder zu bestätigen bestimmt sind. Zahllose Thierversuche gelten heute als Erforderniss einer mit gutem Gewissen abzugebenden Empfehlung eines neuen, von der Chemie gelieferten Heilmittels. Und doch ist nur sehr selten ein bindender Schluss vom toxikologischen Thierexperiment auf die therapeutische Wirkung am Menschen zu machen. Aber das Thierexperiment ist auch gar nicht der einzige solide, ungefährliche Weg, um zu neuen therapeutischen Maasnahmen zu gelangen. Die Geschichte lehrt uns einen ganz anderen Weg kennen, nämlich den von der dreisten äusseren zur vorsichtigen inneren Anwendung. So fürchtete man im Alterthum die innerliche Darreichung des Quecksilbers. Dioscorides warnt vor ihr ausdrücklich. Als aber am Ende des fünfzehnten Jahrhunderts mit dem Auftreten der Syphilis die bis dahin nur sporadisch geübte Quecksilberschmierkur im Volke populär und von Laienhand mehr als von Aerzten mit diesem Heilmittel ein verschwenderischer Gebrauch getrieben wurde, da tauchten auch alsbald vereinzelte Anwendungsformen zu innerlichem Gebrauche auf. So waren im Anfang des sechszehnten Jahrhunderts die Quecksilber- und Terpentinhaltigen Pillen des Seeräubers Hayreddin beliebt. Gegen Ende des sechszehnten Jahrhunderts wurden zugleich mit der nunmehr von Aerzten geregelten Schmierkur bereits die verschiedensten Quecksilberpräparate innerlich verordnet. Auch die Balsamica haben gewiss diesen Uebergang von der äusseren zur inneren Anwendung, wenn auch schon im Alterthume, erlebt.

Nun, dieser Weg steht uns auch jetzt noch offen.

Lassen Sie mich, um diesen Satz zu illustriren, ein einziges Beispiel aus meiner eigenen Erfahrung wählen.

Ich experimentirte mit einem neuen Schwefelmittel, welches von Laien den Namen Ichthyol erhalten hat, bei den verschiedensten Hautaffektionen und fand u. A., dass es bei bestimmten Erkrankungen nervöser Natur, z. B. bei gewissen nervösen Ekzemen, in äusserlicher Anwendung einen constanten, überraschend günstigen Effekt übt. Ich fand aber auch gleichzeitig, dass bei fortgesetztem äusseren Gebrauch dieser gute Effekt sich fast mit Nothwendigkeit in sein Gegentheil verkehrt, und kam daher auf die Idee, es innerlich bei diesen Affektionen zu geben. Hier fand ich nun die Wirkung durchaus constant und befriedigend, es zeigte sich, dass das Anwendungsgebiet des Ichthyols in Bezug auf nervöse Hauterkrankungen nur zum Theil mit dem des Arseniks zusammen-, zum grossen Theil auseinanderfällt. Bei weiteren Versuchen fiel es nun mir wie den Patienten auf, dass gewisse Complikationen dieser Ekzeme, so besonders wahres, nervöses Bronchialasthma, ebenfalls unter dem Ichthyolgebrauche verschwanden. Was war natürlicher, als in der Folge auch andere Fälle von Bronchialasthma, die nicht mit Ekzem combinirt waren, in gedachter Weise zu behandeln? Die guten Erfolge rechtfertigten diese Vermuthung, und ich betrachte heute das Ichthyol als eines unserer besten Mittel gegen Bronchialasthma überhaupt und stelle es direkt neben und hinter das Iodkalium. Auf ähnliche Weise habe ich, von der Haut ausgehend, die bereits vielen Aerzten bekannten guten Eigenschaften dieses Mittels bei chronischen Magen- und Darmkatarrhen und bei Chlorose gefunden.

Es giebt also auch einen unschuldigen, sicheren Weg des therapeutischen Experi-

mentes, der nicht durch den Thierkörper, sondern über die menschliche Haut führt, und da diese bei verständiger Behandlung sehr geduldig ist, so sehe ich eine grosse Reihe von auf diesem Wege gefundenen Bereicherungen der Therapie voraus.

Nicht weniger segensreiche Einwirkung erwarte ich von einer fortgeschrittenen Dermatologie der Zukunft für die *Chirurgie*. Unsere Specialität liegt ja eigentlich so recht in der Mitte zwischen der inneren Medicin und Chirurgie und überbrückt die weite Kluft beider, indem sie mit der inneren Medicin mehr die Art der therapeutischen Einwirkung, mit der Chirurgie mehr die Art der unmittelbaren Diagnostik theilt. Wenn wir Dermatologen von der neueren Chirurgie den Segen der antiseptischen Methode im engeren Sinne und die Bedeutung der alle Schädlichkeiten abhaltenden passiven Methode im Allgemeinen gelernt haben, so wird die Zeit kommen, wo die Chirurgie sich wieder nach activerem Einschreiten in dem Wundheilungsprozess sehnt, und dann wird sie sich mit den Resultaten der neueren Dermatologie zu befreunden haben, welche unablässig die direkte Einwirkung chemischer und physikalischer Agentien auf die Gewebe studirt. Die Iodoformdebatte der neuesten Zeit hat gezeigt, dass ein nicht unbedeutender Theil der Chirurgen noch die Begriffe des antiseptischen Mittels im strengen Sinne und des Wundheilmittels im Allgemeinen identificirt, während nach unserer Meinung die Antiseptica nur einen, allerdings sehr wichtigen, Theil der Wundheilmittel ausmachen. Glücklicherweise ist das Iodoform dieses Mal noch gerettet, aber wenn sich auch alle Chirurgen aus Angst von ihm abgewendet hätten, weil das Iodoform die Culturen des Staphylococcus aureus nicht vernichtet, wir Dermatologen hätten nach wie vor unsere weichen Geschwüre mit demselben entgiftet und nicht aufgehört, es hochzuhalten, bis bei den Chirurgen der richtige Begriff für den Segen des Iodoforms und die unantastbare Formel für seine Verehrung gefunden wäre.

Auch wir Dermatologen haben so gut wie der innere Mediciner und der Chirurg mit unserer Form der Tuberkulose zu kämpfen — und das ist der Lupus. Vielleicht mag es für die Chirurgen ein werthvoller Fingerzeig sein, dass sich vor allen anderen örtlichen Antituberculosis, vor dem Iodoform, dem Pyrogallol, dem Ichthyol, dem Höllenstein, neuerdings die *Salicylsäure* bewährt hat, in reiner Form als Pflastermull oder als hoch procentuirte Salbe, besonders aber die schmerzlosere Mischung von Salicylsäure und Creosot. Für die Tuberculose der Lymphgefässe und Lymphdrüsen ist dieselbe ein ebenso vorzügliches Mittel, ob für die der Knochen und Gelenke, muss nun weitere, chirurgische Erfahrung lehren.

Ich hoffe, meine Herren, dass Sie, den hier von mir angeregten Gedankengang fortdenkend, mit mir zur übereinstimmenden Ansicht gelangen werden, dass die Förderung der Dermatologie als Specialwissenschaft im Interesse der Gesammtmedicin, im Interesse des Naturforschers ebenso wie des praktischen Arztes liegt. Eine solche Förderung sehe ich schon in dem Zuwachs junger praktischer Aerzte, welche sich mit idealer Begeisterung diesem Fache widmen — und glücklicherweise fehlt es heutzutage an solchen nicht. Aber eine wesentlichere Förderung sollte vom Staate und von den Universitäten ausgehen. In Frankreich und England ruht noch die ganze Last wissenschaftlicher Forschung in diesem Zweige auf den Schultern von Medicinern und Chirurgen, welche lediglich aus Liebe zu diesem Fache sich seiner speciellen Förderung gewidmet haben. Durch besondere Lehrinstitute ist dagegen gesorgt in steigendem Umfange in Russland, Deutschland, Italien und Nordamerika. Nirgends sitzen so viele berufene Gelehrte in unserem Fache bereits auf Lehrstühlen als hier in den Vereinigten Staaten — und doch reichen diese Mittel zu einer ergiebigen Förderung unserer Specialität noch nicht aus. Die Auf-

gaben sind eben zu mannigfaltig, zu verwirrend, zu schwierig für den Einzelnen, der Umfang der zu beherrschenden Vorwissenschaften ist zu ausgedehnt. Ganz andere Fortschritte würden hier erreicht werden durch eine Association von gleichstrebenden Forschern, durch eine Condensation der wissenschaftlichen Mittel und Methoden, wie sie nur an einem wissenschaftlichen Centralinstitute gefunden werden können. Nord-Amerika hat, wie kein anderes Land, Bürger hervorgebracht, welche sich durch grossartige, für europäische Verhältnisse geradezu unglaublich reiche, wissenschaftliche Stiftungen verdient gemacht haben. Die erste und älteste Universität dieses Landes wurde ja, wie viele spätere, von einem einzelnen Manne gegründet. Vielleicht ehrt auch ein einzelner amerikanischer Bürger sich und sein Land durch Gründung eines Instituts, welches die Dermatologie zum Range eines der ersten medicinischen Fächer erhebt und durch sie der Gesammtmedicin eine reiche Förderung in sichere Aussicht stellt. (Applause.)

At the conclusion of Professor Unna's address, SIR JAMES A. GRANT, of Ottawa, offered a motion of thanks. He said :—

MR. PRESIDENT: It affords me great pleasure to propose a hearty vote of thanks to Dr. Unna, of Hamburg, for his excellent address on "The Relations of Dermatology to General Medicine." The clear and comprehensive manner in which the subject has been dealt with is in keeping with the previous able productions of the same author. The close and intimate relationship of skin diseases to the general system, which feeds and supports the abnormal cutaneous outcrops, is all-important. It is very gratifying to have in our midst two such German lights in the profession as Martin, of Berlin, and Unna, of Hamburg. The progress of intellectual development in the various departments of medical and surgical science has given to Germany a world-wide reputation. To-day, it is the great clinical centre to which our young medical men are attracted, and certainly this Congress proves that the influence of its teaching is felt in this great American Republic. Their clear, concise and clinical methods cannot fail to be productive of great and lasting benefit to the medical profession. It is the varied and combined influences now at work, supported on a scientific basis, which have stamped success on the present meeting of the Congress. When its proceedings are published, the record of facts will demonstrate in an undoubted manner the zeal with which America and the outside world in the profession have coöperated in a common cause, that of the promotion of medical science as a whole. Under these circumstances I have great pleasure in proposing a vote of thanks to Professor Unna for the exceedingly instructive address to which we have listened with such pride and gratification.

The motion was seconded, and the question being taken was unanimously carried. The general session then adjourned.

In the evening the members of the Congress were entertained at a banquet at the Pension Hall.

FIFTH DAY.

The Congress met in general session at 10 A. M. The following resolutions were read from various Sections. From the Section on Public and International Hygiene :—

WHEREAS, The whole community has been repeatedly shocked by the almost daily occurrence of terrible accidents on many of the railroads, causing considerable loss of life, and as well by habitual neglect of the most elementary sanitary laws ;

WHEREAS, This Section considers itself, in a degree, a guardian of public health ;

Be it Resolved, That the attention of this Ninth International Medical Congress be respectfully called to this most important question, and it be requested to use its influence to obtain the necessary reforms.

The question being taken on the adoption of this resolution, it was adopted amid great applause.

The following was referred to the "Transactions" without a vote.

SECTION XV, FIFTH DAY, September 9th.

The following resolution was offered in this Section by Dr. BENJAMIN LEE, of Philadelphia, and adopted :—

Resolved, That this Section cordially endorses the suggestions contained in the paper of Dr. Cook, of Nashville, Tenn., on the "Necessity for teaching Hygiene in Schools," and recommends to the Congress the passage of the following resolutions :—

Resolved, That it is the sense of the Ninth International Medical Congress—

1. That every medical college should place the chair of Hygiene on its curriculum, and on an equal footing with the other regular branches of instruction.

2. That in all universities, colleges and high schools, Hygiene should form a part of the compulsory course of study, and should be taught not simply through text-books, but by educated physicians.

3. That in all public schools the teaching of Hygiene should form a prominent and essential feature ; and

4. That every State legislature should establish a museum and laboratory of Hygiene.

The following was also read and referred to the "Transactions :"—

SECTION XV, SECTION OF INTERNATIONAL HYGIENE,
THIRD DAY, September 7th.

After the reading by Dr. DOMINGOS FREIRE, of Rio Janeiro, representative in the Congress of the Brazilian Government, of a paper entitled "Vaccination with the Attenuated Culture of the Microbe of Yellow Fever," with demonstration of the microbe under the microscope, the following preamble and resolutions were adopted by the Section :—

WHEREAS, inoculation against yellow fever, if it proves successful after further examination, is calculated to benefit the human race throughout the world ; and

WHEREAS, The facts presented by the experiments of Dr. Domingos Freire afford a reasonable assurance of its protective influence in Rio Janeiro, therefore

Resolved, That this Section recommends the coöperative investigation of the *results obtained* by yellow fever inoculations as a protection against that disease, and that adequate appropriations by the Governments represented in this Congress be made for that purpose.

Resolved, That this action be communicated forthwith for consideration in the general session of the Congress.

JOSEPH JONES, President Section XV.
WALTER WYMAN, Secretary Section XV.

The following resolutions were also read by request of the Section on Medical Climatology and Demography, which resolutions had also received the endorsement of the Section on Public and International Hygiene, and they were referred to the "Transactions" to be printed :—

Resolved, That in the opinion of the Section on Medical Climatology and Demography of the Ninth International Medical Congress, assembled in the city of Washington, September 5–10, 1887, it is important there should be established in every country a national department, bureau or commission for the record of vital statistics upon a uniform basis, to include not only accurate returns of births and deaths, but the results of collective investigation by government officials of facts bearing upon the natural history of disease as manifested among men, women and children separately, especially with regard to climatic and other discoverable causes of the several forms of disease, race, residence and occupation being also made matters of record, in order that necessary preventive measures may be determined and enforced for the preservation of the public health.

Resolved, That the Secretary-general be requested to have this expression of opinion communicated to the several governments.

The following resolutions were reported from the Section on Military and Naval Medicine and Surgery, and respectively referred to the "Transactions."

Resolved, That this Section recommends the Ninth International Medical Congress to take such action as will direct the attention of all nations to the importance of preventing, by International law, the employment of explosive bullets in warfare.

Resolutions accompanying the paper of Joseph R. Smith, A.M., M.D., U.S.A., and adopted by the Section on Military and Naval Surgery and Medicine.

1. The main object of a "Report of Sick and Wounded" for an army is to give the diseases and injuries, their number and their proportions, occurring among the troops, and the results thereof; all other objects being incidental and secondary, or better effected in certain cases by "Special Reports."

2. For purposes of convenience this Report should be monthly.

3. It is desirable that, for comparison and study, the form of this Report should be uniform for all armies and as simple as may be.

4. No nosological arrangement can now be made which commends itself to every one, and which all will agree on as the only one compatible with scientific knowledge, and therefore that nomenclature and arrangement of diseases should be adopted in this Report which is most convenient for use in the exigencies of military service.

5. In view of the great desirability of a uniform "Report of the Sick and Wounded" by all armies, and in the absence of any other form better fitted to fulfill the ends for which this Report is made, this Section recommends the adoption of the subjoined form in the Medical Department of all armies.

MONTHLY REPORT OF SICK AND WOUNDED.

This Report will be sent to the Medical Director, and a copy to the Surgeon-general direct, by the fifth of each month. Separate Reports are to be made for white and colored troops.

STATION..... MONTH ..... 18 .

MEAN STRENGTH OF THE COMMAND: OFFICERS .....ENLISTED MEN .....TOTAL STRENGTH .....

	TABULAR LIST OF DISEASES. The names of diseases not printed below, and the number of each occurring during the month, will be *written* under the class to which they belong. Each day that a soldier fails to perform duty on account of disease or injury, is a "day's service lost." The addition of the number of days thus lost by all sick with the same disease makes the sum to be entered in the last column.	REMAINING UNDER TREATMENT FROM LAST MONTH.	TAKEN SICK OR WOUNDED DURING THE MONTH.	TOTAL TO BE ACCOUNTED FOR.	RETURNED TO DUTY.	TRANSFERRED TO ANOTHER HOSPITAL OR COMMAND.	DISCHARGED FOR DISABILITY.	DESERTED WHILE UNDER TREATMENT.	DIED.	REMAINING UNDER TREATMENT.	NUMBER OF DAYS' SERVICE LOST.
1st Div.	GENERAL DISEASES (POISONING). YELLOW FEVER. SMALLPOX. ETC.										
2d Div.	DISEASES OF THE NERVOUS SYSTEM. EPILEPSY. INFLAMMATION OF THE BRAIN. ETC.										
3d Div.	DISEASES OF RESPIRATORY ORGANS. ACUTE BRONCHITIS. INFLAMMATION OF LUNGS.										

4th Div.	DISEASES OF CIRCULATORY ORGANS. Hypertrophy of Heart. Aneurism. Etc.										
5th Div.	ORGANS OF ASSIMILATION. Dyspepsia. Etc.										
6th Div.	URINARY AND SEXUAL ORGANS (EXCLUDING VENEREAL). Retention of Urine. Etc.										
7th Div.	VENEREAL DISEASES. Primary Syphilis. Etc.										
8th Div.	DISEASES OF THE EYE. Cataract. Etc.										
9th Div.	DISEASES OF THE EAR. Otorrhœa. Etc.										
10th Div.	INTEGUMENTARY DISEASES. Boil. Etc.										
11th Div.	DISEASES OF ORGANS OF LOCOMOTION. Anchylosis. Etc.										
12th Div.	MECHANICAL INJURIES. Gunshot Wounds. Etc.										
13th Div.	Other Diseases, including, in the order here named, Self-mutilation, Attempted Suicide, Simulation, General Debility of Body, Debility from Old Age.										
14th Div.	Under Observation.										
	TOTAL.										

Vaccinations during the month.	Number of cases.	[1. Officers and soldiers excused from duty because of vaccination will be entered on Register with vaccina 2. Note kind of virus used and general character of lesions produced thereby. Remarks.]
Vaccinated successfully.		
Vaccinated unsuccessfully.		
Undetermined.		
Revaccinated successfully.		
Revaccinated unsuccessfully.		
Undetermined.		

Discharges on Surgeon's Certificate, and Deaths, for the month of....., 18

NOTE.—Discharges on surgeon's certificate, and deaths occurring among those not on Sick Report, will also be reported, but separated from the others by a double line drawn across the page. The remarks will, in each case, specify the manner in which the disease originated, when it is known.

In every case of the death of an officer, whether on duty or not, a special report is to be made to the Surgeon-general.

In every case of discharge or death, it should be stated whether the disease originated in the line of duty.

NO.	NAME.		RANK.	REGIMENT.	CO.	DISEASE OR WOUND.	DATE OF DISCHARGE FROM SERVICE.	DATE OF DEATH.	ORIGINATED IN THE LINE OF DUTY.
	SURNAME.	CHRISTIAN NAME.							

REMARKS.

DIRECTIONS.—Here make any necessary explanations, and communicate any matters of interest with regard to prevailing diseases or the sanitary condition of the troops. Interesting cases and autopsies should be communicated in full, in an accompanying letter. In case a hospital is opened or closed during the month, it should be stated by whose order, and on what day. When the command is moving, the station on the first and last of the month and the route should be given.

I certify that the above Report is correct and true, to the best of my knowledge and belief.

.....*Surgeon* .....

(To be signed by the medical officer in charge, with the name and rank in full.)

MONTHLY REPORT

OF

SICK AND WOUNDED.

Station: .....

Month:....., *18* .

FORWARDED BY

.....

.....*Surgeon*.....

(Here give legibly the name and rank.)

COMMAND.

(Here specify legibly the name of the regiment and the letters of the companies comprising the command, with the brigade, division, corps, and army or department in which it is serving.)

DIRECTIONS.

I. In filling the first pages of this Report, enter, opposite the name of each disease, the number of cases remaining, taken sick, etc., using ordinary numerals.

II. Patients sent on furlough while under treatment will be considered as still "Remaining under treatment," and when they return will not be reported as new cases. Should a patient die or desert while on furlough, the fact will be reported in the columns of "Died" or "Deserted."

III. No arrangement is made on this blank for reporting secondary diseases or complications. Should these be in any case interesting, the facts are to be stated on the fourth page, under the head of "Remarks."

IV. When a soldier, reported during one month as "Taken sick" of any given disease and, still on sick report, dies during a subsequent month of another entirely distinct disease, the death will be entered in the proper column, opposite the name of the disease which was the cause of death, but no new case will be entered to correspond. In all such instances the facts are to be set forth in the "Remarks," and the necessary statistical corrections will be made in the Central Office.

V. Separate reports will, in all cases, be made for white and colored troops. In the case of colored troops, the first page of this Report will present only the diseases of colored men. The name and rank of sick white officers of colored regiments, with disease and its termination, will be placed on the fourth page of the report of colored troops, under the head of "Remarks."

VI. The "Mean strength of the command" is to be ascertained as follows: The total strength present on each day of the month, as obtained from the commanding officer, are to be added together and divided by the number of days. This is to be done for officers and enlisted men separately; the nearest whole numbers are to be used, and not decimals or fractions. No mean strength is to be reported in the case of general hospitals, or of such post hospitals as regularly receive their sick from the several commands, the medical officers of which, of course, make monthly sick reports. In all other cases, when patients are transferred, the name, rank, company, regiment, and disease or injury of each, with the date and place of transfer, will be entered in the "Remarks."

VII. When the command represented by the Report is moving, the station on the first and last of the month, and the route, should be given in the "Remarks."

WASHINGTON, D. C., Sept. 8th, 1887.

THE SECRETARY-GENERAL OF THE NINTH INTERNATIONAL MEDICAL CONGRESS.

I have the honor to report that Section III resolved to forward, for the consideration of the Congress in general session, the following proposition from a paper by Dr. Voorhees.

As the European powers initiated the movement which brought about the convention of St. Petersburg, it is now the duty of the United States Government to take the initiative and request the powers to join in a second congress for the purpose of abolishing by international laws such projectiles as may be deemed barbarous and unnecessary in war.

Respectfully submitted,

JOHN F. GASTON, M.D., *Secretary.*

WASHINGTON, D. C., Sept. 8th, 1887.

THE SECRETARY-GENERAL OF THE NINTH INTERNATIONAL MEDICAL CONGRESS.

I have the honor to inform you that a resolution was passed by the Section of Military and Naval Surgery and Medicine, authorizing the following conclusions of Dr. Lamb's paper to be forwarded for the consideration of the Congress in general session:

1st. A prisoner of war is not *ipso facto* a criminal, to be put to death or harshly treated. He is human, and should be so treated as to preserve his life and health.

2d. If captured while sick, or taken sick after capture, he should receive such treatment, both hygienic and medical, as will tend to promote recovery.

3d. If his captors are unable to afford the proper prophylaxis or treatment, he should be paroled and returned to his own government, which also should be bound to receive him.

4th. There should be international regulations specifying the minimum allowances of food, clothing, shelter, fuel, etc., of prisoners of war.

5th. The best ends of economy and humanity by the paroling of all prisoners of war as soon after capture as practicable, on condition that they do not bear arms until exchanged.

All of which is respectfully submitted.

JOHN F. GASTON, M.D., *Secretary.*

The SECRETARY-GENERAL then announced that the report of the Committee to select the place of meeting for the Tenth International Medical Congress had been received. The Committee had organized by the election of Dr. Semmola as President, and Dr. Assaky as Secretary, and after discussion, had agreed to recommend Berlin as the next place of meeting, in 1890. The announcement was received with enthusiasm, and the President put the question on the adoption of the report. The report was unanimously adopted.

The PRESIDENT then announced as the next business in order the reading of the address of Dr. Blandford, of London, and Vice-president PHILLIPS, of London, was called to the chair.

Dr. BLANDFORD began by saying that it was due to himself to state that his address had been prepared for delivery before one of the Sections, and that he might have changed it somewhat had he known the Committee would have requested him to read it as a general address.

THE TREATMENT OF RECENT CASES OF INSANITY IN PRIVATE AND IN ASYLUMS.

BY G. FIELDING BLANDFORD, M.D., F.R.C.P., LOND.

Happy is the psychologist who is not concerned with his patients till he receives them within the walls of his asylum. Many of the physicians of our large public institutions are thus fortunate. They do not see or hear of those they are about to treat till they are admitted. They have not to advise removal, to write certificates, to plan means of conveyance. They receive them only in hospitals fitted with the best contrivances for the care and cure of the insane of every kind, and can apply themselves without drawback or hindrance to the task.

Very different is the lot of a consultant summoned to see a patient who has become insane at home, or to chase him from place to place, and examine him, perhaps, in an assumed character, in order to advise a plan of treatment. Although anxiously consulted by the friends, his advice is as often opposed as adopted. Every conceivable objection is urged against his proposals, and he is supposed to carry about with him some potent elixir which, without any restraint, will speedily disperse the cloud and bring back the sunlight of reason.

The object of this paper is not to discuss the treatment of the chronic or incurable insane, but that of recent cases only which are, for the most part, somewhat acute, not admitting of question as regards the insanity, but in which prognosis rather than diagnosis will play the most important part. We have to consider, when called to an acute case of mania, whether it is likely to be of brief or of lengthy duration, and where and how we can deal with it.

Recent insanity is a very curable disorder, as statistics from all quarters prove, and it behooves us to place the sufferer under such conditions that the cure may be effected as surely and speedily as possible. To the majority of cases a well-ordered asylum affords the best means of treatment; for many it is a necessity, and removal ought to be urged in the strongest terms. But from time to time patients come before us whom, for various reasons, we wish to save from the stigma which, rightly or wrongly, is certainly attached to one who has been an inmate of an asylum. This point is sure to be strongly brought up by the friends, and it is one not lightly to be put aside. Many fathers of families may be seriously damaged in position or prospects, or may even lose the post they hold. Some may be lads or young girls at the outset of life, others young mothers in their first confinement. The reputation of having been in an asylum will never be lost by any one of these. The public attaches little importance to a person being sent away ill to the country or the seaside. If sent to an asylum, it is quite another matter. In a practice of some thirty years, I have seen many recover from a brief though acute attack of insanity, either at home or in a lodging, or house suitable for treatment. My time does not allow me to enumerate even a fraction of such cases in anything like detail, but I wish to state very briefly some points which may help us to determine the prognosis, and afford a hope that the malady will soon pass away. That this passing mania is recognized by psychologists is certain. Its name, *mania transitoria* is adopted by many, and well expresses the fleeting nature of the attack.

The chief point of prognostic value is the suddenness of the attack. It may come on with scarcely any warning, in the course of a few hours, and in that brief time may develop symptoms of a very violent and acute character. There may or may not have been premonitory symptoms. This will depend very much on the cause, for there generally is a cause—an exciting cause—lighting up the disorder in a brain

prone to "instability" or "explosiveness." The illness comes on suddenly; we may hope that it will, if not as suddenly, yet, at any rate, in a short time pass away. It frequently does, but not always. I have known a young lady pass into chronic and incurable mania of the worst kind, whose malady commenced in the most sudden way and who was at once placed in an asylum. It was not, therefore, for want of skilled treatment that she did not recover.

Some guidance may be derived from the cause of the attack, if we are so fortunate as to be able to put our finger on it. If the cause is recent, not spread over a long period, but definite and rapidly producing its result, it is to be hoped that by removal of it the effect may in a brief time subside. Such a cause may be a sudden mental shock or fright; the loss of a relative or a surgical operation. Here the suddenness of the event upsets the unstable equilibrium of the individual; and although the cause may not be removable, as some are, yet the equilibrium is regained by the mind becoming by degrees able to contemplate the event, or being distracted therefrom by change of scene or surroundings. Another potent cause is religious excitement. Probably most of my hearers have seen women—aye, and men too—attacked by hysterical mania after attending revival meetings and exciting religious services of all kinds. We may hope that in such patients the symptoms will quickly abate. I have seen several women, not young, but elderly, who have had sharp attacks of mania after a hot course of spiritualism, attendance at spiritualistic seances, and the like. Two I have encountered lately. They were as violent cases of mania as one often meets with, but both got well in the course of two or three weeks without even being removed from their own houses.

Another cause of sudden maniacal attacks which I have met with several times, is protracted fatigue, an unduly long walk or exposure to a hot sun for a long period. This was, in my opinion, the cause in the case of an officer who had been attending the autumn manœuvres in Germany, and had, for want of a horse, walked many miles in the sun on a hot day. Traveling back day and night to his command, he thoroughly knocked himself up, and developed an irritability which culminated in a violent and public quarrel with his superior officer, with outrageous conduct, exaltation of ideas, great personal conceit, and unmeasured and at times obscene talk. I was summoned from London and asked to bring certificate papers for his removal to an asylum. By the time I got there he was in restraint, having been taken in charge by the authorities as a dangerous lunatic. On hearing a little about his previous history, I anticipated that the attack would not last long, so, as we had him in safe keeping, we carried him to the railroad and took him bodily to his own house in London, whence in a fortnight he went convalescent to the seaside. A very long walk produced an attack of acute mania in a lady of a very nervous temperament, who had had a similar attack on a previous occasion. This entirely subsided in a few days.

Very acute maniacal symptoms occasionally arise in the course or toward the decline of an acute disease, such as measles, scarlatina, erysipelas, pneumonia and typhoid fever, and my friend Dr. Hermann Weber has, in the 48th volume of the Medico-Chirurgical *Transactions*, published particulars of seven cases of this form, to which he has given the name of the "delirium of collapse." He remarks that the delirium commenced almost always immediately on waking from sleep, and at a time when the disorder is declining, not when the temperature is highest and the fever symptoms most prominent. In his experience the mania lasted for a short time, from eight to forty-eight hours, and all recovered. My experience has not been quite so fortunate. I have seen some who have not recovered so quickly, but still

the majority do, and we may give a favorable prognosis. The occurrence of such mania just as an illness is declining causes the greatest consternation, and the ordinary sick-nurse is probably very unfitted to cope with it, but removal from home should not be thought of till sufficient time has elapsed to show whether the mental symptoms will be brief or prolonged. And here I may mention those curious cases of mania which occur in connection with gout or rheumatism. Sudden maniacal symptoms may present themselves in a man or woman and the case assume an appearance of the greatest gravity, and all these may as suddenly disappear on the supervention of an attack of acute rheumatism or gout in some of the joints. [Case: woman; acute mania with refusal of food; nurse sent and returned in two days.] It is always worth while in patients with such a history to try and develop an attack of gout by applications of mustard to the feet, and the administration rather than the withdrawal of a liberal diet.

Brief but very violent attacks of mania may follow epileptic seizures. Here we shall have the history of the case to guide us in the majority of instances, but it sometimes happens that no one has seen the epileptic fit or knows the patient's previous history. It lately happened to me to have a patient sent to my asylum who for a few days was one of the most troublesome that I ever encountered. He then rapidly recovered, and his whole term of residence was only a fortnight. It was impossible to keep this man in his own home, and his means being small, no temporary abode with requisite attendants could be provided for him.

We are all familiar with the acute alcoholic delirium to which is given the name of delirium tremens. It is not uncommon and is easily recognized; it is a transitory mania and generally terminates within a week in recovery or death; but besides this we occasionally see symptoms of insanity produced by drink, delusions and hallucinations very like those of ordinary insanity, but which pass away quickly if the drinking has not been prolonged. Two cases have come before me lately and one last year. One of the two was a young lady between 20 and 30 years, who suddenly showed alarming symptoms, which subsided after two good nights' sleep, produced by chloral and bromide. The second was an elderly lady whose hallucinations of hearing were said to have been produced by chlorodyne. I suspect, however, that she mixed it with a good drop of brandy. The lady last year was so bad that I thought she would have to go to an asylum. She came to me from the country, and her relatives at first concealed the fact of her having drank, but she suddenly got well in about a fortnight and the secret came out.

We shall derive great aid in our prognosis, if we are able to ascertain that the patient has had previously similar acute attacks which have passed off in a short time. These sudden violent accessions of mania are just what we should expect to find in people of such unstable nature that they are ready to explode at the least thing which upsets them, just as we see so many who are driven into furious gusts of passion by mere trifles, and for a time are hardly responsible persons. I know several who habitually have such accessions of mania. One, a gentleman of great intellectual power, but nervous to a degree, has had three within my knowledge. On one occasion he was abroad, and was speedily placed in an asylum, which converted a brief attack into one more prolonged. The last time I saw him he was not violent, but in a kind of cataleptic seizure, unable to speak or take food. I warned his medical attendant that it would pass away, and it did entirely in forty-eight hours or less. The officer of whom I spoke had had something of the kind before, which prompted me to keep him at home and not send him to an asylum. Of course, after repeated attacks, recovery may not, probably will not, be so speedy as after the first or second,

yet we may reasonably expect it to occur without any removal or very special treatment.

In two highly nervous individuals the attacks have been more melancholic than maniacal, being of the nature of intense panic with delusions of fear—fear that every one about was conspiring to inflict some injury or traduce and take away their character. Two cases have lately come under my notice, and both terminated almost suddenly, but owing to different causes. Each tried to escape from his imaginary persecutors—the one by running away, the other by suicide. The former succeeded, and got away to friends on the continent. Feeling himself safe in the hands of his friends, his delusions and fears all vanished, and he returned after a short time to his house and business. The other gentleman took poison, but only succeeded in making himself horribly sick, which cured him. In consequence of the suicidal attempt, the authorities packed him off to the nearest asylum. He was well, however, before he started, and wrote me a letter telling me all about it. I saw him the next day and he appeared absolutely recovered and sane.

What is there in the physical condition of the patient to aid our prognosis and determine the brevity or length of the attack? The temperature will not give us much assistance. Neither in transitory nor prolonged mania do we find any great rise in temperature. The pulse will tell us rather more. It may be quick when great excitement, violence, shouting or singing are going on, and when the paroxysm is over—and all this noisy excitement occurs in paroxysms—it may fall very considerably, and be not much more rapid than the normal. If it does not so fall, but, on the contrary, remains rapid even when the patient is comparatively quiet, the chances are the mania will be a prolonged attack, and we shall have to treat it not for days but for weeks. The tongue, too, will give us some help. In very violent attacks it often happens that this will remain moist and clean, presenting no abnormal appearance. On the other hand, if it becomes dry, more and more furred, and brown with that coat which is so indicative, not of stomach derangement, as we so often hear, but of acute nervous disorder, then we can hardly hope that it will pass away in two or three days. There is often a great deal in one of these maniacal attacks which, for want of a better name, we may call hysterical. As a rule, they are characterized by noise and violence rather than by fixed delusions. They begin suddenly, or after a very short prelude, and delusions have not had time to take definite shape. Now, in hysterical attacks, between the intervals of violence, there is often a good deal of consciousness, and attention directed to what is going on. If this increases, we may hope for a speedy return of rationality; if it diminishes, if incoherence and inability to converse increase, and if delusions are manifest, then the prognosis of a speedy recovery will be unfavorable. This hysterical state and these hysterical symptoms may last some time, not varying very much, and without any increase of the graver symptoms. We feel certain that they will pass away because there is so much reasoning power at one time, though with great excitement and violence at another. We may have reason to suspect that a great deal of the latter is put on, and is to be met by moral control.

Treatment will often aid us in the prognosis of these cases. Sleeplessness is a main feature in many of them, and it not unfrequently happens that, after a long sleep produced by a narcotic, the patient wakes recovered, or so nearly so that we are no longer anxious about him, precisely as we see men recover in delirium tremens. Now, for the procuring of sleep, what a number of medicines we have at hand. When I began practice there was nothing but opium and its compounds, and to administer opium or morphia to a patient in this stage would almost certainly have

the effect not of producing but of preventing sleep. But now there are the bromides, urethan, paraldehyde, chloral hydrate, hyocine, hyocyamine, and perhaps others with which you are acquainted. If a reasonable dose of one of these produces a good and refreshing sleep and the sufferer wakes with his mental symptoms lessened, his mind clearer and excitement diminished, our hope of a speedy termination may be strengthened accordingly. But if the maniacal attack does not subside in a few days or a week, and it appears probable that it will be an affair not of days but of weeks or of months, what are we to do? The friends may be altogether against us if we propose to send the patient to an asylum, and will profess their readiness to consent to any other alternative. The question, I have found, resolves itself chiefly into one of cost. There are many patients, noisy, hilarious, and excited, who are not homicidal or suicidal or dangerously violent; by good attendants they can be nursed through an attack of mania, if the friends will defray the expense and obey the orders of the physician. In nine cases out of ten they must be removed from home. Home surroundings and the presence of near relatives are prejudicial, so that a house or rooms must be taken and made suitable for such a case. The windows must be made safe, bedsteads and furniture must be removed, a bed must be made on the floor, and the services of several good attendants must be secured. To harness a patient to a bedstead by his ankles and wrists, and to keep him thus day after day and week after week, is not treatment; though, unfortunately, it is even now occasionally met with and acquiesced in by friends who disdain the services of a lunacy doctor, and put up with anything rather than listen to what they know would be the advice of such a specialist. And not only will it be necessary to have suitable rooms for the patient. There must be the means of giving him air and exercise, not in the streets of a town, but in a garden or country fields. All this involves considerable expense, but where the case is suitable and money no object, it is often worth while to try to effect a cure by this method, and so save the stigma of an asylum and the subsequent feelings of the individual. Of course, it often happens that the friends, though at first professing that they wish to do anything rather than consent to an asylum, do in time get tired of the heavy expenditure and the trouble which the management of attendants and servants gives them. Being too often very nervous people, and born with the same temperament, they can stand the racket no longer, and to an asylum the patient goes. But many get well without. There are some patients, however, who ought to be sent at once, whom it is hopeless to treat in a single house, and who will become worse and more and more dangerous by such treatment. These are persons affected not by noisy and active mania of recent origin, but by delusions which have gradually deepened till they are dangerous, to a degree, to those about them. Such cannot be kept in one room as sick patients. They require considerable exercise, the vigilance of many attendants, and the authority of a superintending physician. They demand moral control and coercion. The discipline of an asylum and the loss of their liberty act beneficially upon their disorder, and the example of others is frequently of the highest service. We see also many patients whose malady we recognize as chronic and incurable, being often of long standing. It is a pity to let these or their friends be impoverished by the expense incidental to a residence outside an asylum when to the latter they must come at last, and to one less comfortable if all their money has gone. There is, too, one large class of male patients, the general paralytics, whom it is very difficult to treat out of an asylum, except in rare cases where quiet imbecility sets in at once. The restless, exalted, and often angry excitement which characterizes the early stages demands larger room and space than a private house affords, and the

feeling of intense self-satisfaction and happiness prevents their feeling the restraint or chafing at their surroundings.

Passing from the acutely excited and maniacal patients, we may consider for a moment the treatment of the depressed or melancholic. The non-asylum treatment of such has probably been tried in most instances before resort is had to a specialist. They have been sent away from home, have had rest from work and more or less medical treatment. But the gloom has deepened and special advice is sought. Are we to send them to an asylum? Here the question of suicidal propensity will come in. Many I hear say that an asylum is the only proper place for a suicidal patient. But an asylum is not necessarily an absolute safeguard against suicide, as the reports of our commissioners show. Nothing but the persistent vigilance of good attendants will prevent it, and vigilance may be applied both out of and in an asylum. There is more than one kind of suicidal patient. Some there are who will not make the attempt unless they have a tempting opportunity, and are left alone by those who should look after them. But others will try to injure themselves all day long even in the presence of others, will try to gouge out their eyes, throw themselves out of bed to break their bones, and, in fact, damage themselves in every conceivable way. So, again, some will refuse food, but will eat on compulsion or allow themselves to be fed with a spoon. Others require to be fed by force either by nasal or œsophagus tube, entailing the united efforts of several people. Clearly, some of these can only be treated in an asylum, while the less acute may be kept in a suitable house, and by vigilant attendants and judicious companions nursed back to health. The outcome of this melancholia, however, is very different from the short and acute attacks of mania of which I have spoken before. It is the most tedious of all the curable forms of insanity. We have to carry out the treatment, not for days or weeks, but for months, consequently expense becomes a serious question, and poor patients have to go to an asylum because they cannot afford the cost of a house and attendants. It is hopeless to attempt to cure such at home. The sight of home and home surroundings intensifies the gloomy ideas, and urges the sufferer to suicide.

There is a reason for placing melancholic patients in an asylum which in my opinion is far more important than the fear of suicide. It is the intense egoism, self-feeling, or selfishness—whatever we choose to call it—which distinguishes so many. They will tell us that there never was such a case before; that they never can get well; that we are altogether unable to comprehend their individual symptoms. One of these patients will completely upset his home, and we order him away, but placed in a house with attendants and companions, or in a doctor's house, he may still be able to make himself the centre and focus of everybody's attention, and his self-importance is fostered and not diminished by the mode. Place this man in an asylum of, say, a hundred patients, make him the hundredth part of the community instead of the one important unit, and the effect is often wonderful. Possibly he has been refusing his food, and has required to be fed with much coaxing and entreating. Seated in the midst of twenty others, he takes it because they do, and no one seems particularly to care whether he takes it or not. The best medicine for many of these people is what I term *judicious neglect.*

To cure a patient's insanity without having to send him to an asylum is a matter of great satisfaction both to one's self and the friends of the sufferer. It is akin to the feeling of the surgeon when he saves a limb after a severe accident, or heals a diseased joint without having recourse to an amputation. But I am not one of those —and this I wish clearly to enunciate—who think that all, or nearly all, acute cases of

insanity can be treated in private dwellings. We hear from every one who is capable of forming an opinion that early treatment is necessary, and that if a case is not treated early, it will in all probability become chronic and confirmed. With this I cordially agree, and when I have spoken of many cases treated successfully out of an asylum, it is because I wish to point out that there are some such, and that in practice we not unfrequently meet with them, recognizing them amidst the many others for whom an asylum is the proper place, and alone can afford the proper treatment. Many must go because they have no funds and no friends. Taken care of by the State, they must be sent to a public institution. If we do not send a case thither, it must be because we think we can treat it as well and successfully outside; and if our method fails, and it is plain that the patient is getting worse and not better, we ought to bring it to an end and have recourse to an asylum, overcoming the reluctance and prejudices of friends, who so constantly think not of the patient, but of themselves.

It will be obvious that the limit of time allowed here does not allow more than the briefest outline of a subject which to treat in full would demand a chapter far exceeding that which I am able to set before you. It is impossible to enter upon the question of the treatment of such cases, but American physicians are perfectly conversant with this, and I need not enter upon it, even if time allowed. There is another matter connected with it, namely the law. How far are we justified in treating our patients outside an asylum without having recourse to lunacy laws and all the apparatus of medical certificates and orders? I am glad to be spared all reference to this question. In this country the laws, I believe, differ in different States. I can say, however, what the law ought to do. It ought to enable us to treat a case for a certain time outside an asylum without having recourse to certificates, just as we should treat any ordinary case of illness, as the delirium of fever, or delirium tremens. In Scotland there is this power. In England there is not, and I fear there is not much hope that there will be. I hope that here such provision exists, and if it does not, that it will at no distant date.

I have thought fit to bring this subject before you, because it is often thrown at us that lunacy doctors wish to send every patient to an asylum, and have no other method of treatment. As there are many who imperatively demand the care of an asylum, not only that they may be kept in safety but cured, so are there others whose recovery may be brought about without being so sent. It is our duty to discriminate so far as we can between them. Those who suffer from transitory mania are not the dangerous class who should be sent to an asylum as speedily as possible. The delay of a few days will often not materially interfere with the after treatment, if an asylum is eventually necessary; and in this period recovery may take place. I cannot hope to have assisted you much within the limit of this paper in the diagnosis of such cases, but, at any rate, I may claim to have directed your attention to them.

At the conclusion of Dr. Blandford's address, Dr. CORDES, of Geneva, moved a vote of thanks, which was seconded by Dr. KRETCHMAR, of Brooklyn. The question being taken, the motion was carried unanimously.

SIXTH DAY.

The Congress met in final session on Saturday at 9.30 A. M.

Dr. GRAILY HEWITT, of London, then advanced and said :—

I come before you as one of the foreign members of the Congress, and I have been requested to express in a few words the appreciation which is felt by the foreign members of this Congress of the efforts which have been made by the Executive Committee of the Congress for the furtherance of the object of this meeting, and to convey to them our thanks for the attentions that have been bestowed upon us, and our appreciation of the success which has attended their efforts. I desire also to express our sense of the hospitality, of the kindness and attention which we have received, both in public and private, at this great meeting—attentions which will contribute to make our visit to Washington a source of congratulation and a happy memory in the future. I beg to submit to you the following more formal expression of our ideas :—

Dr. HEWITT then read the following resolution :—

On the part of the foreign visitors and officers of the Congress, we desire to convey to the President of the United States our best thanks for his presence at the ceremony of the inauguration of this Congress. We desire also to thank the President of the Congress for his urbanity and attention during the meetings. We desire to express to the Executive Committee of the Congress, particularly to Dr. Henry H. Smith, Dr. John B. Hamilton, Dr. A. Y. P. Garnett, Dr. Toner, and Dr. Arnold, our very high appreciation of the efforts they have made for efficient organization, action and working of the Congress, which have rendered it so great a success. We would convey our warmest thanks to the citizens of Washington for the hospitality, both public and private, we have received during our pleasant visit to their beautiful city.

Dr. HEWITT'S remarks and resolutions were loudly applauded.

Dr. MARTIN, of Berlin, said :—

Das Werk des IX. internationalen medicinischen Congresses naht sich seinem Ende ; es bleibt uns nur noch übrig, der Pflicht der Dankbarkeit zu genügen. Die Arbeit des Congresses ist heute noch nicht zu übersehen. Die Vielgestaltigkeit einer solchen Körperschaft lässt Das, was die einzelnen Glieder geleistet haben, erst allmälig voll zu Tage treten. Aber Alles deutet darauf hin, dass dieser IX. Congress sich an Arbeitsleistung seinen Vorgängern würdig anschliesst.

Uebersehen wir dagegen Das, was von Seiten Derer geschehen, welche die schwere Arbeit übernommen haben, das Werk dieses Congresses zu leiten, so müssen wir voll Dankbarkeit und Bewunderung anerkennen, was sie geleistet haben.

Nicht mit Unrecht hat man diese Congresse mit den olympischen Festen verglichen ; wie damals vor 2000 Jahren, ist die gebildete Welt ausgezogen, um im fröhlichen Reigen ihre Kräfte zu stählen, das Gefühl ihrer Zusammengehörigkeit zu pflegen. Die Griechen konnten die Namen der Sieger und auch wohl die Derjenigen, denen besonderer Dank gezollt wurde, in eherne Tafeln eingraben und im Tempel des Olympischen Zeus der Nachwelt aufbewahren. Wir sind ärmer ; wir

können unseren Dank den Leitern dieses Congresses nur in der bescheidenen Form eines Votums darbringen, das mein Freund Graily Hewitt Ihnen vorgelegt hat, und welches ich Sie herzlich bitte, anzunehmen.

Dr. E. LANDOLT, of Paris, said :—

Monsieur le Président! Mesdames et Messieurs!

J'ai été désigné pour exprimer à Monsieur le Président des Etats Unis nos sentiments de profonde reconnaissance. Tout en craignant de n'être qu'un porte parole bien indigne, je m'empresse d'accepter cet honneur, car la gratitude qui vient du cœur n'a pas besoin d'éloquence pour être entendue.

Monsieur le Président et Messieurs! Nous avons déjà assisté à toute une série de congrès internationaux; nous avons été bien reçu partout; les pays d'Europe ont rivalisé de zèle pour nous rendre agréable le séjour dans leurs plus belles cités; mais il ne nous a pas été donné souvent de voir le Chef de l'Etat en personne se mêler à nos travaux. Le Président de cette Grande République a bien voulu descendre parmi nous, pour nous souhaiter la bien — venue de sa voix sympathique; il nous a conviés a son propre *home* et a échangé avec chacun de nous cette poignée de main qui signifie la plus cordiale hospitalité. Monsieur le Président Cleveland a donné ainsi à notre congrès sa véritable sanction et le plus grand charme à notre séjour dans cette capitole.

En retournant dans nos foyers, nous garderons de Monsieur le Président de la République le souvenir le plus reconnaissant et le plus respectueux et nous dirons au monde entier que les Etats Unis, déjà si favorisés et si bénits, possèdent, avant tout, un chef digne d'eux, qui les dirige sûrement dans la voie du Progrès et de la Prospérité.

Dr. EDMUND OWEN, of London, said he desired to second Dr. Hewitt's resolutions. He thought he was admirably qualified to discourse on American politics, as he was entirely ignorant of the subject. In science they had no politics. "We are extremely happy," said he, "to have visited the United States during the presidency of Mr. Cleveland." He said he asked an American friend the other day, and a very intelligent man, if he voted for Mr. Cleveland, and the American said, "No." Then he asked the American if he were satisfied now, and the laconic answer was, "Guess I am." "If," said Dr. Owen, "that question was put to this meeting, 'Are we satisfied?' I say, 'Guess we are.'" A great man like the President might have found something else to occupy his attention, say, in the White Mountains, about this time of the year; he might have been driven away by the doctors, but instead of that he remained here to welcome them. "When the history of this grand country is written," he said, "we trust President Cleveland will have a niche in the temple of fame side by side with those great men, Lincoln and Garfield. When you are in our country we love to hear you say, 'We admire your Queen.' We can say to you with all truth we admire, we love your queen of beauty and grace, and in seconding this vote of thanks we simply express a prayer that Mr. President Cleveland and Mrs. Cleveland may long continue in health and strength to preside over a happy, a prosperous, and a united country."

THE PRESIDENT. I will put the motion to a vote, as form requires it, but it seems scarcely necessary. Those favoring the motion will say aye.

There were no noes.

The SECRETARY-GENERAL then stated that the Section on Dental and Oral Sur-

gery had presented Dr. Findlay Hunt a gold-headed cane in token of their appreciation of his services.

Dr. JOHN B. HAMILTON, the Secretary-general, said :—

MR. PRESIDENT: I could not fail to be deeply sensible of the great kindness bestowed upon me, and the many words of encouragement received both during this present meeting and for the past six months. I am profoundly grateful for the expressions now placed upon record. But, sir, the success of this Congress is due to no one man. All on this side worked with the spirit and enthusiasm of Americans whose hearts were in the work, but all our efforts would have been in vain and the strongest would have failed had it not been for that noble army of scientific men abroad, who, deaf to all misrepresentations of disappointed factionists, came from old England; the universities and vine-clad hills of France; the seats of learning in Germany; the cities of the Alps; from sunny Italy; the lowlands of Holland; the fastnesses of the Danube; from far-off, but ever-near Russia; from the golden shores of China and Japan; from Cairo and the sands of Egypt, and the everlasting hills of Palestine; and I say to these representatives here assembled that to them we owe a debt of gratitude which time cannot efface, and as these our dear colleagues have braved the dangers of the deep, and sustained, in some cases, the shock of calumny to be with us, so let us here say that when we again meet in Berlin in 1890, let it be to renew those friendships formed here, and to once more grasp the hands of those who have been true to us now. (*Great applause.*)

The PRESIDENT, after referring to his more than half a century of professional life, said it had been one of the objects of his life to promote the harmony which should exist among all branches of medicine. It was to-day the happiest feeling of his life that he could now stand here in this capital of the United States, and not only see the profession he loved united and working harmoniously, but stand here among the representatives from almost every country on the globe. He thanked those who had come across the water to attend the Congress, not only in his own name, but in the name of the profession in America, in whose name the Congress had been invited to sit here. There were, he said, members of the profession from every State in this great Union assembled here to greet the foreign members. There is a larger gathering than has been in any other Congress, leaving out the profession of the city in which they have been held. He thanked the members of the Congress for the courtesy shown him in sustaining him in the discharge of his duties. "Life with me," he said, "is not long, but if it is spared with sufficient health I shall take great satisfaction in meeting my friend, Dr. Martin, and all his comrades, in Berlin in 1890. I now declare the Ninth International Medical Congress adjourned *sine die.*"

The members then proceeded to the Navy Yard and embarked on the U. S. steamer "Dispatch" for Mount Vernon. Refreshments were served on board, and after their return the foreign members were taken on an excursion to the Falls of Niagara, leaving Washington at 6.45 P. M.

REGISTER OF THE CONGRESS.

JOSEPH M. TONER, M.D., Registrar.

LIST OF DELEGATES.

Argentine Republic.

Dr. Angel J. Villa.

Austria—Hungary.

MILITARY DEPARTMENT, ... Dr. Ladislaus von Farkas.
SOCIETY OF AUSTRIAN DENTISTS, ... Joseph Metnitz.

Belgium.

THE BELGIAN GOVERNMENT, ... Dr. D. Leopold Servais.

China.

MEDICAL MISSIONARY SOCIETY, ... Henry Wm. Boone.

France.

DELEGATES, ... Dr. Charpentier.
Dr. Chervin.
Dr. Dujardin-Beaumetz.
Dr. Léon Lablé.
Dr. Léon Le Fort.
Dr. Trélat.
Dr. Vallin.
Dr. Lefébre.

ACADÉMIE DE MÉDECINE, ... Dr. Charpentier.
Dr. Léon Le Fort.

FACULTÉ DE MÉDECINE, ... Dr. Léon Le Fort.
SOCIÉTÉ MÉDICALE, ... Dr. Emile Poussé.
L'ASSOCIATION DES MÉDECINES DE LA SEINE, ... Dr. Grelat.

ÉCOLE DENTAIRE DE PARIS, ... Paul Dubois.
George Kuhn.

German Empire.

THE GERMAN GOVERNMENT, ... Dr. Eulenberg.
ROYAL WIRTEMBERG MEDICAL COLLEGE, . Dr. Wildermuth.

Great Britain and Colonies.

Army Medical Service,	Dr. Jeffery A. Marston.
Royal Navy,	Dr. Wm. H. Lloyd.
Bristol Medical School,	Dr. A. E. Lawrence.
Liverpool Medical Institution,	Dr. Richard Caton. Dr. Boyd Burnett Joll. Dr. Roger Parker. Dr. Geo. G. S. Taylor.
Medical Society of Health,	Dr. George Wilson.
British Medical Association,	Dr. James Stuart.
Inverness Medical Society,	Dr. James M. Chapman.
North Wales Branch British Medical Association,	Dr. Robert Roberts.

AUSTRALIA.

Medical Society of Victoria,	Dr. Walter Balls-Headley.

CANADA.

Lafayette Médicale de Montreal,	Dr. Joseph M. Beausaliel.
Canada Medical Association,	Thos. F. McLean. R. W. B. Smith. William Sloan. Michael Sullivan.
Ontario Medical Council,	Dr. V. H. Moore.
Provincial Board of Health—Ontario,	Dr. Francis Rae.
Local Board of Health—Toronto,	Dr. Wm. Cannill.
Toronto Medical Society,	Dr. J. A. Temple.
L'Union Médicale du Canada,	Dr. A. A. Foucher.
Trinity Medical College,	Prof. W. B. Geikie.

INDIA.

Madras University,	Dr. D. P. Banajee.

Italy.

The Italian Government,	Com. Prof. M. Semmola. Prof. Francesco Durante.
The Italian Surgical Society,	Prof. Durante.
The University and Medical Academy of Rome,	Prof. Durante.
Medical Faculty of Rome, Royal Academy of Medicine,	Prof. E. DeRossi.
Associazione dei Benemeriti Italiani,	Dr. L. Servais.

Japan.

Imperial Japanese Navy,	Dr. Rûchiro Saiki.

Mexico.

THE MEXICAN GOVERNMENT, ... { Dr. Ygnacio Alvarado.
Dr. F. Marin.

Netherlands.

THE GOVERNMENT OF THE NETHERLANDS, Dr. Th. MacGillavry.

Roumania.

THE ROUMANIAN GOVERNMENT, ... Dr. Assaky.

Russia.

THE WAR DEPARTMENT OF RUSSIA, . . . Dr. Charles Reyher.
FINSKA LAKARESÄLLSKAPET, ... Dr. Emil Hougberg.

Spain.

THE SPANISH GOVERNMENT, ... Dr. Lalearda.

Sweden and Norway.

THE SWEDISH ARMY, ... Dr. E. E. Mobeck.

Switzerland.

THE GOVERNMENT OF SWITZERLAND, . . { Prof. Th. Kocher.
Dr. Henry Banga.

Turkey.

SYRIA.

SYRIAN PROTESTANT COLLEGE, ... Prof. Geo. E. Post.

United States of America.

WAR DEPARTMENT, ... Lieut. Col. and Surgeon Joseph R. Smith.

NAVY DEPARTMENT, ... {
Medical Director George Peck.
" " Albert C. Gorgas.
" " David Kindleberger.
Surgeon Theoron Wolverton.
" M. L. Ruth.

MARINE HOSPITAL SERVICE, ... {
Surgeon P. H. Bailhache,
" Walter Wyman.
" Hiram W. Austin.
" John Godfrey.

LIST OF MEMBERS.

EXTRACT FROM THE RULES OF ORGANIZATION.—"The Congress shall consist of members of the regular profession of medicine, who shall have inscribed their names on the register, and shall have taken out their tickets of admission; and of such other scientific men as the Executive Committee of the Congress may see fit to admit."

A

Abadie, Charles, ... Paris, France.
Abbot, Griffith Evan, ... General Wayne, Pa.
Abbott, Frank, ... New York City.
Abney, Olin L., ... Victoria, Texas.
Acker, George Nicholas, ... Washington, D. C.
Acree, Horace Marshall, ... Chattanooga, Tenn.
Adair, Robert Benjamin, ... Gainesville, Ga.
Adams, Daniel S., ... Manchester, N. H.
Adams, Elmon A., ... Washington, D. C.
Adams, Henry Florentine, ... Colton, Cal.
Adams, Jesse Lee, ... Washington, D. C.
Adams, Peter N., ... Dayton, Ohio.
Adelrich, Albert, ... Zürich, Switzerland.
Ainsworth, Frederick C.,* . . . . . U. S. Army.
Akins, William Thomas, ... Chicago, Ill.
Aldendorff, A.,* . . . . . Berlin, Germany.
Alexander, John Dalzelle, ... Wooster, Ohio.
Alexander, Lee M., ... Marshall, Mo.
Alexander, Lewis Daniel, ... Kilcreggan, Scotland.
Alexander, W.,* . . . . . Liverpool, England.
Allaway, J. Johnson, ... Montreal, Canada.
Allbutt, Arthur,* . . . . . Leeds, England.
Alleger, Walter Wheeler, ... Washington, D. C.
Allen, Charles Linnæus, ... Rutland, Vt.
Allen, Charles Stover, ... New York City.
Allen, Dudley Peter, ... Cleveland, Ohio.
Allen, Ezra Pascal, ... Athens, Pa.
Allen, Harlan Preston, ... Columbus, Ohio.
Allen, James Glen, ... Brooklyn, N. Y.
Allen, John, ... New York City.
Allen, J. Thomas, ... Shreveport, La.
Allen, Mary E., ... Philadelphia, Pa.
Allen, Nathan,* . . . . . Lowell, Mass.

Allport, Walter Webb, ... Chicago, Ill.
Allyn, George W., ... Pittsburgh, Pa.
Althaus, Julius,* . . . . . London, England.
Alvarado, Ygnacio, ... Mexico.
Alvord, Austin W., ... Battle Creek, Mich.
Ames, William Bergen, ... Chicago, Ill.
Anawalt, James White, ... Greensburg, Pa.
Ancrum, John L., ... Charleston, S. C.
Andeer, Justus, ... Munich, Germany.
Anderson, John, ... London, England.
Anderson, McCall,* . . . . . Glasgow, Scotland.
Anderson, William,* . . . . . London, England.
Anderson, William, ... Indiana, Pa.
Andrews, Edmund, ... Chicago, Ill.
Andrews, Judson Boardman, ... Buffalo, N. Y.
Andrews, R. R., ... Cambridge, Mass.
Andrieu, Dr.,* . . . . . Paris, France.
Andros, Frederick, ... Mitchell, Dak.
Angle, Edward H., ... Minneapolis, Minn.
Antisell, Thomas, ... Washington, D. C.
Antisell, Thomas, Jr.,* . . . . . Martinsville, Mont.
Aplin, Alfred Swinton, ... Nottingham, England.
Apostoli, George, ... Paris, France.
Appleby, James Frederick Ross, ... Washington, D. C.
Ard, Frank C., ... Baltimore, Md.
Armstrong, S. T., ... U. S. Marine Hospital Service.
Armstrong, William,* . . . . . Manchester, England.
Arndt, Rudolf,* . . . . . Greifswald, Germany.
Arnold, Abram B., ... Baltimore, Md.
Arnold, Edmund Samuel Foster, ... Newport, R. I.
Arnold, Otto, ... Columbus, Ohio.
Arthur, George, ... Washington, D. C.
Arthur, Hugh Wilson, ... Pittsburgh, Pa.
Arwine, John S., ... Columbus, Ind.
Asdale, William James, ... Pittsburgh, Pa.
Ashby, Henry,* . . . . . Manchester, England.
Ashmore-Noaks, Samuel S., ... Nice, France.
Ashton, Lawrence, ... Falmouth, Va.
Aspell, John, ... New York City.
Assaky, Georges, ... Bucarest, Roumania.
Atchison, William A., ... Nashville, Tenn.
Atkinson, George Armstrong,* . . . . . Edinburgh, Scotland.
Atkinson, William Biddle, ... Philadelphia, Pa.
Atkinson, William Henry, ... New York City.
Atwater, H. H.,* . . . . . Burlington, Vt.
Atwood, Le Grand, ... St. Louis, Mo.
Audhoui, Victor,* . . . . . Paris, France.
Austin, Hiram William, ... U. S. Marine Hospital Service.
Avard, Dr.,* . . . . . Paris, France.
Ayres, Stephen C.,* . . . . . Cincinnati, Ohio.

B

Bacon, William Lewis, ... Lexington, Va.
Baggett, John B., ... Washington, D. C.
Baginsky, Adolph,* . . . . . Berlin, Prussia.
Bailey, Charles M., ... Minneapolis, Minn.
Bailey, Jonathan Rice, ... Olmstead, Ky.
Bailey, Wm. Curtiss, ... Albion, N. Y.
Bailhache, Preston Heath, ... Philadelphia, Pa.
Baines, Allen MacKenzie, ... Toronto, Canada.
Baird, George, ... Wheeling, W. Va.
Baker, Albert Rufus, ... Cleveland, Ohio.
Baker, Charles O., ... Auburn, N. Y.
Baker, Frank, ... Washington, D. C.
Baker, George W., ... Brooklyn, N. Y.
Baker, Henry Brooks, ... Lansing, Mich.
Baker, Julian Meredith, ... Tarboro, N. C.
Baldwin, Aristides Edwin, ... Chicago, Ill.
Baldwin, Benjamin J., ... Montgomery, Ala.
Baldwin, Lewis Kemble, ... Philadelphia, Pa.
Baldwin, William Oliver, ... Washington, D. C.
Baldwin, Wm. Wilberforce,* . . . . . Florence, Italy.
Balkwill, William Edward, ... London, England.
Ball, Charles A., ... Washington, D. C.
Balls-Headley, Walter, ... Melbourne, Australia.
Balmer, Abraham F., ... Brookville, Pa.
Banajee, Dadabhoy Pestonjee, ... Bombay, India.
Banes, S. Thompson, ... Camden, N. J.
Banga, Henry, ... Chicago, Ill.
Banker, Charles Coffin, ... Meriden, Conn.
Banks, William Ayer, ... Rockland, Me.
Banks, Wm. Mitchell,* . . . . . Liverpool, England.
Bannister, H. M.,* . . . . . Kankakee, Ill.
Bantock, Geo. Granville,* . . . . . London, England.
Baratoux, Jean, ... Paris, France.
Barclay, James Thomas, ... Cleveland, Ohio.
Barker, T. Ridgway, ... Philadelphia, Pa.
Barker, William, ... Providence, R. I.
Barksdale, Randolph, ... Petersburg, Va.
Barnes, Henry, ... Cleveland, Ohio.
Barnes, Ira Norton, ... Decatur, Ill.
Barnes, Robert,* . . . . . London, England.
Barr, George W., ... Titusville, Pa.
Barrett, William Cary, ... Buffalo, N. Y.
Barrett, William Marshall, ... Boston, Mass.
Bartlett, Charles Herstine, ... Parkersburg, W. Va.
Bartlett, John, ... Chicago, Ill.
Barton, Philip Hale, ... Washington, D. C.
Barwell, Richard,* . . . . . London, England.
Bary, Vera, ... Germantown, Pa.
Bass, William, ... Lowell, Mass.
Bastian, Charlton H., ... London, England.

Batten, John Mullin, ... Pittsburgh, Pa.
Battle, Samuel Westray, ... Asheville, N. C.
Baum, Orion, ... Indian Creek, Va.
Baumler, Christian,* . . . . . Freiberg, Germany.
Baxter, Edwin C., ... Albany, N. Y.
Baxter, Jedediah Hyde, ... U. S. Army.
Bayne, John W., ... Washington, D. C.
Beach, John N., ... West Jefferson, Ohio.
Beall, Elias James, ... Fort Worth, Texas.
Beard, Ferdinand W., ... Vincennes, Ind.
Beardsley, Charles E., ... Ottawa, O.
Beatty, Hugh Wilson, ... Washington, D. C.
Beaumetz, Dujardin,* . . . . . Paris, France.
Beaumont, Charles William, ... Clarksville, Tenn
Beausalicl, Joseph M., ... Montreal, Canada.
Beaver, Daniel B. D., ... Reading, Pa.
Beck, Charles, Sidles, ... Wilkesbarre, Pa.
Beck, John S., ... Dayton, Ohio.
Beebe, Warren L., ... St. Cloud, Minn.
Beers, W. George, ... Montreal, Canada.
Behrend, Adajah, ... Washington, D. C.
Behrens, B. M., ... Chicago, Ill.
Bejach, Max, ... Jena, Germany.
Bell, Agrippa Nelson, ... New York City.
Bell, J. H.,* . . . . . Bradford, England.
Bell, James Richard, ... Cleveland, Ohio.
Bell, John Wesley, ... Minneapolis, Minn.
Bell, William Henry, ... Logansport, Ind.
Bellamy, William J. H., ... Wilmington, N. C.
Belt, Alfred McGill, ... Baltimore, Md.
Bemis, Merick,* . . . . . Worcester, Mass.
Benham, Silas Nelson, ... Pittsburgh, Pa.
Bennett, Ebenezer O., ... Wayne, Mich.
Bennett, George Hosmer, ... Lima, N. Y.
Benson, George W., ... Baltimore, Md.
Bently, Edwin, ... U. S. Army.
Benton, Samuel, ... London, England.
Benton, Stewart Henry, ... Brooklyn, N. Y.
Berens, Bernard, ... Philadelphia, Pa.
Berger, Lyman Adams, ... Kansas City, Mo.
Berlin, Henry, ... Chattanooga, Tenn.
Bermann, Isidore, ... Washington, D. C.
Bernard, Gerald, ... Silksworth, England.
Bernard, Marius, ... Cannes, France.
Bernays, Augustus Charles, ... St. Louis, Mo.
Berrey, John James, ... Portsmouth, N. H.
Berry, William,* . . . . . Lancashire, England.
Berryman, John, ... St. John, New Brunswick.
Besharian, John H., ... Chicago, Ill.
Best, William L., ... Johnson's Mill, N. C.
Betty, Edward George, ... Cincinnati, Ohio.
Beville, Alexander Archer, ... Waco, Texas.

Beyer, Henry Gustav, ... U. S. Navy.
Bickford, Henry H., ... Memphis, Tenn.
Biedkling, Gustav, ... Stuttgart, Germany.
Biedler, Hampson Hubert, ... Baltimore, Md.
Bigelow, Edward E., ... Owatonna, Minn.
Bigelow, Horatio R.,* . . . . . Washington, D. C.
Biggs, Herman M., ... New York City
Bill, Curtis Harvey, ... Bridgeport, Conn.
Billard, Jules F . . . . . Laurel, Md.
Billings, John Shaw, ... U. S. Army.
Billmeyer Uriah Dildine, ... Chattanooga, Tenn.
Binswanger, D.,* . . . . . Jena, Germany.
Bird, James C., ... Washington, D. C.
Birmingham, Henry P., ... Ft. Myer, Va.
Bishop, Seth Scott, ... Chicago, Ill.
Bishop, Sylvester Parker, ... Delta, Ohio.
Bittinger, Joseph Henry, ... Hanover Pa.
Blackader, A. D.,* . . . . . London, England.
Blackburn, Isaac Wright, ... Washington, D. C.
Blackford, Benjamin, ... Lynchburg, Va.
Blackford, Jr., Charles Minor, ... Lynchburg, Va.
Blackley, Oliver L., ... Sparta, Pa.
Blaisdell, Irving C., ... Wilmore, Pa.
Bland, Jasper James, ... Houma, La.
Bland, Mortimer Alberto, ... Charlotte, N. C.
Blandford, George Fielding, ... London, England.
Bleiler, Peter Oliver, ... Denver, Pa.
Bliss, D. Willard, ... Washington, D. C.
Bliss, Ellis B., ... Washington, D. C.
Blitz, Adolph, ... Minneapolis, Minn.
Bloom, Homer C., ... Martinsburg, Pa.
Bloom, J. N., ... Louisville, Ky.
Blose, J. U., *. . . . . Altoona, Pa.
Blount, Rufus Fielding, ... Wabash, Ind.
Blumer, George Alder, ... Utica, N. Y.
Boarman, Charles V., ... Washington, D. C.
Bodecker, Charles W., ... New York City.
Bodkin, Dominick G., ... Brooklyn, N. Y.
Boenning, Henry Casper, ... Philadelphia, Pa.
Bogan, Samuel William, ... Washington, D. C.
Bogie, Marcus A., ... Kansas City, Mo.
Bogue, Edward A., ... New York City.
Boisliniere, Louis C., ... St. Louis, Mo.
Bond, James A., ... Kansas City, Mo.
Bond, Samuel S., ... Washington, D. C.
Bond, Young H., ... St. Louis, Mo.
Bontecou, Reed Brockway, ... Troy N Y
Booker, William David, ... Baltimore, Md.
Boone, Henry Wm., ... Shanghai, China.
Boor, Leonard Nelson,* . . . . . New Zealand.
Boor, Walter Axline, ... New Castle, Ind.
Borck, Edward, ... St. Louis, Mo.

Bornville, Wm. Gibson Arlington, ... Philadelphia, Pa.
Bosher, Lewis C., ... Richmond, Va.
Boskowitz, G. W., ... New York, N. Y.
Bossert, Jacob, ... Washingtonville, Ohio.
Bouchereau, A., ... Paris, France.
Bouchut, E., ... Paris, France.
Boulware, Theodrick Clay, ... Butler, Mo.
Bouton, Alonzo Goule, ... Savannah, Ga.
Bovee, John Wesley, ... Washington, D. C.
Bowen, Asa B., ... Maquoketa, Iowa.
Bowen, Charles Henry, ... Washington, D. C.
Bower, David, ... Bedford, England.
Bower, Richard Norris, ... Sandy, Bedfordshire, England.
Bowker, John Copps, ... Brooklyn, N. Y.
Boyd, John C., ... Washington, D. C.
Boyers, James S., ... Decatur, Ind.
Boysen, Theophilus Henry, ... Egg Harbor City, N. J.
Bozeman, Nathan, ... New York City.
Bozeman, Nathan Gross, ... New York City.
Brackett, Charles Albert, ... Newport, R. I.
Brackett, John Ely, ... Washington, D. C.
Bradford, Edward Hickling, ... Boston, Mass.
Bradford, J. Rose,* . . . . . London, England.
Brady, John, ... Grand Rapids, Mich.
Braffet, James Henry, ... Paw Paw, Ill.
Brailey, W. A.,* . . . . . London, England.
Brainerd, Henry Green, ... Los Angeles, Cal.
Brallier, Emanuel, ... Chambersburg, Pa.
Braman, Francis N., ... New London, Ct.
Bramble, David Denman, ... Cincinnati, Ohio.
Brand, Elihu Edward, ... Exeter, England.
Brandes, C., ... Erie, Pa.
Brandt, John Selden, ... Ontario Centre, N. Y.
Branham, Joseph H., ... Baltimore, Md.
Brant, Austin Colman, ... Canton, Ohio.
Brasseur, Dr.,* . . . . . Paris, France.
Bratton, James Rufus, ... Yorkville, S. C.
Braun, Gustav,* . . . . . Vienna, Austria.
Breinig, P. B., ... Bethlehem, Pa.
Breisky, A.,* . . . . . Vienna, Austria.
Bremer, L.,* . . . . . St. Louis, Mo.
Brewington, Wm. Jason, ... Saint Mary's, Ohio.
Brice, Robert Stockton, ... Keota, Iowa.
Bridges, Vernon Roe, ... Mattoon, Ill.
Briggs, Albert Henry, ... Buffalo, N. Y.
Briggs, Herman M.,* . . . . . New York City.
Briggs, Wm. Thompson, ... Nashville, Tenn.
Bright, Hardin Welch, ... Richmond, Ky.
Brinton, Wilson M., ... Sharpsburg, Pa.
Briscoe, Walter C., ... Washington, D. C.
Bristowe, John S.,* . . . . . London, England.
Brock, Luther S., ... Morgantown, West Va.

Brockway, Albert H., ... Brooklyn, N. Y.
Brodeur, Aydrie, ... Montreal, Canada.
Brodie, Benjamin Pitcher, ... Detroit, Mich.
Brodie, William, ... Detroit, Mich.
Bromwell, Josiah Robson, ... Washington, D. C.
Bronson, Charles Eastbrook,. . . . . Washington, D. C.
Brook, George W., ... Ellsworth, Ohio.
Brookings, David Jenness, ... Woodward, Iowa.
Brooks, James, ... Plains, Pa.
Brooks, John Gaunt, ... Paducah, Ky.
Brooks, John Henry, ... Brooks' Station, D. C.
Brophy, Truman Wm., ... Chicago, Ill.
Brosseau, Alfred T., ... Montreal, Canada.
Brower, Daniel Roberts, ... Chicago, Ill.
Brown, Adelbert Erastus, ... Chicago, Ill.
Brown, Charles W., ... Elmira, N. Y.
Brown, Dillon,* . . . . . New York City.
Brown, E. Parmley, ... Flushing, N. Y.
Brown, Frank Wilmot, ... Detroit, Mich.
Brown, George, ... Barre, Mass.
Brown, Gustavus R., ... Washington, D. C.
Brown, John W., ... Mottville, N. Y.
Brown, Hawkins, ... Hustonville, Ky.
Brown, Seneca Buell, ... Fort Wayne, Ind.
Brown, Ulysses Higgins, ... Syracuse, N. Y.
Brown, William Augustus, ... Washington, D. C.
Browne, James Crichton,* . . . . . London, England.
Browne, John Mills, ... Washington, D. C.
Browne, John Walton, ... Belfast, Ireland.
Browne, Lennox, ... London, England.
Browne, William T., ... Jewett City, Conn.
Browning, William,* . . . . . New York City.
Bruce, Hilary Sanford, ... Waverly, Ala.
Brucker, Charles M., ... Tell City, Ind.
Bruckheimer, Moses, ... Washington, D. C.
Brugsch, A.,* . . . . . Cairo, Egypt.
Brumbaugh, Andrew Boelus, ... Huntingdon, Pa.
Brumme, Carl Conrad George, ... Detroit, Mich.
Brumund, Peter, ... Idaho Springs, Colorado.
Brunno, H. D.,* . . . . . New Orleans, La.
Brush, Edward Nathaniel, ... Philadelphia, Pa.
Bryan, Joseph Hammond, ... Washington, D. C.
Bryant, Joseph D.,* . . . . . New York City.
Bryant, T.,* . . . . . London, England.
Bryce, Peter, ... Tuscaloosa, Ala.
Bryce, Peter Henderson, ... Toronto, Canada.
Bubert, Charles H., ... Baltimore, Md.
Buchanan, John Jenkins, ... Pittsburgh, Pa.
Buchanan, Thomas Drysdale, ... Glasgow, Scotland.
Buchanan, William A., ... Paris, France.
Bucher, Isaac Reilly, ... Lebanon, Pa.
Buchly, Walter C., ... Washington, D. C.

Budia, P.,* . . . . . Paris, France.
Buechner, Wm. Lewis, ... Youngstown, Ohio.
Bulkley, John Wells, ... Washington, D. C.
Bull, Ole Bornemann, ... Christiana, Norway.
Bullard, Archie, ... Norwich, England.
Buller, Frank, ... Montreal, Canada.
Bullington, Richard Edward, ... Memphis, Tenn.
Burbeck, Charles H., ... Troy, N. Y.
Burchard, William Metcalf, ... Uncasville, Conn.
Burd, Edwin, ... Lisbon, Iowa.
Burke, George W., ... New Castle, Ind.
Burnes, Bernard,. . . . . Allegheny, Pa.
Burnett, Swan Moses, ... Washington, D. C.
Burnham, Albert W., ... Lowell, Mass.
Burns, James Hepburn, ... Toronto, Canada.
Burr, W. J., ... Newark Valley, N. Y.
Burten, J. E.,* . . . . . Liverpool, England.
Burton, George Correll, ... Washington, D. C.
Busch, Frederick, ... Berlin, Prussia.
Busey, Samuel C., ... Washington, D. C.
Bush, Lewis Potter, ... Wilmington, Del.
Bush, Robert Thacher, ... Gallatin, Tenn.
Burton, Matthew Henry, ... Troy, N. Y.
Butler, Charles Richard, ... Cleveland, Ohio.
Butler, Charles S., ... Buffalo, N. Y.
Butler, Luins C.,* . . . . . Essex, Vt.
Buvinger, Charles Wesley, ... Pittsburgh, Pa.
Buxton, Dudley W.,* . . . . . London, England.
Byford, Henry T., ... Chicago, Ill.
Byford, W. H.,* . . . . . Chicago, Ill.
Byrd, Daniel Ellis, ... Marvell, Ark.
Byrne, Charles B., ... Washington, D. C.
Byrne, Charles Christopher, ... Washington, D. C.
Byrnes, James Carroll, ... Washington, D. C.

C

Cadwallader, D. Willis, ... Philadelphia, Pa.
Calais, P., ... Hamburg, Germany.
Caldwell, Charles, ... Chicago, Ill.
Caldwell, Charles Tufts, ... Washington, D. C.
Calhoun, A. W., ... Atlanta, Ga.
Calkins, Cheney Homer, ... Springfield, Mass.
Call, Edward B., ... Peoria, Ill.
Callan, Cornelius Van Ness, ... Washington, D. C.
Callender, John Hill, ... Nashville, Tenn.
Calnek, Thomas Maurice, ... Cartago, Costa Rica.
Calvert, J. Thomas, ... Spartanburg, S. C.
Cameron, James C., ... Montreal, Canada.
Campbell, Arthur J., ... Portland, Conn.

Campbell, John, ... Washington, D. C.
Campbell, Samuel Alexander, ... Mattoon, Ill.
Campbell, Walter, ... Dundee, Scotland.
Campbell, Wm. Macfie,* . . . . . Liverpool, England.
Cannill, William, ... Toronto, Canada.
Cardeza, John T. M., ... Claymont, Del.
Carmichael, Spotswood Wellford, ... Fredericksburg, Va.
Carmona, Manuel,* . . . . . Mexico.
Carnochan, John Murray, ... New York City.
Carolin, William T., ... Lowell, Mass.
Carpenter, A. P.,* . . . . . Cleveland, Ohio.
Carpenter, Henry Wesley, ... Oneida, N. Y.
Carpenter, John Evans, ... Washington, D. C.
Carpenter, Wesley M., ... New York City.
Carr, Albert G., ... Durham, N. C.
Carr, Lawrence, ... Charlestown, W. Va.
Carr, William, ... New York City.
Carreau, Joseph S., ... New York City.
Carrier, Frederic, ... Philadelphia, Pa.
Carrigan, Andrew N., ... Washington, Ark.
Carroll, Craft C., ... Meadville, Pa.
Carroll, Harris C., ... Meadville, Pa.
Carroll, James Joseph, ... Washington, D. C.
Carroll, Joseph Myles, ... Washington, D. C.
Carson, Louis Oliver, ... Trader's Point, Ind.
Carson, N. B.,* . . . . . St. Louis, Mo.
Carson, Robert Bruce, ... Durant, Miss.
Carstens, J. Henry, ... Detroit, Mich.
Cartaz, A.,* . . . . . Paris, France.
Carter, C. Shirley, ... Warrenton, Va.
Carter, James Madison Gore, ... Waukegan, Ill.
Carter, James Owen, ... Lincoln, Neb.
Carvelle, Henry D. W., ... Manchester, N. H.
Casati, Luigi,* . . . . . Forti, Italy.
Case, A. G., ... Pittsburgh, Pa.
Case, Calvin S., ... Jackson, Mich.
Casselberry, William Evans, ... Chicago, Ill.
Cassidy, James Stephen, ... Covington, Ky.
Castle, Frank Edwin, ... Waterbury, Conn.
Catching, Benjamin H., ... Atlanta, Ga.
Cathell, William T., ... Baltimore, Md.
Caton, Richard, ... Liverpool, England.
Ceccherelli, Andrea,* . . . . . Parma, Italy.
Centaro, Vincenzo,* . . . . . Barletta, Italy.
Chaille, Stanford E., ... New Orleans, La.
Chamberlin, Frank Tenney, ... Washington, D. C.
Chamberlin, G. M., ... Chicago, Ill.
Chambers, John, ... Indianapolis, Ind.
Champion, Joseph Van Meter, ... Potomac, Md.
Champlin, Anthony Parker, ... Bay St. Louis, Miss.
Chance, George Hubert, ... Portland, Oregon.
Chancellor, Eustathius, ... St. Louis, Mo.

Chancellor, James Edgar, ... University of Virginia, Va.
Channing, Walter, ... Brookline, Mass.
Chapin, H. E., ... Fredericksburg, Va.
Chapin, John Bassett, ... Philadelphia, Pa.
Chapman, James Milne, ... Inverness, Scotland.
Charles, Edmonston, ... Limehurst, England.
Charles, Joseph, ... Newport News, Va.
Charlton, Samuel Henry, ... Seymour, Ind.
Charpentier, Arthur, ... Paris, France.
Charpentier, Louis Arthur, ... Paris, France.
Chase, Robert H., ... Norristown, Pa.
Chase, Rolla Miner, ... Bethel, Vt.
Chauveau, Jean F., ... New York City.
Chavasse, Thomas Frederic, ... Birmingham, England.
Cheatham, Richard, ... Nashville, Tenn.
Cheatham, William, ... Louisville, Ky.
Cheatham, William Thomas, ... Henderson, N. C.
Chervin, A.,* . . . . . Paris, France.
Chew, Thomas John, ... Washington, D. C.
Chewning, George H., ... Fredericksburg, Va.
Chiari, Domenico,* . . . . . Florence, Italy.
Chiari, O.,* . . . . . Vienna, Austria.
Chilton, R. H., ... Dallas, Tex.
Chisholm, Edmund Sevier, ... Tuscaloosa, Ala.
Chisholm, Julian J., ... Baltimore, Md.
Chittenden, Charles C., ... Madison, Wis.
Christen, Eugene, ... Paris, France.
Christian, Edmund P., ... Wyandotte, Mich.
Christie, William, ... St. John, N. B., Canada.
Church, William Paty, ... Providence, R. I.
Cisna, William Reed, ... Ickesburg, Pa
Clagett, Luther S., ... Blairsville, Pa.
Clapp, Dwight Moses, ... Boston, Mass.
Clark, Alfred, ... Englewood, Ill.
Clark, Archibald Campbell, ... Bothwell, Scotland.
Clark, Daniel, ... Toronto, Canada.
Clark, Emert F., ... Minneapolis, Minn.
Clark, Eugene, ... Lockhart, Tex.
Clark, John A., ... Bedford, Pa.
Clark, Rufus Oscar, ... Marlboro', Mass.
Clark, Simeon Tucker, ... Lockport, N. Y.
Clarke, Augustus Peck, ... Cambridge, Mass.
Clarke, Rowan, ... Tyrone, Pa.
Clarkson, Henry Nazyck, ... Haymarket, Va.
Clay, David Milton, ... Shreveport, La.
Cleland, James, ... Detroit, Mich.
Clement, James Noyes, ... Oxford, Pa.
Clifton, William Ridley, ... Waco, Texas.
Cline, Godfrey H., ... Jersey Shore, Pa.
Clippingdale, S. O.,* . . . . . London, England.
Close, James A., ... Summerfield, Ill.
Cluthe William, ... Tell City, Ind.

Coan, Titus Munson,* . . . . . New York City.
Coats, Joseph,* . . . . . Glasgow, Scotland.
Cockerille, Saml. Johnston, ... Washington, D. C.
Coe, William Hayden, ... Washington, D. C.
Coey, A. J.,* . . . . . Chicago, Ill.
Coffman, Victor H., ... Omaha, Neb.
Coggeshall, Henry, ... Newport, R. I.
Coghill, John George Sinclair, ... Ventnor, Isle of Wight, England.
Cohen, J. Solis, ... Philadelphia, Pa.
Cokenower, Harvey L., ... Clarinda, Iowa.
Cole, R. Beverly,* . . . . . San Francisco, Cal.
Coles, Walter, ... St. Louis, Mo.
Colgrove, William Henry, ... Johnstown, N. Y.
Collins, Benjamin Malone, ... Fairfax C. H., Va.
Collins, Eli, ... Little Rock, Ark.
Collins, James, ... Philadelphia, Pa.
Collins, John Dillard, ... Covington, Ky.
Collins, John Webster, ... Toronto, Ohio.
Collins, J. S., ... Frankfort, Ky.
Colton, Aaron, ... Hudson, N. Y.
Comegys, C. G., ... Cincinnati, Ohio.
Comstock, Thomas Griswold, ... St. Louis, Mo.
Coni, Emilio R.,* . . . . . Buenos Ayres, Argentine Repub.
Conklin, William Judkins, ... Dayton, Ohio.
Conlin, Berthold M. J., ... Alexandria, Dak.
Connell, James G., ... Pittsburgh, Pa.
Conner, Leartus, ... Detroit, Mich.
Conrad, J. S., ... St. Denis, Md.
Conrad, William, ... St. Louis, Mo.
Cook, Chas. Delano, ... Brooklyn, N. Y.
Cook, Edgar Humphrey, ... Mendota, Ill.
Cook, George Wythe, ... Washington, D. C.
Cook, Samuel L., ... Washington, D. C.
Cook, William Cowden, ... Nashville, Tenn.
Coomes, Martin Francis, ... Louisville, Ky.
Coop, Wm. Alfred Henderson, ... Friendship, Tenn.
Coope, Albert Franklin, ... Oil City, Pa.
Cooper, Alfred M., ... Point Pleasant, Pa.
Cooper, Joshua M., ... Johnstown, Pa.
Cooper, William D., ... Morrisville, Va.
Corbin, Eliakim L., ... Washington, D. C.
Cordes, August, ... Geneva, Switzerland.
Cormany, James Wilson, ... Mt. Carroll, Ill.
Corson, Hiram, ... Conshohocken, Pa.
Coskery, Oscar John, ... Baltimore, Md.
Cousins, E. Radcliffe,* . . . . . London, England.
Cousins, J. Ward,* . . . . . Portsmouth, England.
Coutts, John Alfred,* . . . . . London, England.
Cowan, George, ... Danville, Ky.
Cowden, John William, ... Rock Island, Ill.
Cox, Ernest F. DeLecq Le Grisley, ... La Rocque, Jersey, England.
Cox-Hippisley, William Ashley, ... Leicester, England.

Cox, John T., ... Moberly, Mo.
Cox, Nathaniel D., ... Spencer, Ind.
Coyle, James Mathias, ... Nashville, Tenn.
Craig, Alexander, ... Columbia, Pa.
Craig, James W., ... Churchville, N. Y.
Cravens, Junius Edgar, ... Indianapolis, Ind.
Craver, S. Belle, ... Bryan, Ohio.
Crawford, James Young, ... Nashville, Tenn.
Crawford, John Henry, ... Raleigh, N. C.
Crawford, S. K.,* . . . . . Chicago, Ill.
Crawford, Thomas A., ... Rock Hill, S. C.
Crego, Floyd S., ... Buffalo, N. Y.
Creighton, Clias,* . . . . . London, England.
Crenshaw, William, ... Atlanta, Ga.
Cronyn, John, ... Buffalo, N. Y.
Crook, Harrison, ... Washington, D. C.
Cross, Elisha Wilder, ... Rochester, Minn.
Cross, Francis Richardson, ... Bristol, England.
Cross, William Henry, ... London, England.
Crothers, Robert Wm., ... Delavan, Ill.
Crothers, Thomas D., ... Hartford, Conn.
Crudeli, Tommasi,* . . . . . Rome, Italy.
Cruttenden, Henry Louis, ... Northfield, Minn.
Culbertson, Howard, ... Zanesville, Ohio.
Cullen, J. S. Dorsey, ... Richmond, Va.
Cunningham, D. J.,* . . . . . Dublin, Ireland.
Cunningham, George, ... Cambridge, England.
Cunningham, Henry Schette, ... Indianapolis, Ind.
Cunningham, Russell McWhorter, ... Pratt Mines, Ala.
Cupples, Geo., ... San Antonio, Texas.
Curtin, Roland G., ... Philadelphia, Pa.
Curtis, George Lenox, ... Syracuse, N. Y.
Curtis, H. H.,* . . . . . New York City.
Curtis, Lester, ... Chicago, Ill.
Cushing, Ernest Watson, ... Boston, Mass.
Cutter, Ephraim, ... New York City.
Cutter, John Ashburton, ... New York City.

D

Dabney, William Cecil, ... University of Virginia, Va.
Daggett, Byron H., ... Buffalo, N. Y.
Dalton, Benj. Neale, ... London, England.
Dalton, Henry Clay, ... St. Louis, Mo.
Daly, William Hudson, ... Pittsburgh, Pa.
Dana, Henry Thomas, ... Cortland, N. Y.
Dandridge, H. P.,* . . . . . Cincinnati, Ohio.
Danforth, Isaac Newton, ... Chicago, Ill.
Daniel, W. O.,* . . . . . Bullards, Ga.
Dann, Archibald, ... Rochester, N. Y.

Dannaker, Christian Augustus, ... Kansas City, Mo.
Darby, Charles Hammond, ... St. Joseph, Mo.
Darby, John E., ... Cleveland, Ohio.
Darr, Hiram Henry, ... Caldwell, Texas.
Daugherty, Charles A., ... South Bend, Ind.
David, John H., ... Little Rock, S. C.
David, William J., ... Bennettsville, S. C.
Davidson, Alexander,* . . . . . Liverpool, England.
Davidson, Edwin M., ... Cleveland, Ohio.
Davies, Ellis Thos.,* . . . . . Liverpool, England.
Davies, Francis Pritchard, ... Maidstone, England.
Davies, Gomer, ... London, England.
Davis, Emory Hawkins, ... Plainfield, Conn.
Davis, Henry H., ... Camden, N. J.
Davis, H. L., ... Wellsboro, Pa.
Davis, Kenyon Brown, ... Springfield, Ill.
Davis, John Allen Wright, ... Galesburg, Ill.
Davis, John Daniel Sinkler, ... Birmingham, Ala.
Davis, Lloyd L., ... Eaton Rapids, Mich.
Davis, Nathan Smith, ... Chicago, Ill.
Davis, Jr., Nathan Smith, ... Chicago, Ill.
Davis, Robt. Chalmers, ... Seneca, S. C.
Davis, Thomas Davis, ... Pittsburgh, Pa.
Davison, Calvin Knox, ... Stanhope, N. J.
Davison, Ferdinand, ... Richmond, Va.
Davison, James Edward, ... Negley, Pa.
Davy, Thomas Henry Maddox, ... Baltimore, Md.
Dawson, Ezekiel, ... Frederica, Del.
Dawson, Jr., John Lawrence, ... Charleston, S. C.
Dawson, W. W., ... Cincinnati, Ohio.
Day, Henry Lawrence, ... Eau Claire, Wis.
Day, Richard Hance, ... Baton Rouge, La.
Day, W. H.,* . . . . . Hamburg, Germany.
Dearing, F. H., ... Braintree, Mass.
Deas, P. Maury,* . . . . . Exeter, England.
De Amicis, Tomasso,* . . . . . Naples, Italy.
Debaker, Felix,* . . . . . Roubaux, France.
D'Espine, Ad.,* . . . . . St. Malo, France.
De Ford, William H., ... Anamosa, Iowa.
De Garmo, Wm. B., ... New York City.
De Gassecourt, Cadet,* . . . . . Paris, France.
De Groer, François, ... Warsaw, Poland (Russia).
Delaney, John O'Fallon, ... St. Louis, Mo.
De Laney, John Pope, ... Geneva, N. Y.
De Montbrun, Domingo,* . . . . . Trinidad, W. I.
Denison, Charles, ... Denver, Col.
Dennis, Charles Parker, ... Portsmouth, Ohio.
Dennis, David Nichols, ... Erie, Pa.
Dennis, Frank Wellington, ... Unionville, N. Y.
Dennis, Frederick Shepard, ... New York City.
Dent, William Marmaduke, ... Newburg, W. Va.
Dercum, Francis X, ... Philadelphia, Pa.

De Rossi, Emilio, ... Rome, Italy.
Desvernine, C. M.,* . . . . . Havana, Cuba.
De Valcourt, Thomas, ... Cannes, France.
De Vilbiss, Allen, ... Toledo, Ohio.
De Villa, Angel J., ... Buenos Ayres, Argentine Repub.
De Vries, Henry, ... New York City.
De Watraszewski, Xavier, ... Warsaw, Poland.
De Wecker, L.,* . . . . . Paris, France.
Dewey, Richards,* . . . . . Kankakee, Ill.
De Winthuyssen, F. Laborde,* . . . . . Madrid, Spain.
De Wolf, James Henry, ... Baltimore, Md.
Dexter, J. E., ... Washington, D. C.
De Zouche, Isaac, ... Gloversville, N. Y.
Dibrell, Jr., James Anthony, ... Little Rock, Ark.
Dickenson, Wm. Howship,* . . . . . London, England.
Dickey, John Lindsay, ... Wheeling, W. Va.
Dickinson, Fannie, ... Chicago, Ill.
Dickinson, Gordon K., ... Jersey City, N. J.
Dickinson, William, ... St. Louis, Mo.
Dickinson, Wm. Pliny, ... Dubuque, Iowa.
Dickson, Charles Rea, ... Kingston, Canada.
Dickson, Joseph Nichols, ... Pittsburgh, Pa.
Dickson, James Glenn, ... Cannonsburg, Pa.
Dickson, Samuel Henry, ... Washington, D. C.
Didama, Henry Darwin, ... Syracuse, N. Y.
Diedel, Charles F., ... Alexandria, Va.
Dight, C. F.,* . . . . . Beirut, Syria.
Dimmitt, Frank W., ... Oneida, Ill.
Dirner, Gustav Adolf, ... Buda Pesth, Hungary.
Dissinger, Hiram, ... Canal Fulton, Ohio.
Divine, James Henry, ... Sioux Rapids, Iowa.
Dixon, R. B.,* . . . . . Boston, Mass.
Dobson, Nelson Congreve, ... Bristol, England.
Dolan, Thomas Michael, ... Halifax, England.
Dolan, Thos. W.,* . . . . . Halifax, N. S.
Doleris F. Amédeé, ... Paris, France.
Donald, James Turner, ... Paisley, Scotland.
Donaldson, Charles Adrien, ... Murrysville, Pa.
Donaldson, Robert Bruce, ... Washington, D. C.
Donan, Hugo, ... Dresden, Germany.
Donelan, Edward A., ... St. Joseph, Mo.
Donnally, Williams, ... Washington, D. C.
Donohue, Florence, ... Washington, D. C.
Doolittle, George Tilton, ... New Haven, Conn.
Dorland, Edwin H., ... Chicago, Ill.
Dorrance, William Henry, ... Ann Arbor, Mich.
Dostor, B. R., ... Blakeley, Ga.
Dotterer, Louis P., ... Charleston, S. C.
Douges, John W., ... Camden, N. J.
Doughty, William Henry, ... Augusta, Ga.
Douglas, Byron, ... Appleton, Wis.
Douglas, George, ... Oxford, N. Y.

Douglass, James Wallace, ... Boonville, N. Y.
Dovilliers, Leopold V., ... Washington, D. C.
Down, John Langdon, ... London, England.
Downey, Jesse W., ... New Market, Md.
Downs, Edwin Day, ... Owego, N. Y.
Doyle, Byron Oscar, ... Louisville, Ky.
Drake, Charles Mütter, ... Knoxville, Tenn.
Drake, Emery Gaston, ... Antrim, Pa.
Drake, Martin E., ... Mount Alton, Pa.
Drake, William Marion, ... Lamar, Mo.
Dreschfield, Julius,* . . . . . Manchester, England.
Driggs, Stoddard, ... Lexington, Ky.
Dronke, F.,* . . . . . Berlin, Prussia.
Drury, George Albert, ... Washington, D. C.
Drysdale, Thomas M., ... Philadelphia, Pa.
Dubois, Paul, ... Paris, France.
Dudley, Albion Manley, ... Salem, Mass.
Dudley, A. Palmer, ... New York City.
Duffield, Samuel P., ... Detroit, Mich.
Duffy, Jr., Charles, ... New Berne, N. C.
Dulaney, Joshua Lambert, ... Washington, D. C.
Dumont, J.,* . . . . . Berne, Switzerland.
Duncan, Arthur McDowell, ... Bucyrus, Ohio.
Duncan, Goelson,* . . . . . London, England.
Duncan, John Harris, ... Kansas City, Mo.
Duncan, Matthews,* . . . . . London, England.
Duncanson, J. J. Kirk,* . . . . . Edinburgh, Scotland.
Dunglison, Richard James, ... Philadelphia, Pa.
Dunlap, Albert, ... Fort Smith, Ark.
Dunlap, Alexander, ... Springfield, Ohio.
Dunlap, Charles Wm., ... Springfield, Ohio.
Dunlap, William Dukes, ... Selma, Ala.
Dunmire, George Benson, ... Philadelphia, Pa.
Dunn, Charles E., ... Louisville, Ky.
Dunning, John D., ... Webster, N. Y.
Dunning, Lehman H., ... South Bend, Ind.
Dunsmoor, Frederick A., ... Minneapolis, Minn.
Durand-Fardel, Max,* . . . . . Paris, France.
Durante, Francisco, ... Rome, Italy.
Durham, William Meigs, ... Atlanta, Ga.
Dupignac, Bezaleel Howe, ... New York City.
Dupignac, Rosa H. S., ... New York City.
Dupree, James William, ... Baton Rouge, La.
Dupusy, Orens, ... Pau, France.
Duquet, E. E., ... Montreal, Canada.
Dwinelle, William H., ... New York City.
Dwyer, J. O., ... New York City.
Dyer, Reuben F., ... Ottawa, Ill.

E

Earle, Charles Warrington, ... Chicago, Ill.
Earley, Charles Richard, ... Ridgway, Pa.
Early, Lewis M., ... Columbus, Ohio.
Easley, Francis Guernsey, ... Ridgway, Pa.
East, Edward,* . . . . . London, England.
Eastland, Orin, ... Wichita Falls, Texas.
Eastman, Lewis M., ... Baltimore, Md.
Eastman, William, ... Mineral Point, Wis.
Eaton, F. B.,* . . . . . Portland, Oregon.
Eaton, Roswell G., ... Manchester, N. H.
Eddowes, Alfred, ... Salop, England.
Edebohls, George M., ... New York City.
Edgerly, Daniel W., ... Cambridge, Mass.
Edis, Arthur W.,* . . . . . London, England.
Edmondson, James J., ... Philadelphia, Pa.
Edson, Cyrus, ... New York City.
Edwards, Charles Gray, ... Louisville, Ky.
Edwards, John, ... Gloversville, N. Y.
Edwards, Landon B., ... Richmond, Va.
Edwards, T. E.,* . . . . . Memphis, Tenn.
Eggers, John Thaddeus, ... Kansas City, Mo.
Egle, William Henry, ... Harrisburg, Pa.
Egleston, George W., ... Washington, D. C.
Ehrendorfer, Emil,* . . . . . Vienna, Austria.
Eisenberg, Philip Yerger, ... Norristown, Pa.
Eisner, Maurice, ... Carlsbad, Austria.
Elder, Elijah S., ... Indianapolis, Ind.
Eldridge, Edward Fayette, ... New London, Wis.
Eliot, Ellsworth, ... New York City.
Eliot, Gustavus, ... New Haven, Conn.
Eliot, Llewellyn, ... Washington, D. C.
Ellegood, Robert G., ... Concord, Del.
Ellery, William, ... LaGrange, Mo.
Ellinwood, Albert E., ... Attica, N. Y.
Elliott, Francis M., ... Aurora, Ill.
Elliott, George Robert, ... New York City.
Elliott, W. St. George, ... London, England.
Ellis, Heber Dowling, ... Eastbourne, England.
Elmer, Andrew B., ... Rochelle, Ill.
Elmer, Henry W., ... Bridgeton, N. J.
Elsner, Henry L., ... Syracuse, N. Y.
Elsner, John, ... Denver, Col.
Emack, Frank Dulin, ... Phœnixville, Pa.
Emminger, Allen Franklin, ... Columbus, Ohio.
Encbuske, Claes, ... Lund, Sweden.
Encbuske, Claes Julius, ... Lund, Sweden.
Enfield, Americus, ... Bedford, Pa.
Ensign, Herbert Dwight, ... Boone, Iowa.
Entrikin, Franklin Wayne, ... Findlay, Ohio.
Erdman, William Breinig, ... Macungie, Pa.

Erwin, Alexander James, ... Mansfield, Ohio.
Esmarch, Friederich,* . . . . . Kiel, Germany.
Etheridge, James Henry, ... Chicago, Ill.
Eubank, Alfred, ... Birmingham, Ala.
Eubank, George, ... Birmingham, Ala.
Eulenberg, A.,* . . . . . Berlin, Prussia.
Evans, Earl, ... Winchester, N. H.
Evans, Edwin, ... Rome, N. Y.
Evans, Jr., J. M., ... Evansville, Wis.
Evans, Thomas Benjamin, ... Baltimore, Md.
Evans, Warwick, ... Washington, D. C.
Evans, W. Warrington, ... Washington, D. C.
Evart, Joseph,* . . . . . Brighton, England.
Evatt, Geo. Joseph Hamilton,* . . . . . London, England.
Eve, Paul Fitzsimmons, ... Nashville, Tenn.
Everts, O.,* . . . . . Cincinnati, Ohio.
Ewart, Joseph,* . . . . . London, England.
Ewing, Fayette Clay, ... Washington, D. C.
Ewing, James Breading, ... Uniontown, Pa.
Ewing, William G., ... Nashville, Tenn.

F

Faison, Isaac Wellington, ... Mt. Olive, N. C.
Farnham, Robert, ... Washington, D. C.
Farrar, John Natting, ... New York City.
Farrier, Edmund,* . . . . . Halifax, N. S.
Fay, John, ... Altoona, Pa.
Fazio, Eugenio,* . . . . . Naples, Italy.
Felkin, Robert W.,* . . . . . Edinburgh, Scotland.
Fell, George E., ... Buffalo, N. Y.
Fellows, Dana Willis, ... Portland, Me.
Felsenreich, T.,* . . . . . Vienna, Austria.
Fenwick, George P., ... Washington, D. C.
Fenwicke, George E.,* . . . . . Montreal, Canada.
Ferguson, Everard D., ... Troy, N. Y.
Ferguson, Franklin B., ... Deer Isle, Me.
Ferguson, James Gordon, ... Brooklyn, N. Y.
Ferrier, David,* . . . . . London, England.
Ferris, Allison Bunnell, ... New Paris, Ohio.
Ferris, Edgar Samuel, ... New Castle, Ind.
Field, George Lindsey, ... Detroit, Mich.
Field, Martin H., ... Indianapolis, Ind.
Filanus, J. W.,* . . . . . Amsterdam, Holland.
Filbert, Peter K., ... Pottsville, Pa.
Fillebrown, Thomas, ... Boston, Mass.
Finch, Cyrus Myron, ... Columbus, Ohio.
Finck, Theodore D., ... Louisville, Ky.
Findley, William Martin, ... Altoona, Pa.
Findley, William Robison, ... Altoona, Pa.

Fink, Isaac William, ... Hillsboro, Ill.
Finley, Mark Florus, ... Washington, D. C.
Finley, Mary J., ... Mansfield, Ohio.
Finney, A. Gorden, ... St. Louis, Mo.
Firman, Francis W., ... Findlay, Ohio.
Fischer, Karl Edwin, ... Trenton, N. J.
Fischel, Washington E., ... St. Louis, Mo.
Fishback, Martin Luther, ... Columbus, Mo.
Fisher, Charles H., ... Providence, R. I.
Fisher, Frank, ... Philadelphia, Pa.
Fisher, George Jackson,* . . . . . Sing Sing, N. Y.
Fisher, Theodore Willis, ... Boston, Mass.
Fitch, James P., ... Morgantown, W. Va.
Fitch, Simon,* . . . . . Halifax, N. S.
Fitch, William H., ... Rockford, Ill.
Fitch, Willard Rufus, ... Knowlesville, N. Y.
Flandrau, Thomas Macomb, ... Rome, N. Y.
Flannigan, Edward, ... St. Louis, Mo.
Fleming, Andrew, ... Pittsburgh, Pa.
Fletcher, Mordecai Hiatt, ... Cincinnati, Ohio.
Fletcher, Thomas Asa, ... New York City.
Fletcher, W. B.,* . . . . . Indianapolis, Ind.
Flick, John William, ... Honeoye Falls, N. Y.
Flint, Austin, Sr., ... New York City.
Flint, Austin, ... New York City.
Fly, A. W., ... Austin, Tex.
Flynn, William, ... Marion, Ind.
Follitt, John A., ... Boston, Mass.
Fonis, Civilion, ... Bridgeport, Conn.
Foot, W. K.,* . . . . . New Orleans, La.
Forbes, Samuel Franklin, ... Toledo, Ohio.
Forbes, Wm. Henry, ... Richmond Hill, N. Y.
Ford, August,* . . . . . Zürich, Switzerland.
Ford, Thomas Griffin, ... Shreveport, La.
Ford, W. Hutson, ... St. Louis, Mo.
Formad, Henry F., ... Philadelphia, Pa.
Formad, Marie K., ... Philadelphia, Pa.
Formento, Felix, ... New Orleans, La.
Forsythe, Robert,* . . . . . Leeds, England.
Forwood, William Stroup, ... Darlington, Me.
Foster, A. Clarence,* . . . . . Leeds, England.
Foster, H. L., ... Kansas City, Mo.
Foster, John Milton, ... Richmond, Ky.
Foster, Thomas Albert, ... Portland, Me.
Foster, Warren Woden, ... Putnam, Conn.
Foster, William Sill, ... Pittsburgh, Pa.
Foucher, A. A., ... Montreal, Canada.
Fountain, Jas. Lide, ... Bryan, Texas.
Fowler, Silas W., ... Delaware, Ohio.
Fox, David Raymond, ... Jesuit Bend, La.
Fox, Gustav Theodore, ... Bath, Pa.
Fox, Lorenzo Smith, ... Lowell, Mass.

Fox, T. Colcott,* . . . . . London, England.
Fox, William, ... Milwaukee, Wis.
Francis, Charles E., ... New York City.
Franzoni, Charles William, ... Washington, D. C.
Fraser, William Edwin, ... Bismarck, Dakota Ter.
Frazer, James Leslie, ... Inverness, Scotland.
Frazer, William,* . . . . . Bournemouth, England.
Freeman, Alfred John, ... San Remo, Italy.
Freeman, Andrew Washington, ... Chicago, Ill.
Freeman, Charles M., ... Washington, D. C.
Freeman, Daniel Bigelow, ... Chicago, Ill.
Freeman, Henry William, ... Bath, England.
Freeman, Robert Ross, ... Nashville, Tenn.
Freeman, Samuel A., ... Buffalo, N. Y.
Freeman, Walter Watson, ... Warren, Pa.
Freire, Dominigos, ... Rio de Janeiro, Brazil.
French, George F.,* . . . . . Minneapolis, Minn.
French, James Magoffin, ... Cincinnati, Ohio.
French, R. D. DeL., ... Washington, D. C.
French, Simon Starkweather, ... Battle Creek, Mich.
French, William B., ... Washington, D. C.
Fresenius, A., ... Frankfort-on-the-Main, Germany.
Freudenthal, Wolff, ... New York City.
Frick, Theodore, ... Zürich, Switzerland.
Fricke, Albert, ... Philadelphia, Pa.
Friedenwald, Aaron, ... Baltimore, Md.
Friedrich, L. L., ... Washington, D. C.
Friedrichs, George John, ... New Orleans, La.
Frigerio, Louis,* . . . . . Alexandria, Italy.
Frothingham, G. E., ... Ann Arbor, Mich.
Fruitnight, John Henry, ... New York City.
Fry, Frank R.,* . . . . . St. Louis, Mo.
Fry, Henry Davidson, ... Washington, D. C.
Fuller, Albert Homer, ... St. Louis, Mo.
Fuller, Andrew J., ... Bath, Me.
Fuller, George E., ... Monson, Mass.
Fuller, S. E.,* . . . . . Brooklyn, N. Y.
Fullmer, Charles L., ... Renovo, Pa.
Fulton, John F., ... St. Paul, Minn.
Fundenberg, Walter Franklin, ... Pittsburgh, Pa.
Fundenberg, Walter Hulliken, ... Pittsburgh, Pa.
Furbeck, Peter R., ... Gloversville, N. Y.

G

Gable, Isaac C., ... York, Pa.,
Gabriel, J. Frank, ... Piqua, Ohio.
Gaine, Charles, ... Bath, England.
Gaines, Thomas Foster, ... Warrenton, Va.
Galabin, Alfred Lewis,* . . . . . London, England.

Galezowski, Dr., ... Paris, France.
Gallaher, Thomas Jones, ... Pittsburgh, Pa.
Gallet, A., ... Brussels, Belgium.
Galton, John Henry, ... London, England.
Galuzzo, Vicente, ... Havana, Cuba.
Gamble, Hamilton McSparrin, ... Moorefield, W. Va.
Gans, Emanuel S., ... Philadelphia, Pa.
Gantt, Harry Baldwin, ... Waterbury, Md.
Gardiner, E. J., ... Chicago, Ill.
Gardiner, Frank H., ... Chicago, Ill.
Gardner, Charles H., ... Philadelphia, Pa.
Gardner, Ira Kilborne, ... New Hampton, Iowa.
Gardner, William,* . . . . . Montreal, Canada.
Garlock, William D., ... Little Falls, N. Y.
Garmany, Jasper Jewett, ... New York City.
Garner, Edward Samuel, ... St. Joseph, Mo.
Garnett, Alexander P., ... Washington, D. C.
Garrabrant, Clarence, ... New Gretna, N. J.
Garratt, Alfred Chas., ... Boston, Mass.
Gartrell, John Hutchens, ... Cornwall, England.
Garvin, Henry D., ... Washington, D. C.
Gary, Franklin F., ... Abbeville, S. C.
Gaston, Jas. McF., ... Atlanta, Ga.
Gates, Lowell M., ... Scranton, Pa.
Gawne, Augustus J., ... Sandusky, Ohio.
Gay, Norman, ... Columbus, Ohio.
Gayet, A.,* . . . . . Lyons, France.
Gayle, Virginius Wilton, ... Hancock, Md.
Gaylord, Edward Sanford, ... New Haven, Conn.
Gazzo, John Baptiste Columbus, ... Thibodeaux, La.
Geiger, Jacob, ... St. Joseph, Mo.
Geikie, Walter Bayne, ... Toronto, Canada.
Gemmill, Jacob M., ... Philadelphia, Pa.
Genese, David, ... Baltimore, Md.
George, Conrad, ... Ann Arbor, Mich.
Geran, Josiah Pease, ... Brooklyn, N. Y.
Gerhard, Jerome, Zwingli, ... Harrisburg, Pa.
Gerlach, Joseph V.,* . . . . . Erlangen, Germany.
Getz, H. Landis, ... Marshalltown, Iowa.
Ghent, Henry Clay, ... Belton, Tex.
Gibbon, Robert, ... Charlotte, N. C.
Gibbs, Edward Augustus, ... Washington, D. C.
Gibson, John B., ... Cowansville, Canada.
Gibson, John St. Pierre, ... Staunton, Va.
Gidden, Charles H., ... Little Falls, N. Y.
Giesecking, Henry N., ... Washington, D. C.
Gifford, H.,* . . . . . Omaha, Neb.
Gihon, Albert Leary, ... U. S. Navy.
Gilbert, Caleb B., ... Detroit, Mich.
Gilbert, Lloyd S., ... Susquehanna, Pa.
Gilbert, S. Eldred, ... Philadelphia, Pa.
Gill, Henry Z.,* . . . . . El'Dorado, Kan.

Gillet, Charles Henri Alexander, ... Paris, France.
Gillett, Harry Webster, ... Newport, R. I.
Gilliam, Charles Frederick, ... Washington, D. C.
Gilliam, David Tod, ... Columbus, Ohio.
Gilliam, William C., ... New York City.
Gillis, William, ... Fort Covington, N. Y.
Gilman, John Henry, ... Lowell, Mass.
Gilmer, W. B. S., ... New York City.
Gilmore, Arnold P., ... Chicago, Ill.
Girard, A. C., ... Boisé City, Idaho Territory.
Gish, John L., ... Jackson, Mich.
Glasgow, Frank A., ... St. Louis, Mo.
Glennan, A. H., ... Washington, D. C.
Glisan, R., ... Portland, Oregon.
Gnezda, Julius, ... Berlin, Germany.
Goddard, Clark La Motte, ... San Francisco, Cal.
Godding, William Whitney, ... Washington, D. C.
Godfrey, Edmund L. B., ... Camden, N. J.
Godfrey, John, ... Louisville, Ky.
Godwin, Chas. Henry Young,* . . . . . London, England.
Goelet, Augustus H., ... New York City.
Goldspohn, Albert, ... Chicago, Ill.
Gonalsey, Miquel Gonzalez,* . . . . . Periana, Spain.
Goodheart, James F.,* . . . . . London, England.
Goodlove, Wm. McKinnon, ... Washington, D. C.
Goodman, Henry Earnest, ... Philadelphia, Pa.
Goodrich, Loftus Hyatt, ... Phœnix, Arizona Territory.
Goodwillie, James, ... New York City.
Goodwin, Azeo Edson, ... Rockford, Ill.
Goodwin, Ralph Schuyler, ... Thomaston, Conn.
Gordon, Samuel,* . . . . . Dublin, Ireland.
Gordon, Seth C., ... Portland, Me.
Goré, M. W. C.,* . . . . . Amsterdam, Holland.
Gorrell, J. R., ... Newton, Iowa.
Goss, Isham Hamilton, ... Fort Lamar, Ga.
Goss, Ossian Wilbur, ... Lake Village, N. H.
Gottschaldt, Charles Martin, ... New York City.
Gould, Alfred Pearce,* . . . . . London, England.
Gould, John, ... Hatherleigh, England.
Gouley, John W. S., ... New York City.
Govan, William, ... Stony Point.
Gradle, Henry, ... Chicago, Ill.
Graefe, William, ... Sandusky, Ohio.
Graham, George W., ... Charlotte, N. C.
Graham, John W., ... Denver, Col.
Grancher, Dr.,* . . . . . Paris, France.
Grant, Gabriel, ... New York City.
Grant, George Franklin, ... Boston, Mass.
Grant, James Alexander, ... Ottawa, Canada.
Grant-Bey, James Andrew Sandilands, ... Cairo, Egypt.
Grant, William West, ... Davenport, Iowa.
Grauer, Frank, ... New York City.

Gravatt, Charles U.,* . . . . . .
Gravens, Dr.,* . . . . . Indianapolis, Ind.
Graves, Eli Edwin, ... Boscawen, N. H.
Gray, George,* . . . . . Castlewellan, Ireland.
Gray, Joseph Preston, ... Sedalia, Mo.
Gray, Richardson,* . . . . . Orange, N. J.
Gray, Robert, ... Armagh, Ireland.
Gray, R. H., ... Oneida, N. Y.
Gray, William Merrick, ... Washington, D. C.
Grayston, Frederick Samuel Cooper, ... Huntington, Ind.
Greathead, John Baldwin, ... Grahamstown, South Africa.
Green, John J., ... Pittsburgh, Pa.
Green, Robert Lee, ... Kansas City, Mo.
Green, Samuel Ellis, ... Newport, R. I.
Green, Stephen S., ... Buffalo, N. Y.
Green, Traill, ... Easton, Pa.
Green, William .C., ... St. Louis, Mo.
Greenawalt, George Leiter, ... Fort Wayne, Ind.
Greene, Joseph Chase, ... Buffalo, N. Y.
Greenish, Thomas,* . . . . . London, England.
Greenleaf, Charles Ravenscroft, ... Washington, D. C.
Greenley, Thomas Brady, ... West Point, Ky.
Greenshields, William, ... Romeo, Mich.
Gregory, E. H., ... St. Louis, Mo.
Grelat, Charles Edward, ... Boulogne, France.
Gretton, J. H., ... London, England.
Grevers, John E., ... Amsterdam, Holland.
Griffith, Francis Plummer, ... La Grange, Ind.
Griffith, Jefferson D., ... Kansas City, Mo.
Grim, Adam, ... Franklin Grove, Ill.
Grimes, John,* . . . . . Blackburn, England.
Grindall, Charles S., ... Baltimore, Md.
Grissom, Eugene, ... Raleigh, N. C.
Griswold, Elisha, ... Sharon, Pa.
Gritstrom, E.,* . . . . . Hermosands, Switzerland.
Groer, Frank, ... Warsaw, Poland.
Gross, Onan Bowman, ... Camden, N. J.
Grossman, Karl August,* . . . . . Liverpool, England.
Grubb, Alfred S., ... London, England.
Grube, G. W., ... Gallitzin, Pa.
Guhman, Nicholas, ... St. Louis, Mo.
Guilford, Simeon Hayden, ... Philadelphia, Pa.
Gundry, Richard, ... Catonsville, Md.
Gunn, Moses, ... Chicago, Ill.
Gunnell, Francis M., ... Washington, D. C.
Gunnell, Robert H., ... Washington, D. C.
Gusserow, M. D.,* . . . . . Berlin, Prussia.
Guye, Prof. A. A. G.,* . . . . . Amsterdam, Holland.

H

Hackett, Colin Johnson, ... Le Mars, Iowa.
Haddad, S.,* . . . . . Alexandria, Egypt.
Haggard, William David, ... Nashville, Tenn.
Hagner, Charles Evelyn, ... Washington, D. C.
Hagner, Daniel Randall, ... Washington, D. C.
Hale, James I. . . . . Anna, Ill.
Halke, J W ,* . . . . . London, England.
Hall, Arthur Joseph, ... Washington, D. C.
Hall, G. Stanley * . . . . . Baltimore, Md.
Hall, Junius Merwin, ... Chicago, Ill.
Hall, John C., ... Frankford, Pa.
Hall, John Randolph, ... Marshall, Mo.
Hall, Rufus B., ... Chillicothe, Ill.
Hall, Willis Woodbury, ... Springfield, Ohio.
Halladay Walter Marton Hampden, ... Sidney, Va.
Hallam, John Locker, ... Centralia, Ill.
Haller, Francis Boggs, ... Vandalia, Ill.
Halley, George, ... Kansas City, Mo.
Halliburton, William Dillard, ... Jacksonville, Mo.
Halliburton, William Dobinson, ... London, England.
Hallowell, Rebecca C., ... Atlantic City, N. J.
Hamilton, D. J.,* . . . . . Aberdeen, Scotland.
Hamilton, Hugh, ... Harrisburg, Pa.
Hamilton, John Brown, ... Washington, D. C.
Hamilton, John Waterman, ... Columbus, Ohio.
Hammett, Charles Massey, ... Washington, D. C.
Hammond, Henry L., ... Killingly Conn.
Hammond, Thomas V., ... Washington, D. C.
Hanawalt, George P., ... Des Moines, Iowa.
Hanbury, Jas. Arthur,* . . . . . London, England.
Hance, Samuel F . . . . . Minneapolis, Minn.
Hand, Daniel W.,* . . . . . St. Paul, Minn.
Handy William E., ... Washington, D. C.
Hanna, William M., ... Henderson Ky
Hannan, James C., ... Hoosac Falls, N. Y.
Hansmann, Theodore, ... Washington, D. C.
Harbon, Walter S., ... Washington, D. C.
Harbon, William T., ... Washington, D. C.
Harding, Thomas John Best, ... Brookville, Canada.
Harding, William Edward, ... Shrewsbury, England.
Hardman, Lamartine Griffin, ... Harmony Grove, Ga.
Hardy, A.,* . . . . . Paris, France.
Hargrave, William Ward, ... Hophton England.
Harker, Charles, ... Mount Holly, N. J.
Harlan, Alison Wright, ... Chicago, Ill.
Harman, Frank W., ... Carthage, Ohio.
Harris, Alexander, ... Jeffersonton, Va.
Harris, George M., ... Washington, D. C.
Harris, H. Crittenden, ... Philadelphia, Pa.
Harris, James H., ... Baltimore, Md.

Harris, John E., ... Bloomington, Ind.
Harris, Joseph Edwards, ... Nashville, Tenn.
Harrison, George Tucker, ... New York City.
Harrison, Henry Marshall, ... Bushnell, Ill.
Harrison, J. Stewart, ... Anacostia, D. C.
Harrison, Joseph, ... Sheffield, England.
Harrison, Reginald,* . . . . . Liverpool, England.
Harrison, William, ... Marshall, Mo.
Harroun, Chester Hannum, ... Toledo, Ohio.
Hart, Benjamin Franklin, ... Marietta, Ohio.
Hart, Hugh Allison, ... Wooster, Ohio.
Hart, John Isaac, ... New York City.
Hartigan, James French, ... Washington, D. C.
Hartley, John Davis, ... San Francisco, Cal.
Hartman, Jacob H., ... Baltimore, Md.
Hartmann, Robert,* . . . . . Potsdam, Germany.
Harvey, Henry,* . . . . . Liverpool, England.
Harvey, Herbert F., ... Cleveland, Ohio.
Harvey, Thomas Burges, ... Indianapolis, Ind.
Harvie, John B., ... Troy, N. Y.
Harvie, Lewis E., ... Danville, Va,
Harwood, Edward E., ... New York City.
Haskell, Loomis P., ... Chicago, Ill.
Hatch, James Henry, ... San Francisco, Cal.
Hatch, Thomas Edwards, ... Washington, D. C.
Hatchett, Buchanan, ... Fort Smith, Ark.
Hauben, Prof.,* . . . . . Brussels, Belgium.
Havard, Valery,* . . . . . New York City.
Haviland, Alfred,* . . . . . .
Hawkes, William Hines, ... Washington, D. C.
Hawkins, William Harrison, ... Texarkana, Ark.
Hay, Thomas, ... Philadelphia, Pa.
Hay, Walter, ... Chicago, Ill.
Hayd, H. E.,* . . . . . Buffalo, N. Y.
Hayes, Charles Addison, ... Chippewa Falls, Wis.
Hays, D. S., ... Hollidaysburg, Pa.
Hays, Edward Stephen, ... Eau Claire, Wis.
Hays, James Mackintosh, ... Oxford, N. C.
Haywood, Edmund Burke, ... Raleigh, N. C.
Hazen, David Henry . . . . . Washington, D. C.
Hazlewood, Arthur, ... Grand Rapids, Mich.
Hearne, Joseph Carter, ... Hannibal, Mo.
Heckscher, Joseph . . . . . Hamburg, Germany.
Heddens, James Weir, ... St. Joseph, Mo.
Hedges, William B., ... Delaware, Ohio.
Hedley, John, ... Middleborough, England.
Heerdink, John William, ... San Francisco, Cal.
Hegar, Dr.,* . . . . . Freiberg, Germany.
Heighway Archibald E., ... Cincinnati, Ohio.
Heish, George Kenton, ... Winchester, Va.
Heiss, Oscar Noble, ... Cincinnati, Ohio.
Heminway, Henry Bixby, ... Kalamazoo, Mich.

Henderson, George, ... Washington, D. C.
Hendricks, George A., ... Ann Arbor, Mich.
Hengst, David Alfred, ... Pittsburgh, Pa.
Henry, R. S., ... Charleston, W. Va.
Henry, Stewart L., ... New Orleans, La.
Hensman, Frank Henry, ... London, England.
Herbert, J. Fred., ... Philadelphia, Pa.
Herbert, J. Wells, ... Washington, D. C.
Herbert, Thomas, ... New Iberia, La.
Herdegen, Robert,* . . . . . Milwaukee, Wis.
Herdman, William James, ... Ann Arbor, Mich.
Hereford, Henry F., ... Kansas City, Mo.
Herff, Ferdinand, ... San Antonio, Tex.
Herrick, H. J., ... Cleveland, Ohio.
Herter, Erwin, ... Berlin, Germany.
Hertz, William Henry, ... Hazleton, Pa.
Hervey, James Walter, ... Indianapolis, Ind.
Hess, Frank Clifton, ... Cadiz, Ind.
Hesse, R.,* . . . . . Dresden, Germany.
Heustis, James Walter, ... Pittsburgh, Pa.
Hewetson, H. Bendelack,* . . . . . Leeds, England.
Hewitt, Charles M., ... Red Wing, Minn.
Hewitt, Graily, ... London, England.
Hewitt, Richard Newton, ... Evington, Va.
Hewlett, John C., ... Beckenham, England.
Hewson, Addinell, ... Philadelphia, Pa.
Heyl, Albert Gallatin, ... Philadelphia, Pa.
Hibberd, James Farquhar, ... Richmond, Ind.
Hickling, Daniel Percy, ... Washington, D. C.
Hickman, Napoleon, ... Philadelphia, Pa.
Hickman, Thomas Gerdeon, ... Vandalia, Ill.
Hill, Charles, ... Pine Island, Minn.
Hill, Charles Edwin, ... East Killingly, Conn.
Hill, Edgar Dwight, ... Plymouth, Mass.
Hill, Gershom H.,* . . . . . Independence, Iowa.
Hill, Robert I., ... St. Louis, Mo.
Hills, Garnett L., ... Washington, D. C.
Hills, Thomas Hyde, ... Cambridge, England.
Hills, Thomas O., ... Washington, D. C.
Hills, T. Morton, ... Willimantic, Conn.
Himes, Isaac Newton, ... Cleveland, Ohio.
Hinds, Clara Bliss, ... Washington, D. C.
Hines, William Franklin, ... Chestertown, Md.
Hingston, William H., ... Montreal, Canada.
Hinton, John Henry, ... New York City.
Hinze, V.,* . . . . . St. Petersburg, Russia.
Hipolite, W. W., ... Devall's Bluff, Ark.
Hirschberg, J.,* . . . . . Berlin, Germany.
Hirschfeld, G. Ludovic,* . . . . . Paris, France.
Hischspring, H.,* . . . . . Copenhagen, Denmark.
Hitchcock, Frank E., ... Rockland, Maine.
Hitchcock, Homer O., ... Kalamazoo, Mich.

Hjertstrom, Ernst, ... Hernosand, Sweden.
Hoadley, Albert Edward, ... Chicago, Ill.
Hobbs, Arthur G., ... Atlanta, Ga.
Hobby, Cicero Mead, ... Iowa City, Iowa.
Hodges, James Allison, ... Fayetteville, N. C.
Hodgkin, James Orlando, ... Warrenton, Va.
Hodgman, Abbott, ... New York City.
Hoff, Josiah Washington, ... Pomeroy, Ohio.
Hoffman, Joseph Henry, ... St. Mary's, Pa.
Hofheinz, Rudolph Henry, ... Rochester, N. Y.
Hogan, Samuel Mardis, ... Union Springs, Ala.
Hogg, Walter Douglass,* . . . . . Paris, France.
Holcombe, William Frederic, ... New York City.
Holden, Raymond T., ... Washington, D. C.
Hollenbeck, Orlando Jacob, ... Canandaigua, N. Y.
Hollister, John H., ... Chicago, Ill.
Holloway, James M., ... Louisville, Ky.
Holloway, John W., ... Keene, Ky.
Holly, James H., ... Warwick, N. Y.
Holmes, Charles, ... Washington, D. C.
Holt, Wm. Llewellyn, ... Macon, Ga.
Holton, Henry Dwight, ... Brattleboro, Vt.
Holyoke, Frank, ... Holyoke, Mass.
Homans, John, ... Boston, Mass.
Homen, E. A., ... Helsingfors, Finland.
Hood, Thomas B., ... Washington, D. C.
Hooper, Josephus, ... Louisville, Ky.
Hoover, N. M., ... North Hope, Pa.
Hopkins, Horace G., ... Willard, N. Y.
Hopkins, James Adam, ... Parkerville, Kansas.
Hopkins, Thomas Spaulding, ... Thomasville, Ga.
Hord, William T., ... Washington, D. C.
Horton, William Perry, ... Cleveland, Ohio.
Hotz, Ferdinand Carl, ... Chicago, Ill.
Hougberg, Emil, ... Helsingfors, Finland.
Hough, Garvy de Neaville, ... New Bedford, Mass.
Hough, Wayland S., ... Cuyahoga Falls, Ohio.
Houston, David Walker, ... Cohoes, N. Y.
How, Woodbury Stover, ... Philadelphia, Pa.
Howard, Joseph T., ... Washington, D. C.
Howard, John C., ... Washington, D. C.
Howard, Robert Early, ... Durant, Miss.
Howe, George, ... Columbia, S. C.
Howell, Fleming, ... Clarksburg, W. Va.
Howell, William M., ... Cogan's Station, Pa.
Howitt, Henry, ... Guelph, Canada.
Howland, Clarence Henry, ... Washington, D. C.
Hubbard, Alvin Allace, ... Buffalo, N. Y.
Hubbard, George Whipple, ... Nashville, Tenn.
Hubbard, Samuel Thomas, ... New York City.
Huckins, Payson Taylor, ... Los Angeles, Cal.
Hudson, Salmon, ... Medina, Ohio.

Hughes, Amos Jarman, ... Latour, Ark.
Hughes, Charles H., ... St. Louis, Mo.
Hughes, John W., ... Latrobe, Pa.
Hughes, Wallace Kirkwood, ... Berlin Centre, Ohio.
Hughes, William Todd, ... Bedford, Pa.
Hugo, Louis Charles Frederick, ... Washington, D. C.
Hull, George S., ... Chambersburg, Pa.
Hume, Edgar Enoch, ... Frankfort, Ky.
Humes, Mareen D., ... Beltsville, Md.
Hummel, A. L., ... Philadelphia, Pa.
Hunt, Alfred Onias, ... Iowa City, Iowa.
Hunt, Chas. Cummins, ... Dixon, Ill.
Hunt, E. K., ... Hartford, Conn.
Hunt, Ezra M., ... Trenton, N. J.
Hunt, Herman D., ... Preble, N. Y.
Hunt, Lebbeus Clark, ... Parkersburg, W. Va.
Hunt, Robert, ... Blackburn, England.
Hunt, Robert Finley, ... Washington, D. C.
Hunter, John W., ... Salem, N. C.
Huntington, William Clarke, ... Howell, Mich.
Hurd, Henry Mills, ... Pontiac, Mich.
Hurd, Henry S., ... Galesburg, Ill.
Hurry, Jamison Boyd, ... Reading, England.
Hurt, Charles Davis, ... Columbus, Ga.
Huselton, William Sterling, ... Allegheny City, Pa.
Husted, Nathaniel C., ... Tarrytown, N. Y.
Hutchings, Wm. Davis, ... Madison, Ind.
Hutchins, William Sheldon, ... Wheeling, W. Va.
Hutchinson, Jonathan,* . . . . . London, England.
Hutchinson, Woods, ... Des Moines, Iowa.
Hutton, Terry John, ... Fergus Falls, Minn.
Huxley, Frank E., ... Birmingham, England.
Hyatt, Benj. Franklin, ... Ottumwa, Iowa.
Hyatt, Franck, ... Washington, D. C.
Hyde, Frederick, ... Cortland, N. Y.

I

Iglehart, James Davidson, ... Baltimore, Md.
Ill, Edward Joseph, ... Newark, N. J.
Imlack, Thomas,* . . . . . Liverpool, England.
Ince, John, ... Farmingham, England.
Ingals, Ephraim Fletcher, ... Chicago, Ill.
Inge, Harry Tritwiler, ... Mobile, Ala.
Ingersoll, Denman Bevis, ... May's Landing, N. J.
Ingersoll, Luman Church, ... Keokuk, Iowa.
Ingram, Thomas Darlington, ... Washington, D. C.
Irion, John L., ... Montgomery, Texas.
Irish, John Carroll, ... Lowell, Mass.

Irwin, Fairfax, ... U. S. Marine Hospital Service.
Irwin, Thomas, ... Maberly, Mo.
Ish, Milton A., ... Neabsco Mills, Va.

J

Jackson, Abraham Reeves, ... Chicago, Ill.
Jackson, Edward, ... Philadelphia, Pa.
Jackson, Harry H., ... Detroit, Mich.
Jackson, John Henry, ... Fall River, Mass.
Jackson, John Wesley, ... Kansas City, Mo.
Jackson, Samuel K., ... Norfolk, Va.
Jackson, Victor H., ... New York City.
Jacobs, Christopher Columbus, ... Frostburg, Md.
Jacobson, Nathan, ... Syracuse, N. Y.
Jacobson, Sigismund Daniel, ... Chicago, Ill.
Jacobus, Arthur Middleton, ... New York City.
Jaggard, William Wright, ... Chicago, Ill.
Jamar, John Henry, ... Elkton, Md.
James, Henry, ... Waterbury, Vt.
James, Joseph Abraham, ... Bushnell, Ill.
Jamieson, J.,* . . . . . Edinburgh, Scotland.
Jamieson, W. Allen,* . . . . . Edinburgh, Scotland.
Jamison, John Stearns, ... Hornellsville, N. Y.
Janney, Charles Hamilton, ... Alexandria, Va.
Jarrett, Absalom M., ... Grafton, W. Va.
Jarvis, George C., ... Hartford, Conn.
Jatzow, Frederick Heinrich Carl Rudolph, ... Lubeck, Germany.
Jefferis, Charles Robinson, ... Wilmington, Del.
Jenison, Minot Gaylor, ... Minneapolis, Minn.
Jenkins, George Washington, ... Kilbourn City, Wis.
Jenkins, John F., ... Tecumseh, Mich.
Jenkins, Robert Hartsfield, ... Hoganville, Ga.
Jenks, Edward W.,* . . . . . Detroit, Mich.
Jennings, Charles Godwin, ... Detroit, Mich.
Jennings, R. G., ... Little Rock, Ark.
Jessett, Fred. B.,* . . . . . London, England.
Jewett, John H., ... Canandaigua, N. Y.
Johnson, Agnes M., ... Athens, Ohio.
Johnson, Francis M., ... Kansas City, Mo.
Johnson, George,* . . . . . London, England.
Johnson, Hosmer Allen, ... Chicago, Ill.
Johnson, John Elias, ... Lebanon Junction, Ky.
Johnson, Joseph Taber, ... Washington, D. C.
Johnson, Orville, ... Washington, Ohio.
Johnson, Thomas W., ... Danville, Ind.
Johnston, George Ben., ... Richmond, Va.
Johnston, James, ... Staunton, Va.
Johnston, William Adams, ... Peoria, Ill.
Johnston, W. E., ... Etna, Pa.

Johnston, Wm. Waring, ... Washington, D. C.
Johnstone, Albert Pope, ... Anderson, S. C.
Joll, Boyd Burnett, ... Liverpool, England.
Jones, Carmalt,* . . . . . London, England.
Jones, Charles Hyland, ... Baltimore, Md.
Jones, D. Edgar,* . . . . . Cardiff, Wales.
Jones, Daniel Wentworth, ... Portsmouth, N. H.
Jones, Evan,* . . . . . Aberdeen, S. Wales.
Jones, George Wheeler, ... Danville, Ill.
Jones, Henry Cabell, ... Richmond, Va.
Jones, Henry King, ... Parkersburg, W. Va.
Jones, John Curtis, ... Gonzales, Texas.
Jones, John Wesley, ... Tarboro, N. C.
Jones, Joseph, ... New Orleans, La.
Jones, Samuel J., ... Chicago, Ill.
Jones, Thomas W., ... Columbus, Ohio.
Jones, Toland, ... London, Ohio.
Jones, Victor Samuel, ... Bethlehem, Pa.
Jordan, John Lawrence, ... Bennettsville, S. C.
Jordan, Robin Merton, ... St. Louis, Mo.
Judd, Herbert, ... Galesburg, Ill.
Judson, Adoniram Brown, ... New York City.
Jüler, Henry Cundell, ... Cincinnati, Ohio.
Jump, David W., ... Plainfield, Ill.

K

Kaiser, Augustus, ... Detroit, Mich.
Kaposi, Mariz,* . . . . . Wien, Austria.
Kartulis, Dr.,* . . . . . Alexandria, Egypt.
Kauffman, J. S., ... Blue Island, Ill.
Kaufmann, C., ... Zürich, Switzerland.
Kay, Thos. W.,* . . . . . Beyrout, Syria.
Keables, Thomas Ash, ... Wilmington, Del.
Keen, Thomas Farron, ... Hamilton, Va.
Keith, Thos.,* . . . . . Edinburgh, Scotland.
Keller, James M., ... Hot Springs, Ark.
Keller, Luther H., ... Luray, Va.
Kelley, C. W.,* . . . . . Louisville, Ky.
Kelley, Samuel W.,* . . . . . Cleveland, Ohio.
Kellogg, J. H., ... Battle Creek, Mich.
Kells, Charles Edmund, ... New Orleans, La.
Kelly, Emil, ... Liverpool, England.
Kelly, James Edward, ... New York City.
Kemp, W. Frederick A., ... Baltimore, Md.
Kemper, General William Harrison, ... Muncie, Ind.
Kemper, Philip A., ... Mattoon, Ill.
Kendall, Henry Wm., ... Quincy, Ill.
Kendall, James Emory, ... Parkersburg, W. Va.

Kengla, Louis A., ... Kansas City, Mo.
Kennedy, Charles Ulrich, ... Washington, D. C.
Kerr, E. E., ... Chicago, Ill.
Kerr, James, ... Winnipeg, Manitoba, Canada.
Keser, Samuel, ... Paris, France.
Ketchum, George A., ... Mobile, Ala.
Keutisch, R. H.,* . . . . . Hamburg, Germany.
Keyser, Peter Derick, ... Philadelphia, Pa.
Kidd, Geo. H.,* . . . . . Dublin, Ireland.
Killaugh, David S., ... Galveston, Texas.
Kimball, Gilman, ... Lowell, Mass.
Kimmel, Jacob A., ... Findlay, Ohio.
Kindleberger, David, ... Washington, D. C.
King, Albert F. A., ... Washington, D. C.
King, Edmund Eleazer, ... Toronto, Canada.
King, James K., ... Clifton Springs, N. Y.
King, George Weirs, ... Mayresville, Montana Ter.
Kingsley, Byron F., ... San Antonio, Texas.
Kinloch, Robert Alexander, ... Charleston, S. C.
Kirch, Jules,* . . . . . Rignano, Italy.
Kirk, Samuel T., ... Kokomo, Ind.
Kisch, E. H.,* . . . . . Prague, Austria.
Kittenger, Martin S., ... Lockport, N. Y.
Kleen, Emil, ... Stockholm, Sweden.
Kleinschmidt, Carl H. A., ... Washington, D. C.
Kline, William Jno. K., ... Greensburg, Pa.
Klingensmith, Israel Putnam, ... Blairsville, Pa.
Klotz, Herman G., ... New York City.
Knaggs, Henry Valentine, ... London, England.
Knapp, Charles, ... Evansville, Ind.
Knapp, James Rollo, ... New Orleans, La
Knapp, William M., ... Lincoln, Neb.
Knight, Amos, ... Eaton Rapids, Mich.
Knight, James, ... New York City.
Knight, S. R., ... Philadelphia, Pa.
Knight, William, ... Cincinnati, Ohio.
Knod, Hans, ... Prusse, Germany
Knode, Robert S., ... Fort Wayne, Ind
Knott, John M., ... Sioux City, Iowa.
Knowles, Samuel E., ... San Francisco, Cal.
Knowles, William A., ... San Francisco, Cal.
Knox, James Suydam, ... Chicago, Ill.
Koch, Paul,* . . . . . Luxembourg, France.
Kocher, Dr.,* . . . . . Berne, Switzerland.
Koehler, Ferdinand, ... Pittsburgh, Pa.
Kölicker, Dr.,* . . . . . Zürich, Switzerland.
Körosi, Joseph, ... Buda Pesth, Hungary.
Koser, Simon Schmucker, ... Williamsport, Pa.
Kramer, Thomas Best, ... Washington, D. C.
Krause, W.,* . . . . . Göttingen, Germany.
Kreider, George N., ... Springfield, Ill.
Kretzschmar, Paul H., ... Brooklyn, N. Y.

Kucher, Joseph, ... New York City.
Kuehn, Rolando, ... Lahaima, South W. Island.
Kuhn, George, ... Paris, France.
Kulp, Joseph S., ... Muscatine, Iowa.
Kulp, William Overholt, ... Davenport, Iowa.

L

Lablé, Léon,* . . . . . France.
Laborderie-Dubosquet, Dr.,* . . . . . Paris, France.
Lache, Hans,* . . . . . Zehlendorf, Germany.
Ladd, Charles W., ... Cannelton, Ind.
Laehr, Hans, ... Schweizerhof, Germany.
Lafetra, George Henry, ... Washington, D. C.
Lalor, Wm. S., ... Trenton, N. J.
Lamadrid, Julio J., ... Brooklyn, N. Y.
Lamb, Daniel Smith, ... Washington, D. C.
Lamb, George E., ... Port Henry, N. Y.
Lamb, Theodore, ... Augusta, Ga.
Lambert, Jorden W., ... St. Louis, Mo.
Lambuth, Wm. Wesley, ... Sonora, Ky.
Lancaster, Francis Matthews, ... Wayside, Md.
Landan, Leopold,* . . . . . Berlin, Germany.
Landan, Newell Egbert, ... Newark, N. Y.
Landolt, Edmund, ... Paris, France.
Landon, David M., ... Burton, Ill.
Lane, Carr, ... St. Louis, Mo.
Langdon, F. W.,* . . . . . Cincinnati, Ohio.
Lange, F., ... New York City.
Lange, Victor,* . . . . . Kjobenhaon, Denmark.
Langfelt, W. J., ... Allegheny City, Pa.
Langridge, George Thomas, ... Woolwich, England.
Lankford, Livius, ... Bowers, Va.
Larimore, Frank Carter, ... Mt. Vernon, Ohio.
La Roche, William Tell, ... New York City.
Larrabee, John Albert, ... Louisville, Ky.
Lashells, Theodore B., ... Meadville, Pa.
Lassar, Oscar,* . . . . . Berlin, Germany.
Latham, Henry Grey, ... Lynchburg, Va.
Latham, Peter Haverman, ... Weatherly, Pa.
Lathrop, Joseph, ... Detroit, Mich.
Lathrop, Moses Craft, ... Dover, N. H.
Latimer, Charles M. N., ... Washington, D. C.
Latimer, Thomas Sargent, ... Baltimore, Md.
Laton, Winfield S., ... Minneapolis, Minn.
Lautenbach, Louis J., ... Philadelphia, Pa.
Lawrence, Alfred Edward Aust., ... Bristol, England.
Lawrence, Humphrey,* . . . . . Cambridge, England.
Lay, H. Brooke,* . . . . . Manchester, England.
Lazarewitch, Dr.,* . . . . . St. Petersburg, Russia.

Leach, Hamilton Evans, ... Washington, D. C.
Leach, John Comyns,* . . . . . Sturminster, England.
Leake, Elgin Kossuth, ... Colliersville, Tenn.
Leale, Charles Augustus, ... New York City.
Leaning, John Kenington, ... Fly Creek, N. Y.
Leavell, Nancy M., ... St. Louis, Mo.
Le Baron, Robert, ... Pontiac, Mich.
Lee, Alfred Howland, ... Washington, D. C.
Lee, Benjamin, ... Philadelphia, Pa.
Lee, Frederick H., ... Auburn, N. Y.
Lee, Millard Fillmore, ... Columbus, Ohio.
Lee, W. Augustus, ... Richmond, Va.
Leeds, Albert,* . . . . . Hoboken, N. J.
Leeds, Lucian Lavassa, ... Lincoln, Ill.
Lefebvre, Gustav, ... Paris, France.
Le Fort, Leon, ... Paris, France.
Leigh, Alfred, ... Calvin Run, Va.
Leighton, Alton Winslow, ... New Haven, Conn.
Leighton, Nathaniel Wilson, ... Brooklyn, N. Y.
Leishman, William, ... Glasgow, Scotland.
Leloir, H.,* . . . . . Lille, France.
Le Monnier, Ives Rene, ... New Orleans, La.
Le Moyne, Francis, ... Pittsburgh, Pa.
Lenoir, Benjamin Ballard, ... Lenoir, Tenn.
Leonard, Benjamin F., ... Baltimore, Md.
Leonard, C. Henri, ... Detroit, Mich.
Leonard, Rensselaer, ... Mauch Chunk, Pa.
Leopold, Prof.,* . . . . . Dresden, Germany.
Leprohou, Jean Lukin, ... Montreal, Canada.
Leslie, James, ... Cincinnati, Ohio.
Leslie, James, ... Hamilton, Canada.
Lesser, Adolph M., ... New York City.
Lester, Elias, ... Seneca Falls, N. Y.
Lester, Frank Harvey, ... Skaneateles, N. Y.
Lester, Thomas Bryan, ... Kansas City, Mo.
Letts, James P., ... Romeo, Mich.
Leuf, A. U. P., ... Philadelphia, Pa.
Lewes, James, ... Burlington, Vermont.
Lewin, L., ... Berlin, Prussia.
Lewis, Denslow, ... Chicago, Ill.
Lewis, Edwin Rufus, ... Crawfordsville, Ind.
Lewis, Eugene R., ... Kansas City, Mo.
Lewis, Frank L., ... Green Bay, Wis.
Lewis, James Hall, ... Washington, D. C.
Lewis, Theodore G., ... Buffalo, N. Y.
Lichliter, David Conrad, ... Dayton, Ohio.
Lichty, Daniel, ... Rockford, Ill.
Liell, E. N.,* . . . . . New York City.
Limberg, Alexander Chas., ... St. Petersburg, Russia.
Lincoln, Nathan Smith, ... Washington, D. C.
Lindsay, William S., ... Topeka, Kansas.
Lindsley, John Berrien, ... Nashville, Tenn.

Lineaweaver, John Krause, ... Columbia, Pa.
Link, John Ephraim, ... Terre Haute, Ind.
Linthicum, D. A., ... Helena, Ark.
Littig, James Band, ... New York City.
Livingston, Thomas Moore, ... Columbia, Pa.
Lloyd, William Harris, ... London, England.
Lockard, Daniel Harrison, ... Plymouth, Pa.
Lockhart, Daniel Harvie, ... Chappel Hill, Texas.
Loewenberg, B.,* . . . . . Paris, France.
Lofthouse, Richard Chapman,* . . . . . London, England.
Logan, Samuel, ... New Orleans, La.
Logan, Thomas, ... Bradford, England.
Lombard, J. N.,* . . . . . Geneva, Switzerland.
Long, William, ... Washington, D. C.
Longmore, Thomas,* . . . . . Netley, England.
Longstreth, Miers Fisher, ... Sharon Hill, Pa.
Lorini, Raphael, ... Washington, D. C.
Lowell, William Hughes, ... Lancaster, Pa.
Lowrance, Henry Alexander, ... Athens, Ga.
Love, Isaac Newton, ... St. Louis, Mo.
Love, Wm. Abraham, ... Atlanta, Ga.
Lovejoy, James W. H., ... Washington, D. C.
Luckett, Wm. Fleet, ... Washington, D. C.
Luckey, Benjamin Franklin, ... Paterson, N. J.
Luckie, Robert Kerr, ... Holly Springs, Miss.
Luckie, Samuel Blair, ... Chester, Pa.
Ludington, Horace, ... Omaha, Neb.
Ludwig, Roscoe F., ... Chicago, Ill.
Lumell, E. H.,* . . . . . Norwich, Conn.
Lundgran, C. E., ... St. Paul, Minn.
Lundy, C. J.,* . . . . . Detroit, Mich.
Lusk, William Thompson, ... New York City.
Luteaud, A.,* . . . . . Paris, France.
Lutz, Frank Joseph, ... St. Louis, Mo.
Lynch, John S., ... Baltimore, Md.

M

McAlister, Alexander, ... Camden, N. J.
McAlpin, Archie, ... Bradford, Pa.
McArthur, David S., ... La Crosse, Wis.
McCahey, Peter, ... Philadelphia, Pa.
McCausey, George H., ... Janesville, Wis.
McChord, Robert Caldwell, ... Lebanon, Ky.
McClellan, Ezra Spencer, ... Paterson, N. J.
McCluer, Benjamin, ... Dubuque, Iowa.
McClurg, John R., ... West Chester, Pa.
McColl, Hugh, ... Lapeer, Mich.
McCollum, William, ... Brooklyn, N. Y.
McComas, Josiah Lee, ... Oakland, Md.

McConaughy, Robert, ... York, Neb.
McCorkle, George Baxter, ... Covington, Va.
McCormack, Joseph N., ... Bowling Green, Ky.
McCoy, John C., ... Los Angeles, Cal.
McCurdy, Stewart L., ... Dennison, Ohio.
McDavitt, Thomas S., ... Winona, Minn.
McDonald, Calvin D., ... Kansas City, Mo.
McDonald, Carlos F., ... Auburn, N. Y.
McDonald, John A.,*. . . . . London, England.
McDowall, John Greig, ... Wadsley, England.
McElhaney, George W., ... Columbus, Ga.
MacEwen, Wm.,* . . . . . Glasgow, Scotland.
McEwen, William W., ... Mound Valley, Kansas.
McFadyen, Archibald, ... Chico, Cal.
McFall, David Miller, ... Mattoon, Ill.
McFarlan, Daniel, ... Washington, D. C.
MacGeagh, Thos. Edwin Foster, ... London, England.
McGill, Samuel, ... Leesburg, Va.
MacGillavry, Thomas H., ... Leiden, Holland.
MacGregor, Donald, ... London, England.
MacGuire, Constantine J., ... New York City.
McGuire, J. Clark, ... Louisville, Ky.
McGrew, Henry, ... Pleasant Ridge, Ohio.
McHatton, Henry, ... Macon, Ga.
McIntosh, Lyman Durkee, ... Chicago, Ill.
McIntosh, W. Page, ... New Orleans, La.
McIntyre, John H., ... St. Louis, Mo.
McKay, Read Jennings, ... Wilmington, Del.
McKee, Edward Sydney . . . . . Cincinnati, Ohio.
McKellops, Henry James, ... St. Louis, Mo.
McKennan, F. M. T.,* . . . . . Pittsburgh, Pa.
McKenzie, Wm. Robert, ... Chester, Ill.
MacKinnon, Wm. Alex.,* . . . . . London, England.
McKnew, Wilber Richmond, ... Baltimore, Md.
McLain, John S., ... Washington, D. C.
McLaughlin, James Wharton, ... Austin, Texas.
McLaughlin, Thomas N., ... Washington, D. C.
McLean, Thomas Ferguson, ... Goderich, Canada.
McLennan, Nignhart,* . . . . . Liverpool, England.
McLeod, Samuel B. W., ... New York City.
McManus, James, ... Hartford, Conn.
McMillen, Drury J., ... Kansas City, Mo.
McMillen, John Warren, ... Columbus, Ohio.
McMurtry, Louis S., ... Danville, Ky.
McNeil, George Washington, ... Pittsburgh, Pa.
McNeill, James William, ... Fayetteville, N. C.
McNutt, H. E., ... Aberdeen, Dakota Territory.
McNutt, W. F., ... San Francisco, Cal.
McPhatter, N. Lincoln, ... Guelph, Canada.
McReynolds, John O., ... Elkton, Ky.
McShane, Augustus, ... New Orleans, La.
McSwegan, Daniel, ... San Diego, Cal.

McWilliam, J. A.,* . . . . . Aberdeen, Scotland.
Mabon, Thomas, ... Allegheny City, Pa.
MacCallum, Duncan C., ... Montreal, Canada.
Mackall, Louis, ... Washington, D. C.
Mackellar, C. K., ... Sydney, Australia.
Mackenzie, John Noland, ... Baltimore, Md.
Mackern, George,* . . . . . Buenos Ayres.
Maclean, Donald, ... Detroit, Mich.
Macleod, W. Bowman,* . . . . . Edinburgh, Scotland.
Macrae, Donald, ... Council Bluffs, Iowa.
Maddin, Thomas Lafayette, ... Nashville, Tenn.
Made, Karl, ... Vienna, Austria.
Madison, Benjamin Franklin, ... Washington, D. C.
Madison, Frank Maitland, ... Chicago, Ill.
Magill, Donald Douglass, ... Erie, Pa.
Magitot, M.,* . . . . . Paris, France.
Magnin, Dr.,* . . . . . Paris, France.
Magruder, George Lloyd, ... Washington, D. C.
Maiquez, Emiliano, ... Haverstraw, N. Y.
Maiston, Jeffrey A.,* . . . . . Liverpool, England.
Mallam, Charles Edward, ... Washington, D. C.
Mallan, Thomas Francis, ... Washington, D. C.
Mallett, William P., ... Chapel Hill, N. C.
Mandevill, F. A., ... Rochester, N. Y.
Manley, Thomas H., ... New York City.
Mann, Edward C., . * . . . . . Brooklyn, N. Y.
Mannaway, Lucus W. W., ... Jacksonville, Fla.
Manning, Wm. Price, ... Washington, D. C.
Manolescu, N., ... Bucarest, Roumania.
Mansell, Edward Rosser, ... Hastings, England.
Mansfield, A. S.,* . . . . . Ashland, Neb.
Manton, Walter Park, ... Detroit, Mich.
Mapother, E. D.,* . . . . . Dublin, Ireland.
Marchand, James Irwin, ... Irwin, Pa.
Marcy, Henry Orlando, ... Boston, Mass.
Marin, Francisco, ... Mexican Legation, . . . Washington, D. C.
Marlow, Frank William, ... Syracuse, N. Y.
Marmion, William V., ... Washington, D. C.
Marquardt, Carl Heinrich, ... Lacrosse, Wis.
Marsh, James Thomas, ... Liberty, Mo.
Marshall, John S., ... Chicago, Ill.
Marshall, John Schenck, ... Green Spring, Ohio.
Marshall, Milton Culbertson, ... Little Rock, Ark.
Marshall, William, ... Milford, Del.
Marston, Jeffery Allen, ... London, England.
Martens, Christian, ... Hadersleben, Germany.
Martin, August, ... Berlin, Prussia.
Martin, Christopher C., ... Shreveport, La.
Martin, Franklin H., ... Chicago, Ill.
Martin, Hugh McD., ... Fredericksburg, Va.
Martin, James Cooper, ... Wyandotte City, Kansas.
Martin, Rawley White, ... Chatham, Va.

Martin, Sidney H. C.,* . . . . . London, England.
Martine, Godfrey Richard, ... Glens Falls, N. Y.
Marx, George, ... Washington, D. C.
Mason, George Wilcoxon, ... Bloomington, Ill.
Mason, John Edwin, ... Manchester, N. H.
Massei, F.,* . . . . . Naples, Italy.
Matteson, Arthur E., ... Chicago, Ill.
Matthes, Albert, ... Tilsit, Germany.
Matthews, John E., ... Kenansville, N. C.
Matthews, John P., ... Carlinville, Ill.
Matthews, Josiah Wright, ... Cape Town, South Africa.
Mathews, Joseph M., ... Louisville, Ky.
Mathiot, Edward B., ... West Newton, Pa.
Matox, Frank W., ... London, England.
Mattison, J. B.,* . . . . . Brooklyn, N. Y.
Maury, Richard B., ... Memphis, Tenn.
Maus, L. M., ... Fort Schuyler, N. Y.
Maxey, Frederic Edward, ... Saco, Me.
Maxwell, George Troup, ... Orange Lake, Fla.
Maxwell, James D., ... Bloomington, Ind.
May, William, ... Washington, D. C.
Mayer, Jr., Orlando B., ... Newbury, S. C.
Maynard, Edward, ... Washington, D. C.
Mayo, William W., ... Rochester, Minn.
Mead, John Ames, ... Pearlington, Miss.
Mead, Theodore, ... Washington, D. C.
Mead, William Bradley, ... Providence, R. I.
Medwin, A. G.,* . . . . . London, England.
Megalhaes, Lemos,* . . . . . Oporto, Portugal.
Meisenbach, Albert H., ... St. Louis, Mo.
Mendel, Emanuel, ... Berlin, Germany.
Menees, Orville Harrison, ... Nashville, Tenn.
Meneorre, Dr.,* . . . . . Rio de Janeiro, Brazil.
Meng, Edwin R., ... Dover, Mo.
Meng, John William, ... Lexington, Mo.
Méniére, P.,* . . . . . Paris, France.
Mercer, Charles A., ... Richmond, Va.
Mercer, William Mercer, ... Pittsfield, Mass.
Meriam, Ephraim C., ... Washington, D. C.
Meriam, Horatio Cook, ... Salem, Mass.
Merriam, Edwin H., ... Coxsackie, N. Y.
Merrill, Frederic Augustus, ... Boston, Mass.
Merriman, George, ... Bucyrus, Ohio.
Messemer, Michael J. B., ... New York City.
Metcalf, Abraham Tolles, ... Kalamazoo, Mich.
Metnitz, Joseph, ... Vienna, Austria.
Mewborn, Ala. Duke, ... Memphis, Tenn.
Meyer, Leopold,* . . . . . Copenhagen, Denmark.
Meyer, Wilhelm,* . . . . . Copenhagen, Denmark.
Michel, Middleton,* . . . . . Charleston, S. C.
Mickle, William Julius, ... London, England.
Middelkamp, Henry Hermann, ... Warrenton, Mo.

Middleton, William Drummond, ... Davenport, Iowa.
Miles, Albert B., ... New Orleans, La.
Miller, Abner Myers, ... Bird-in-Hand, Pa.
Miller, Albert Eber, ... Needham, Mass.
Miller, Carroll Everard, ... Cadillac, Mich.
Miller, DeLaskie, ... Chicago, Ill.
Miller, Enoch Hutchinson, ... Liberty, Mo.
Miller, Francis H., ... Brooklyn, N. Y.
Miller, Herman W., ... Chicago, Ill.
Miller, Jacob Preston, ... Buckhannon, W. Va.
Miller, John, ... Andover, N. J.
Miller, J. E., ... Pittsburgh, Pa.
Miller, John F., ... Goldsboro, N. C.
Miller, Joseph S., ... York, Pa.
Miller, Oliver Leaird, ... Allegheny City, Pa.
Miller, Vesta Delphine, ... Needham, Mass.
Millikin, Daniel, ... Hamilton, Ohio.
Mills, Robert James, ... Norwich, England.
Mills, William Albert, ... Baltimore, Md.
Milnamow, John Thaddeus, ... Central Park, Ill.
Milner, Uriel Richardson, ... New Orleans, La.
Milroy, William Forsythe, ... Omaha, Neb.
Milton, Samuel A., ... Clinton, Mo.
Minich, Andrew K., ... Philadelphia, Pa.
Minney, J. E.,* . . . . . Topeka, Kan.
Minor, S. L.,* . . . . . Memphis, Tenn.
Miranda, Ramon Levis, ... New York City.
Mitchell, Arthur,* . . . . . Edinburgh, Scotland.
Mitchell, Edwin Waterman, ... Cincinnati, Ohio.
Mitchell, Howard E., ... Troy, N. Y.
Mitchell, R. B.,* . . . . . .
Mittendorf, William F., ... New York City.
Mobeck, Ejlert Edward, ... Jonkoping, Sweden.
Mocentkowski, T.,* . . . . . Odessa, Russia.
Moise, Giam Bettini, ... New York City.
Moncorvo, Dr.,* . . . . . Rio de Janeiro.
Monosmith, George Washington, ... Kipton, Ohio.
Montabet, John,* . . . . . Cairo, Egypt,
Monte, Alvis,* . . . . . Vienna, Austria.
Montgomery, Edward E., ... Philadelphia, Pa.
Montgomery, Liston Horner, ... Chicago, Ill.
Mooney, Fletcher D., ... St. Louis, Mo.
Moore, David Newlin, ... Decatur, Ill.
Moore, Edward Charles, ... Detroit, Mich.
Moore, Edward Mott, ... Rochester, N. Y.
Moore, Edwin W., ... Franklin, Pa.
Moore, Enoch Walker, ... Decatur, Ill.
Moore, Harris Lindsay, ... Cincinnati, Ohio.
Moore, John, ... Washington, D. C.
Moore, J. Hall, ... Richmond, Va.
Moore, John Wm.,* . . . . . Dublin, Ireland.
Moore-Madden, Thomas,* . . . . . Dublin, Ireland.

Moore, Thomas J., ... Richmond, Va.
Moore, Thomas Thompson, ... Columbia, S. C.
Moore, Vincent Howard, ... Brockville, Canada.
Moore, Wm. N., ... Covelo, Cal.
Mooren, Albert, ... Dusseldorf, Germany.
Morales, Rafael, ... San José, Central America.
Moran, John F., ... Washington, D. C.
Morgan, Edwin Lee, ... Washington, D. C.
Morgan, Ethelbert Carroll, ... Washington, D. C.
Morgan, Frederick, ... Lambscott, England.
Morgan, James Ethelbert, ... Washington, D. C.
Morgan, William Henry, ... Nashville, Tenn.
Morisiana, A.,* . . . . . Naples, Italy.
Morris, George B., ... Morgantown, W. Va.
Morris, Jas. Cheston, ... Philadelphia, Pa.
Morris, Robert Tuttle, ... New York City.
Morrison, Alexander, ... Kilmarnock, Scotland.
Morrison, Samuel Brown, ... Rockbridge Baths, Va.
Morrison, William Henry, ... Holmesburg, Pa.
Morrison, William Newton, ... St. Louis, Mo.
Morse, John Wesley, ... Mount Pleasant, N. C.
Morton, Douglas, ... Louisville, Ky.
Morton, Edward I. C., ... Chicago, Ill.
Morton, Thomas G., ... Philadelphia, Pa.
Mosby, J. J., ... Culpeper, Va.
Moses, Gratz Ashe, ... St. Louis, Mo.
Mott, Armstead Randolph, ... Leesburg, Va.
Mound, Thomas, ... Rutland, Vt.
Moura, Dr.,* . . . . . Rio Janeiro.
Moura, F.,* . . . . . Paris, France.
Moyer, Frank G., ... Baltimore, Md.
Moyer, Harold N.,* . . . . . Chicago, Ill.
Mudd, Henry H., ... St. Louis, Mo.
Mulheron, John Jolleffe, ... Detroit, Mich.
Müller, Otto,* . . . . . Blankenburg, Germany.
Müller, Peter,* . . . . . Berne, Switzerland,
Mules, P. H.,* . . . . . Manchester, England.
Mulligan, Edward W., ... Rochester, N. Y.
Mummery, John Howard, ... London, England.
Muncaster, Magruder, ... Washington, D. C.
Muncaster, Stewart Brown, ... Washington, D. C.
Mundell, John Hodges, ... Washington, D. C.
Munn, John P., ... New York City.
Munroe, Grafton, ... Annapolis, Md.
Munson, George S.,* . . . . . Albany, N. Y.
Murphy, John B., ... Chicago, Ill.
Murphy, John Joseph, ... Dublin, Ireland.
Murphy, P. J., ... Washington, D. C.
Murray, T. Morris, ... Washington, D. C.
Murrell, T. E., ... Little Rock, Ark.
Murrell, William, ... London, England.
Muscroft, Sr., C. S., ... Cincinnati, Ohio.

Musser, Charles Sumner, ... Aaronsburg, Pa.
Myers, Eliab, ... Springfield, Ohio.
Myers, Everett C., ... Wheeling, W. Va.
Myers, William H., ... Fort Wayne, Ind.

N

Nadaillac, M. C. Marquise,* . . . . . Paris, France.
Nanyu, Dr.,* . . . . . Königsberg, Prussia.
Nardyz, Mark Leonard, ... Philadelphia, Pa.
Nash, Alfred, ... Joliet, Ill.
Nash, Benjamin Charles, ... New York City.
Nash, Francis Smith, ... Washington, D. C.
Nash, Herbert Milton, ... Norfolk, Va.
Neely, John Thomas, ... Washington, D. C.
Neff, John, ... Baltimore, Md.
Neftel, William B.,* . . . . . Chicago, Ill.
Nelson, Daniel E., ... Chattanooga, Tenn.
Nelson, Daniel Thurber, ... Chicago, Ill.
Nelson, Samuel Newell, ... Boston, Mass.
Neudörfer, Dr.,* . . . . . Wien, Austria.
Neville, Thomas, ... London, England.
Newby, James Broadhead, ... St. Louis, Mo.
Newland, Benjamin, ... Bedford, Ind.
Newlin, John W.,* . . . . . Nashville, Tenn.
Newman, Henry Parker, ... Chicago, Ill.
Newman, Henry Martel, ... Washington, D. C.
Newman, Jesse Pittman, ... Tarborough, N. C.
Newman, Robert, ... New York City.
Newmann, Isador,* . . . . . Wien, Austria.
Newton, Lewis Edgar, ... Washington, D. C.
Nichols, Charles Henry, ... New York City.
Nichols, Stella B., ... Davenport, Iowa.
Nickson, William Perrion,* . . . . . Atlanta, Ga.
Nihiser, Winton Markwood, ... Keedysville, Md.
Niles, Edward S., ... Boston, Mass.
Noble, Henry Bliss, ... Washington, D. C.
Noble, Jr., Henry Bliss, ... Washington, D. C.
Noel, Llewellyn G., ... Nashville, Tenn.
Nolen, James George, ... Toledo, Ohio.
Norris, William H., ... Baltimore, Md.
North, John, ... Keokuk, Iowa.
Northrop, Aaron Lockwood, ... New York City.
Northrup, W. P.,* . . . . . New York City.
Norton, Arthur Trehern,* . . . . . London, England.
Norton, George S., ... New York City.
Norton, Thomas Marshall, ... Washington, D. C.
Nott, Thomas Heath, ... Goliad, Texas.
Nowlin, John Bryan Ward, ... Nashville, Tenn.
Nunn, Richard Joseph, ... Savannah, Ga.

O

Oatman, Ira Ellis, ... Sacramento, Cal.
Ober, George Clarke, ... Washington, D. C.
O'Brien, John Newton, ... Milwaukee, Wis.
O'Conner, Frank J., ... Washington, D. C.
O'Daniel, Wm., ... Bullards, Ga.
O'Dwyer, J.,* . . . . . New York City.
Ogden, Uzziel, ... Toronto, Canada.
Ogg, Robert Taylor, ... Kansas City, Mo.
O'Hagan, Charles James, ... Greenville, N. C.
O'Hara, Michael, ... Philadelphia, Pa.
Ohmann-Dumesnil, A. H.,* . . . . . St. Louis, Mo.
Oldendorf, A.,* . . . . . Berlin, Prussia.
Oliphant, John, ... Leicester, England.
Oliven, Albert, ... New York City.
Oliven, Ludwig, ... New York City.
Oliver, James Peter, ... Caldwell, Texas.
Ollier, M.,* . . . . . Lyons, France.
Olmstead, Edwin B., ... Washington, D. C.
Olmsted, Ingersoll, ... Hamilton, Canada.
O'Neal, J. W. C., ... Gettysburg, Pa.
Opie, Thomas, ... Baltimore, Md.
Oppenheimer, Henry Semon, ... New York City.
Oppenheimer, William Tell, ... Richmond, Va.
O'Reilly, Charles, ... Toronto, Canada.
O'Reilly, Robert Maitland, ... Washington, D. C.
Orme, Henry Sayre, ... Los Angeles, Cal.
Orth, Henry L., ... Harrisburg, Pa.
Orton, John G., ... Binghamton, N. Y.
Orwin, Arthur W.,* . . . . . London, England.
Osborne, Harris Burnett, ... Kalamazoo, Mich.
O'Toole, M. C.,* . . . . . San Francisco, Cal.
Ottilie, Charles, ... Lacrosse, Wis.
Otto, Ernst, ... Munich, Bavaria.
Ottofy, Louis, ... Chicago, Ill.
Ouacheé, Charles, ... Paris, France.
Ouchterlony, John A., ... Louisville, Ky.
Ourt, Andrew Jackson, ... Philadelphia, Pa.
Owen, Abraham Miconious, ... Evansville, Ind.
Owen, Isambard,* . . . . . London, England.
Owen, William Ottway, ... Lynchburg, Va.
Owens, Edmund, ... London, England.
Owens, John Edwin, ... Chicago, Ill.
Oxley, Martin G. B.,* . . . . . Liverpool, England.
Oyler, Philip H., ... Mount Pulaski, Ill.

P

Pace, Jesse M., ... Dallas, Texas.
Paddack, Henry Clay, ... New York City.
Page, Richard Channing Moore, ... New York City.
Page, Washburn Eddy, ... Boston, Mass.
Paine, John F. Y., ... Galveston, Texas.
Palmer, Alonzo B., ... Ann Arbor, Mich.
Palmer, Edgar, ... Lacrosse, Mich.
Palmer, E. R.,* . . . . . Louisville, Ky.
Palmer, Gideon Stinson, ... Washington, D. C.
Palmer, Henry, ... Janesville, Wis.
Palmer, James Gregory, ... New Brunswick, N. J.
Palmer, Stewart Bailey, ... Syracuse, N. Y.
Pancoast, William H., ... Philadelphia, Pa.
Park, George, ... Sioux City, Iowa.
Park, John Walton, ... Williamstown, Pa.
Park, Roswell, ... Buffalo, N. Y.
Parke, Charles Ross, ... Bloomington, Ill.
Parker, James Monroe, ... Davenport, Iowa.
Parker, Moses Greeley, ... Lowell, Mass.
Parker, Roger, ... Liverpool, England.
Parker, William Thornton, ... Newport, R. I.
Parkes, Charles Theodore, ... Chicago, Ill.
Parkinson, Charles Henry, ... Wimborne, England.
Parr, Henry Albert, ... New York City.
Parrish, Joseph, ... Burlington, N. J.
Parsons, Horace Warren, ... Wamego, Kansas.
Parsons, John William, ... Portsmouth, N. H.
Parsons, Mary Almera, ... Washington, D. C.
Parsons, Starr, ... Washington, D. C.
Paschal, Franklin, ... Chihuahua, Mexico.
Pasquàli, E.,* . . . . . Rome, Italy.
Patrick, J. Joseph Ravenscroft, ... Belleville, Ind.
Pattee, Asa Flanders, ... Boston, Mass.
Patten, Nathaniel Newton, ... Monongahela City, Pa.
Patterson, Albert Clark, ... Washington, D. C.
Patterson, DeWitt Clinton, ... Washington, D. C.
Patterson, John Deans, ... Kansas City, Mo.
Pavy, Frederick William, ... London, England.
Pawleck, Karl,* . . . . . Vienna, Austria.
Payne, Frank H., ... Berkeley, Cal.
Payne, Joseph Frank, ... London, England.
Peabody, James Henry, ... Omaha, Neb.
Péan, Dr.,* . . . . . Paris, France.
Pearce, Enoch, ... Steubenville, Ohio.
Peck, George, ... Elizabeth, N. J.
Peck, Solomon H., ... Ithaca, N. Y.
Peck, Washington Freeman, ... Davenport, Iowa.
Pemberthy, Joseph W., ... Minneapolis, Minn.
Pennepacker, Henry, ... Scranton, Pa.
Perez, G. V.,* . . . . . Orotava, Canary Islands.

Perkins, Jabez, ... Owosso, Mich.
Perroud, Dr.,* . . . . . Lyons, France.
Perruzzi, Domenico,* . . . . . Bologna, Italy.
Perry, George N., ... Washington, D. C.
Peter, Armistead, ... Washington, D. C.
Peters, Ford Sylvester, ... Washington, D. C.
Peterson, Francis Marion, ... Greensboro', Ala.
Petit, Leon,* . . . . . Paris, France.
Pfeiffer, Oscar J., ... Denver, Col.
Philips, Edgar L., ... Galesburg, Ill.
Philips, J. Willoughby, ... Washington, Pa.
Phillips, Albanus M., ... Battle Creek, Mich.
Phillips, Chas. D. F., ... London, England.
Phillips, George A., ... Ellsworth, Me.
Phillips, Thomas H., ... Canton, Ohio.
Philp, William, ... Hamilton, Canada.
Philpott, Ferdinand Bruce, ... Salisbury, Mo.
Picard, Henri,* . . . . . Paris, France.
Pickel, John Ulrich, ... Baltimore, Md.
Pickett, Manhattan, ... Corry, Pa.
Pickup, John Walwork, ... Brockville, Canada.
Picot, Louis Julian, ... Littleton, N. C.
Pierce, F. M.,* . . . . . Manchester, England.
Pilcher, James E., ... New York City.
Pilcher, Lewis Stephen, ... Brooklyn, N. Y.
Pine, Oran Steadman, ... Aberdeen, Dakota.
Pinkerton, Thos. H., ... Oakland, Cal.
Pinney, Charles H., ... Derby, Conn.
Pitman, John, ... Kirkwood, Mo.
Pitner, Thomas Jefferson, ... Jacksonville, Ill.
Pitres, Dr.,* . . . . . Bordeaux, France.
Pitts, Barton, ... St. Joseph, Mo.
Planck, Milton G., ... Schenectady, N. Y.
Plessner, Louis, ... Bay City, Mich.
Plunket, James D.,* . . . . . Nashville, Tenn.
Pocock, Eli Dudley, ... Shreve, Ohio.
Pollock, Alexander McCandless, ... Pittsburgh, Pa.
Pollok, Robert, ... Glasgow, Scotland
Pomerene, Peter Peirsol, ... Berlin, Ohio.
Poole, Thomas Wesley, ... Lindsay, Ontario, Canada.
Pope, Bolling Anthony, ... Dallas, Texas.
Porcher, F. Peyre, ... Charleston, S. C.
Porre, Richard Johnson, ... Cincinnati, Ohio.
Porter, George Loring, ... Bridgeport, Conn.
Porter, Henry Norton, ... Washington, D. C.
Porter, John, ... McKeesport, Pa.
Porter, Robert Henry, ... Louisville, Ky.
Porter, Robert J., ... Youngsville, N. C.
Porter, William, ... St. Louis, Mo.
Post, George Edward, ... Beirut, Syria.
Postley, Charles E., ... Washington, D. C.
Potter, William W.,* . . . . . Buffalo, N. Y.

Poussie, Emile, ... Paris, France.
Pouzet, Paul, ... Cannes, France.
Powell, Lionel L., ... Melton Mowbry, England.
Powell, Robert C., ... Alexandria, Va.
Powell, Robert Wyngard, ... Roden Station, Ottawa, Canada.
Powell, Thomas K., ... Dancyville, Tenn.
Powell, Thomas Spencer, ... Atlanta, Ga.
Power, Henry, ... London, England.
Pozzi, Samuel,* . . . . . Paris, France.
Pradire, Dr.,* . . . . . Lyons, France.
Prater, William Warren, ... Sheffield, Ala.
Pratt, John Richmond, ... Manchester, N. Y.
Prentice, Frederick William, ... Greensborough, Md.
Prentiss, Daniel W., ... Washington, D. C.
Presbrey, Silas Dean, ... Taunton, Mass.
Preston, Byron Isaac, ... Rochester, N. Y.
Preston, Geo. M., ... Lynchburg, Va.
Prewitt, James Harvey, ... Madisonville, Ky.
Price, Anselm D., ... Harrodsburgh, Ky.
Price, Jacob,* . . . . . West Chester, Pa.
Price, Sherman B., ... New York City.
Price, Theophilus T., ... Tuckerton, N. J.
Pride, Joseph Peebles, ... Pride's Station, Ala.
Priest, Albert N., ... Utica, N. Y.
Priest, Stephen Curtis, ... Newark, Ohio.
Prince, A. E., ... Jacksonville, Ill.
Prince, David, ... Jacksonville, Ill.
Prion, P. L., ...
Pritchard, Urban,* . . . . . London, England.
Probst, Charles O, ... Columbus, Ohio.
Proctor, Edwin Gover, ... Kane, Ill.
Prosser, Abel James, ... St. Louis, Mo.
Provins, Clark Braeling, ... Kissimee, Florida.
Pruyn, Charles Putman, ... Chicago, Ill.
Purcell, F. A.,* . . . . . Manchester, England.
Purple, S. S., ... New York City.
Purves, Laidlaw,* . . . . . London, England.
Putney, Lorenzo S., ... Sturgis, Mich.
Pye-Smith, Philip H.,* . . . . . London, England.
Pyles, Richard Albert, ... Anacostia, D. C.

Q

Quimby, Isaac N., ... Jersey City, N. J.

R

Radcliffe, Samuel J., ... Washington, D. C.
Rae, Francis, ... Oshawa, Canada.
Rahter, Charles Augustus, ... Harrisburg, Pa.
Ramsey, A. C. Lamothe, ... St. Cloud, Minn.
Ramsey, Robert W., ... St. Thomas, Pa.
Randall, Jr., Edward, ... Galveston, Texas.
Randolph, Robert Lee, ... Baltimore, Md.
Rankin, David Nevin, ... Allegheny, Pa.
Ranney, Ambrose Loomis, ... New York City.
Rariden, Samuel Angell, ... Bedford, Ind.
Rathgen, Herman, ... Hamburg, Germany.
Rauch, John H., ... Springfield, Ill.
Ravogli, Augustus, ... Cincinnati, Ohio.
Rawlins, John Windsor, ... Washington, D. C.
Rawls, Albert Oliver, ... Lexington, Ky.
Rawson, Allen Abel, ... Corning, Iowa.
Ray, P. W., ... Brooklyn, N. Y.
Rea, John, ... New Castle, Ind.
Read, Louis W., ... Norristown, Pa.
Read, Thomas George, ... London, England.
Récamier, Joseph, ... Paris, France.
Records, Frank, ... Philadelphia, Pa.
Redardo, Paul, ... Paris, France.
Redick, Samuel Tate, ... Allegheny City, Pa.
Redlick, Henry, ... Chicago, Ill.
Redman, Spence, ... Platte City, Mo.
Redmond, N.,* . . . . . Dublin, Ireland.
Redrow, Isaac, ... Williamsburg, Ohio.
Reece, Madison, ... Abingdon, Ill.
Reed, Caleb R., ... Middleport, Ohio.
Reed, George Washington, ... Belpre, Ohio.
Reed, Robert Hawey, ... Mansfield, Ohio.
Reed, Thomas Jefferson, ... Washington, D. C.
Reeve, James T., ... Appleton, Wis.
Regis, Emanuel,* . . . . . Bordeaux, France.
Rehwinkel, Frederic Henry, ... Chillicothe, Ohio.
Reid, William London, ... Glasgow, Scotland.
Reid, William L.,* . . . . . Liverpool, England.
Reinvann, H.,* . . . . . Kief, Russia.
Remandino, Peter Charles, ... San Diego, Cal.
Remington, Frank Antoine, ... New York City.
Rennolds, Henry Thomas, ... Baltimore, Md.
Rettich, Hugo, ... New York City.
Reyburn, Robert, ... Washington, D. C.
Reyher, Chas., ... St. Petersburg, Russia.
Reynolds, Dudley Sharpe, ... Louisville, Ky.
Reynolds, Henry James, ... Chicago, Ill.
Reynolds, Lawrence, ... Victor, Iowa.
Rhein, Meyer Louis, ... New York City.
Rheinfrank, John Henry, ... Perrysburg, Ohio.

Rhett, Jr., Robert Barnwell, ... Charleston, S. C.
Rhodes, John Neeley, ... Philadelphia, Pa.
Rich, Cyrus F., ... Saratoga Springs, N. Y.
Richards, C. H., ... Georgetown, Del.
Richards, William Henry, ... Knoxville, Tenn.
Richards, William M., ... Joliet, Ill.
Richards, William Molton Leland, ... Philadelphia, Pa.
Richardson, Alonzo B., ... Athens, Ohio.
Richardson, Benj. Ward,* . . . . . London, England.
Richardson, Charles Boards, ... Aldrington, England.
Richardson, Edward, ... Louisville, Ky.
Richardson, Horace, ... Boston, Mass.
Richardson, Maurice Howe, ... Boston, Mass.
Richardson, Tobias G., ... New Orleans, La.
Richardson, William Lee, ... Montrose, Pa.
Richey, Stephen Olin, ... Washington, D. C.
Rickert, William, ... Baltimore, Md.
Ricketts, B. Merrill, ... Cincinnati, Ohio.
Ricketts, Edwin Saunders, ... Portsmouth, Ohio.
Riddelle, Philip S., ... Washington, D. C.
Ridenour, Albert Wellington, ... Massillon, Ohio.
Rider, William H., ... Danbury, Conn.
Ridge, Isaac M., ... Kansas City, Mo.
Ridge, James Marshall, ... Camden, N. J.
Rieger, Joel Henry, ... Kansas City, Mo.
Riffe, John M., ... Covington, Ky.
Riggs, Elliott Swift, ... Allegheny City, Pa.
Ritchie, James,* . . . . . Munkirk, N. B.
Ritchie, Louis W., ... Washington, D. C.
Ritchie, Park, ... St. Paul, Minn.
Ritchey, John Alexander, ... Oil City, Pa.
Robb, William H., ... Amsterdam, N. Y.
Robbins, Harry A., ... Washington, D. C.
Roberts, Edmund H.,* . . . . . Rochester, England.
Roberts, Leonidas Gossette, ... Eureka, Ark.
Roberts, Lloyd, ... Manchester, England.
Roberts, Milton Josiah, ... New York City.
Roberts, Oscar S., ... Pittsfield, Mass.
Roberts, Robert, ... Portmadoc, North Wales.
Roberts, William E., ... Washington, D. C.
Robertson, John Worcester,* . . . . . Napa, Cal.
Robinson, Andrew Rose, ... New York City.
Robinson, G. R. B., ... Sharpsburg, Pa.
Robinson, William Lavaille, ... Danville, Va.
Robison, Jas. D., ... Wooster, Ohio.
Roche, Martin, ... Philadelphia, Pa.
Rockwell, A. D.,* . . . . . New York City.
Rockwell, La Rue D., ... Union City, Pa.
Roe, John O., ... Rochester, N. Y.
Rogers, Carolyn S., ... Rochester, N. Y.
Rogers, W. B.,* . . . . . Memphis, Tenn.
Rohe, George H., ... Baltimore, Md.

Rohland, Charles B., ... Alton, Ill.
Rohlfing, C. H. F., ... St. Louis, Mo.
Rokitanski, Karl,* . . . . . Vienna, Austria.
Roller, William C., ... Hollidaysburg, Pa.
Rooker, James Iddings, ... Castleton, Ind.
Rosenbach, O.,* . . . . . Breslau, Germany.
Rosenthal, Isaac Max, ... Fort Wayne, Ind.
Rosenthal, M.,* . . . . . Vienna, Austria.
Ross, Alexander M., ... Toronto, Canada.
Ross, Frank Ward, ... Elmira, N. Y.
Ross, J. Frank, ... Clarion, Pa.
Ross, Samuel McNutt, ... Altoona, Pa.
Ross, William Henry, ... New York City.
Rosse, Irving C., ... Washington, D. C.
Roudebush, Daniel Winter, ... Covington, Ky.
Rouse, Rolla Edward, ... Brighton, England.
Rousseau, George Mozea, ... Montgomery, Ala.
Roy, Philip Seddon, ... Washington, D. C.
Ruckard, Rabe,* . . . . . .
Rudolphia, Theophil, ... Dowagiac, Mich.
Rugg, David Fletcher, ... Hartland, Vt.
Ruggles, Augustus Dennett, ... New York City.
Rumbold, Thomas F., ... St. Louis, Mo.
Rush, Edward Warren, ... Paris, Tex.
Russ, Eben James, ... St. Mary's, Pa.
Russell, Frederick William, ... Winchendon, Mass.
Russell, Ira,* . . . . . Winchendon, Mass.
Russell, Thomas Pember, ... Oshkosh, Wis.
Rust, David Newton, ... Alexandria, Va.
Rust, Edgar Rose, ... Washington, D. C.
Ruth, Melancthon Love, ... Washington, D. C.
Rutherford, Wm.,* . . . . . Edinburgh, Scotland.
Rutledge, Shallus R., ... Blairsville, Pa.
Rutter, F.,* . . . . . Montreal, Canada.
Rutter, H. C., ... Bellefontaine, Ohio.
Ryburn, John S., ... Ottawa, Ill.

S

Sabine, Andrew, ... Garden City, Kan.
Saiki, Ruchiro, ... Tokio, Japan.
Sale, Eugene Paul, ... Aberdeen, Miss.
Salisbury Co. Med. Society (by F. B. Philpott), . . Missouri.
Salmon, D. E.,* . . . . . Washington, D. C.
Salmon, Ira A., ... Boston, Mass.
Salomon, Godfrey S., ... Chicago, Ill.
Sanborn, Christopher Allen, ... Newport, N. H.
Sanders, Charity A., ... Ottawa, Ill.
Sandwith, F. M., ... Cairo, Egypt.
Sanger, M.,* . . . . . Leipsic, Germany.

Santoire, Samuel, ... Brooklyn, N. Y.
Satterwhite, Thomas P., ... Louisville, Ky.
Saunders, Dudley Dunn, ... Memphis, Tenn.
Savage, G. C.,* . . . . . Nashville, Tenn.
Savage, George H., ... Sandwith, Eng.
Sawyer, Pascal Hall, ... Cleveland, Ohio.
Saxer, Leonard A., ... Syracuse, N. Y.
Sayre, Lewis Albert, ... New York City.
Sayre, Lewis Hall, ... New York City.
Sayre, Reginald Hall, ... New York City.
Scarff, J. H., ... Baltimore, Md.
Scheafer, Frederick Christian, ... Chicago, Ill.
Scheaffer, Edward Martin, ... Washington, D. C.
Schanta, Prof.,* . . . . . Janspruck, Austria.
Schauffer, Edward William, ... Kansas City, Mo.
Schell, Walker, ... Spencer, Ind.
Schenck, W. L., ... Osage City, Kan.
Schmitz, George,* . . . . . Cologne, Germany.
Schmitzler, John,* . . . . . Vienna, Austria.
Schneck, Jacob, ... Mt. Carmel, Ill.
Schnee, A.,* . . . . . Nice, France.
Schoales, Joseph D., ... Philadelphia, Pa.
Schultz, Solomon S., ... Danville, Pa.
Schurig, Edmund,* . . . . . Dresden, Germany.
Schwartz, H.,* . . . . . Halle, Germany.
Schwartz, Henry, ... St. Louis, Mo.
Schwarze, Alfred Bernhardt Paul, ... Germany.
Schwimmer, E.,* . . . . . Buda Pesth, Hungary.
Scott, Andrew Jackson, ... Loudonville, Ohio.
Scott, A. Victoria, ... Philadelphia, Pa.
Scott, Charles W., ... Kansas City, Mo.
Scott, Preston B., ... Louisville, Ky.
Scott, Thomas A., ... Chicago, Ill.
Scott, William Johnson, ... Cleveland, Ohio.
Seabury, F. N., ... Providence, R. I.
Seals, Richmond Daniel, ... Fort Smith, Ark.
Seaman, Marinus Willett, ... Shipman, Ill.
Seaton, Edward, ... London, England.
Sedgwick, William Hall, ... Granville, Ohio.
Seifriz, Paul, ... Washington, D. C.
Seiler, Carl,* . . . . . Philadelphia, Pa.
Seiler, George, ... Alma, Wis.
Selden, Robert, ... Catskill, N. Y.
Sellhausen, Earnest Augustus, ... Washington, D. C.
Sellman, Wm. Alfred Belt, ... Baltimore, Md.
Seltzer, Charles Meck, ... Philadelphia, Pa.
Semeleder, F.,* . . . . . City of Mexico.
Semmola, M., ... Naples, Italy.
Semple, John, ... Wilkinsburg, Pa.
Senn, Nicholas, ... Milwaukee, Wis.
Servais, D. Leopold, ... Antwerp, Belgium.
Sewall, Henry,* . . . . . Ann Arbor, Mich.

Seymour, William Pierce, ... Troy, N. Y.
Shadle, Jacob Evans, ... Shenandoah, Pa.
Shakespeare, Edward Oram, ... Philadelphia, Pa.
Sharer, John P., ... Little Falls, N. Y.
Sharkey, Seymour J.,* . . . . . London, England.
Sharp, Henry J., ... London, Ohio.
Sharp, W. H.,* . . . . . Volcano, W. Va.
Shaw, Edwin B., ... Osage City, Kansas.
Shearer, James Y., ... Sinking Springs, Pa.
Shearer, Juliet Gambrill, ... Washington, D. C.
Shee, Yow Jiar, China, ... (Chinese Legation), Washington, D. C.
Shepard, Charles Henry, ... Brooklyn, N. Y.
Shepard, Edward Taylor, ... New Orleans, La.
Shepard, Luther Dimmick, ... Boston, Mass.
Sheppard, Wm. J., ... Cleveland, Ohio.
Sheridan, John Campbell, ... Johnstown, Pa.
Sherman, Fletcher J., ... Lyons, N. Y.
Shertzer, A. Trego, ... Baltimore, Md.
Sherwood, Alfred C., ... Union City, Pa.
Sherwood, Thomas H., ... Washington, D. C.
Shields, Charles M., ... Richmond, Va.
Shillito, George M., ... Allegheny, Pa.
Shirley, John Smith, ... Clarion, Pa.
Shoemaker, John V., ... Philadelphia, Pa.
Shoemaker, William P., ... Bradford, Pa.
Short, William H., ... LaGrange, Ind.
Shrady, John, ... New York City.
Shriver, Francis M., ... Glenwood, Iowa.
Shriver, Henry W., ... Red Oak, Iowa.
Shulze, William Heister, ... Atchison, Kansas.
Shurley, Ernest Lorenzo, ... Detroit, Mich.
Sibbett, Robert Lowry, ... Carlisle, Pa.
Sibley, Amos Harrington, ... Philadelphia, Pa.
Sidney, Austin Wilbur, ... Fitchburg, Mass.
Sieveking, Edward,* . . . . . London, England.
Sim, Frank L., ... Memphis, Tenn.
Simon, John,* . . . . . London, England.
Simon, Jules,* . . . . . Paris, France.
Simpson, A. R., ... Edinburgh, Scotland.
Simpson, John Crayke, ... Washington, D. C.
Simpson, Theodore Parker, ... Beaver Falls, Pa.
Simpson, William Curtis, ... New Brighton, Pa.
Sims, H. Marion,* . . . . . New York City.
Sinclair, Alexander Grant, ... Memphis, Tenn.
Sinclair, John Grant, ... Nashville, Tenn.
Sisler, Lewis, ... Clinton, Ohio.
Sitherwood, George Dallas, ... Bloomington, Ill.
Sjöberg, Ernst Gustaf Konstantin, ... Stockholm, Sweden.
Skeer, John D., ... Chicago, Ill.
Skilling, Wm. Quail, ... Lonaconing, Md.
Skillman, Henry Martin, ... Lexington, Ky.
Skinner, J. O., ... Washington, D. C.

Slater, Catherine B., ... Aurora, Ill.
Slaughter, Robert Madison, ... Theological Seminary, Va.
Sloan, Alfred B., ... Kansas City, Mo.
Sloan, William Blyth, ... Ontario, Canada.
Slocum, Charles Elihu, ... Defiance, Ohio.
Slocum, Jr., Charles Elihu, ... El Paso, Ill.
Smart, Charles, ... Washington, D. C.
Smith, Alcius T., ... Minneapolis, Minn.
Smith, Algernon Wood, ... Glasgow, Scotland.
Smith, Andrew Kingsbury, ... West Point, N. Y.
Smith, B. Hally, ... Baltimore, Md.
Smith, Charles M., ... Evansville, Wis.
Smith, David Thomas, ... Louisville, Ky.
Smith, Eugene, ... Detroit, Mich.
Smith, Eustace,* . . . . . London, England.
Smith, Ferdinand, ... Frankford, Mo.
Smith, Frank F., ... St. Augustine, Florida.
Smith, H., ... Norfolk, Va.
Smith, Henry Alexander, ... Cincinnati, Ohio.
Smith, Henry Hadley, ... Hudson, N. Y.
Smith, Henry Hollingsworth, ... Philadelphia, Pa.
Smith, Jerome Candee, ... Baltimore, Md.
Smith, J. Lewis, ... New York City.
Smith, Joel Washington, ... Charles City, Iowa.
Smith, John Milton, ... New Philadelphia, Ohio.
Smith, Joseph D., ... St. Joseph, Mo.
Smith, Joseph Henry, ... New Haven, Conn.
Smith, Joseph R., ... U. S. Army.
Smith, Laurence Adkinson, ... Port Gibson, Miss.
Smith, Marvin Eli, ... Chicago, Ill.
Smith, Noble,* . . . . . London, England.
Smith, Robert Shingleton, ... Bristol, England.
Smith, Robert Wallace Bruce, ... Ontario, Canada.
Smith, Samuel W., ... New York City.
Smith, Stephen, ... New York City.
Smith, Theobald, ... Washington, D. C.
Smith, Thomas C., ... Washington, D. C.
Smith, Wood,*. . . . . Glasgow, Scotland.
Smith, W. Wilberforce,* . . . . . London, England.
Smolt, C. F., ... Nickerson, Kansas.
Smouse, David W., ... Des Moines, Iowa.
Snively, Andrew J., ... Hanover, Pa.
Snively, Whitmore, ... Pittsburgh, Pa.
Snook, Jerome Marvine, ... Kalamazoo, Mich.
Snowden, Arthur, ... Washington, D. C.
Snowden, John Wiegand, ... Hammonton, N. J.
Snyder, Arthur Augustine, ... Washington, D. C.
Snyder, Daniel J., ... Scio, Ohio.
Solly, S. E.,* . . . . . London, England.
Sothoron, James T., ... Washington, D. C.
Souchon, Edward,* . . . . . New Orleans, La.
Sowers, James Richard, ... Warrenton, Va.

Spackman, Mary Dora, ... Washington, D. C.
Spanton, W. Dunnett,* . . . . . Hanley, England.
Spaulding, Wm. Augustus, ... Minneapolis, Minn.
Spear, John Wesley, ... Mason City, Ill.
Speer, Anderson T., ... Newark, Ohio.
Spencer, Ezra R., ... Doylestown, Ohio.
Spencer, William Crayton, ... Howell, Mich.
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Spinks, E. E., ... Meridian, Miss.
Spitzka, Edward C., ... New York City.
Sprague, William B., ... Detroit, Mich.
Sprengel, Otto, ... Dresden, Germany.
Sprinkel, George Alsop, ... Culpeper, Va.
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Stackpole, Paul A., ... Dover, N. H.
Stafford, John Joseph, ... Washington, D. C.
Stamm, M., ... Fremont, Ohio.
Stanton, Joshua Otis, ... Washington, D. C.
Staples, Franklin, ... Winona, Minn.
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Staples, George S., ... Sherman, Texas.
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Starr, Eli T., ... Philadelphia, Pa.
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St. Clair, R. O., ... Washington, D. C.
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Steele, Daniel A. K., ... Chicago, Ill.
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Steinke, Carl Otto Herman, ... Brooklyn, N. Y.
Steinmetz, Wm. Rudolph, ... Baltimore, Md.
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Stern, Max J.,* . . . . . Philadelphia, Pa.
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Stevens, Stephen Gifford, ... Boston, Mass.
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Stewart, James L., ... Erie, Pa.
Stewart, James, ... Montreal, Canada.
Stewart, Robert Wray, ... Pittsburgh, Pa.
Stewart, William Christy, ... Elmira, N. Y.

Stewart, William Shaw, ... Philadelphia.
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Stimson, Charles W., ... New York City.
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Stockman, Ralph, ... Edinburgh, Scotland.
Stockton, Charles S., ... Newark, N. J.
Stockton, Frank O., ... Chicago, Ill.
Stone, Alexander J., ... St. Paul, Minn.
Stone, Edgar C., ... Galesburg, Ill.
Stone, Isaac Scott, ... Lincoln, Va.
Stoner, George W., ... Washington, D. C.
Storer, Humphrey R., ... Newport, R. I.
Storey, John C., ... Dallas, Texas.
Storrs, Melancthon, ... Hartford, Conn.
Stout, Melancthon, ... Chicago, Ill.
Stout, S. H.,* . . . . . Cisco, Tex.
Stowell, James Herbert, ... Chicago, Ill.
Stowell, Sidney S., ... Pittsfield, Mass.
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Stringer, Sheldon, ... Brooksville, Fla.
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Struebens, Alf.,* . . . . . Brussels, Belgium.
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Stubbs, George Eastman, ... Philadelphia, Pa.
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Sullivan, Michael, ... Kingston, Canada.
Sullivan, N.,* . . . . . Kingston, Canada.
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Swasey, Erastus P., ... New Britain. Conn.
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T

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Tyler, Lachlan, ... Washington, D. C.
Tyler, W. Bowie, ... Washington, D. C.
Tyner, Thomas Jefferson, ... Austin, Texas.

U

Unna, P. G., ... Hamburg, Germany.
Upshur, John N., ... Richmond, Va.

V

Valk, Francis, ... New York City.
Valliet, Dr.,* . . . . . Geneva, Switzerland.
Vallin, Prof.,* . . . . . Paris, France.
Van Amringe, J. Howard, ... New York City.
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Van Horne, Augustus Knapp, ... Jerseyville, Ill.
Van Reypen, William Knickerbocker, ... U. S. Navy.
Van Valzah, H. B., ... Clearfield, Pa.
Van Valzah, Robert W., ... Terre Haute, Ind.
Van Velsor, J. B., ... Yankton, Dakota.
Varian, William, ... Titusville, Pa.
Varick, Theodore Romeyn, ... Jersey City, N. J.
Vastine, Jacob H., ... Catawissa, Pa.
Vaughan, Bolivar A., ... Columbus, Miss.
Vaughan, John Wesley, ... St. Louis, Mo.
Vaughan, Victor C., ... Ann Arbor, Mich.
Vedder, Wentworth D., ... Mansfield, Pa.
Veit, J.,* . . . . . Berlin, Germany.
Venn, Gustav, ... Bad Driburg, Westphalia, Ger.
Verbryck, George G., ... Meshoppen, Pa.
Vertrees, Charles M., ... Murrayville, Ill.
Villari, Eugenio, ... Baronissi, Italy.
Vogel, Alfred,* . . . . . Munich, Bavaria.
Von Campe, Hans, ... Hanover, Prussia.

Von Coler, Dr.,* . . . . . Berlin, Germany.
Von Farkas, Ladislaus, ... Buda Pesth, Hungary.
Von Hartleben, Carl August, ... Washington, D. C.
Von Hebra, Hans, ... Wien, Austria.
Von Klein, Carl H., ... Dayton, Ohio.
Von Wachtel, August R., ... Prague, Austria.
Voorhees, Charles Holbert, ... New Brunswick, N. J.

W

Wade, De Witt Clinton, ... Holly, Mich.
Wadsworth, Hiram Nichols, ... Washington, D. C.
Waggoner, Joseph, ... Ravenna, Ohio.
Wagner, Effingham, ... Montgomery, Ala.
Wagner, Lewis C., ... Nicholasville, Ky.
Waid, Jason T., ... Ridgway, Pa.
Wainwright, William A. M., ... Hartford, Conn.
Waite, Henry Edward, ... New York City.
Waldeyer, W.,* . . . . . Berlin, Germany.
Wales, Theron Augustus, ... Elmira, N. Y.
Walfender, R. Morris,* . . . . . London, England.
Walker, Alonzo Byron, ... Canton, Ohio.
Walker, Benjamin Maitland, ... Danville, Va.
Walker, Daniel R., ... Reese's Mills, Ind.
Walker, George, ... Birkenhead, England.
Walker, H. E., ... Hamilton, Va.
Walker, Joseph, ... London, England.
Walker, William Wallace, ... New York City.
Wall, John Perry, ... Tampa, Fla.
Wallace, W. B., ... New York City.
Wallian, Samuel Spencer, ... New York City.
Walsh, Ralph, ... Washington, D. C.
Walshe, Walter H.,* . . . . . London, England.
Walter, John, ... Washington, D. C.
Walter, Pye,* . . . . . London, England.
Walton, J. Clarence, ... Howell, Mich.
Walton, Joseph Richardson, ... Washington, D. C.
Walton, Thomas Cameron, ... Annapolis, Md.
Warburg, John R., ... Hamburg, Germany.
Ward, John Jackson, ... Ellenville, N. Y.
Ward, Milo B., ... Topeka, Kansas.
Ward, Richard Halsted, ... Troy, N. Y.
Ward, William, ... Washington, D. C.
Warden, Charles,* . . . . . Birmingham, England.
Warder, William H., ... Philadelphia, Pa.
Wardlow, William C., ... Augusta, Ga.
Wardner, Horace, ... Anna, Ill.
Warman, William H. H., ... Washington, D. C.
Warner, Francis,* . . . . . London, England.
Wassal, Joseph William, ... Chicago, Ill.

Waterman, Luther D., ... Indianapolis, Ind.
Waters, A. F. H.,* . . . . . Liverpool, England.
Waters, Thomas Solens, ... Baltimore, Md.
Wathen, William H., ... Louisville, Ky.
Watkins, Claibourne, ... Little Rock, Ark.
Watkins, John, ... Granville, Ohio.
Watkins, John Lindsay, ... Nashville, Tenn.
Watkins, Samuel Charles G., ... Montclair, N. J.
Watraszewski, X., ... Warsaw, Poland.
Watson, A.,* . . . . . London, England.
Watson, Beriah A., ... Jersey City, N. J.
Watson, David, ... Bellefontaine, Ohio.
Watson, William, ... Dubuque, Iowa.
Watson, William Seth, ... Matteawan, N. Y.
Watts, Samuel Roger, ... Washington, D. C.
Waugh, William Francis, ... Philadelphia, Pa.
Waxham, Frank E., ... Chicago, Ill.
Way, Joe Howell, ... Waynesville, N. C.
Wear, Isaac N., ... Fargo, Dakota.
Weaver, James Montfort, ... Dayton, Ohio.
Weaver, Joseph K., ... Norristown, Pa.
Webb, Clarence W., ... Wellsboro, Pa.
Weber, Charles,* . . . . . Hamburg, Germany.
Wedgewood, Joseph James, ... London, England.
Weed, Frank J., ... Cleveland, Ohio.
Weeks, Oliver W., ... Marion, Ohio.
Weeks, Stephen H., ... Portland, Me.
Weeks, Thomas Edwin, ... Minneapolis, Minn.
Weidman, W. Murray, ... Reading, Pa.
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Werler, Oscar Frederick Louis, ... College Point, N. Y.
Wernich, A., ... Cöslin, Germany.
Wesseler, Frederick W., ... St. Louis, Mo.
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West, Hamilton Atchison, ... Galveston, Texas.
West, Thomas H., ... Keyser, W. Va.
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White, John Verner, ... Oscoda, Mich.
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Wiehle, Carl Adolph Max, ... Philadelphia, Pa.
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Wilder, Flavius Myron, ... Chicago, Ill.
Wildermuth, H. A., ... Wittenberg, Germany.
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Wilkins, John A., ... Delta, Ohio.
Wilkinson, B. M.,* . . . . . Baltimore, Md.
Willard, DeForest, ... Philadelphia, Pa.
Wille, L.,* . . . . . Basle, Switzerland.
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Williams, John,* . . . . . London, England.
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Williams, Roger, ... Pittsburgh, Pa.
Williams, Salathiel T., ... Kendallville, Ind.
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Williamson, William Alan, ... Topeka, Kan.
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Wilson, Clarence G., ... St. Mary's, Pa.
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Wilson, Israel P., ... Burlington, Iowa.
Wilson, James,* . . . . . Liverpool, England.
Wilson, James Espy, ... Pittsburgh, Pa.
Wilson, Jefferson Henry, ... Beaver, Pa.
Wilson, John Harpin, ... Chicago, Ill.

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Wilson, William H., ... Gastonia, N. C.
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Winger, Frederick Wesley, ... Rew City, Pa.
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Wise, Peter M., ... Willard, N. Y.
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Wolfe, Emanuel Miller, ... Oil City, Pa.
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Wolverton, Theoron, ... Washington, D. C.
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Woodhouse, Robert Hall, ... London, England.
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Y

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Z

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John R. Bennett, M. D., Cleveland, Ohio.
Peter Bryce, M. D., Tuscaloosa, Ala.
S. S. Clark, M. D., St. Albans, Vt.
E. J. Doering, M. D., Chicago, Ill.
Joseph Draper, M. D., Brattleboro', Vt.
N. McL. Gamble, M. D., Columbus, Ohio.
Hubert Haywood, M. D., Raleigh, N. C.
G. A. Ketchum, M. D., Mobile, Ala.
D. S. Kinsman, M. D., Columbus, Ohio.
C. A. Leale, M. D., New York.
T. B. Lester, M. D., Kansas City, Mo.
Joseph P. Logan, M. D., Atlanta, Ga.
Louis Mackall, M. D., Washington, D. C.
J. B. Marvin, M. D., Louisville, Ky.
Thomas Hay, M. D., Philadelphia, Pa.
A. K. Minich, M. D., Philadelphia, Pa.
W. H. Phillips, M. D., Kenton, Ohio.
A. M. Pollock, M. D., Pittsburgh, Pa.
Dr. W. J. Sheppard, Cleveland, Ohio.
Dr. S. F. Tanner, Shepherdstown, W. Va.
Dr. P. C. Williams, Baltimore, Md.
J. Williamson, M. D., Ottumwa, Iowa.

FIRST DAY.

The Section met on Monday, September 5th, at the Congregational Church, corner of Tenth and G streets, at 3 P. M.

The PRESIDENT called the Section to order, and read the following address :—

THE PRACTICE OF MEDICINE AT THE PRESENT DAY.

LA PRATIQUE MEDICAL ACTUELLE.

DIE ÄRZTLICHE PRAXIS DER GEGENWART.

BY A. B. ARNOLD, M. D.,
Professor of Clinical Medicine, College of Physicians and Surgeons, Baltimore, Md.,
President of the Section.

The occasion which gives me the privilege of addressing an assemblage of distinguished representatives of our profession, appears to me not inappropriate to cast a retrospective glance at the movements which exerted a dominant influence on medical practice of the present day. An inquiry concerning the extent and value of our therapeutical resources under changing circumstances, may not be deemed uninteresting to the reflecting physician. The remarkable acquisitions of comparatively recent date in the different departments of medicine, but more especially the high degree of precision which medical diagnosis has attained, and the invaluable contributions of pharmacology by which therapeutics has entered the ranks of the exact sciences, could not fail to tell heavily on many a traditional method of treatment, and to weaken confidence in the efficacy of many a reputed remedy. Every cheer from our modern laboratories that hailed some brilliant discovery in the domains of physiology and pathology, helped to unsettle the faith of the practitioner in the current system of therapeutics. It is a historical fact that Morgagni's and Rokitansky's revelations of the secrets of morbid anatomy caused such a thorough distrust, at least in Germany, of the official university teaching of medical science, that skepticism and nihilism became the fashion among the younger members of the profession. How could they, it was asked, still continue to direct their drugs to the cure of degenerations and destroying lesions, being without any therapeutical means of controlling or arresting tissue changes? It was, of course, a substantial gain when forward speculations and dogmatisms gave way to the true method of scientific investigation. Men of a practical turn of mind were at first little influenced by the new acquisitions, which appeared to demand a recast of accepted pathological theories. Others were loth to break with the continuity of medical doctrines on which the routine of practice was founded. But the strength of the influence which advanced views had exerted is best illustrated by the disgrace that befell the most universal and potent remedy that had the sanction of centuries of experiences. Such was the fate of the lancet. It certainly did not require the demonstrations of patho-

logical anatomy to show that there are incurable diseases, neither was it rational to underrate the value of therapeutical measures in deference to the microscope. The enlightened practitioner knows very well that the tentative and progressive spirit of science must inevitably cause fluctuations in theory and practice; but he is also convinced that medical art is in possession of sound and substantial means which enable him, amidst all these changes, to meet successfully numerous and dangerous deviations from health. A candid and competent criticism is, evidently, the only safeguard against the overweening confidence in favorite therapeutical measures, as well as the best corrective of an irrational skepticism. As little as I am prepared to engage in such a task, it is, nevertheless, clear to my mind that it should embrace the subjects which I propose to touch upon, though at the risk of rehearsing what has been far more ably presented by others.

If there is one class of diseases of a well-defined character in which unanimity of treatment might be fairly expected, one should suppose it to be the large group of acute febrile diseases which are self-limiting, or, rather, show a tendency toward spontaneous recovery—*sit veniæ verbo.* I think I do not exaggerate when I say that there is an English, a French, a German, an Italian, a Spanish and an American treatment of fevers. Statistical tables, showing the rate of recoveries under different modes of treatment, would lend themselves to no satisfactory decision, for the variable conditions and circumstances that influence the mildness or gravity of these diseases would obviously vitiate the result of a comparative calculation. It may, however, be safely asserted that the rate of mortality in fevers in general has steadily diminished in the last three or four decades. In order to narrow the compass of the inquiry in regard to the cause or causes to which this improvement is to be attributed, it will be convenient to consider the therapeutics of typhoid fever. Now and then the medical world is tantalized for a short period by the promise of aborting this disease by a special manner of treatment. A far more hopeful event is the unabated effort to discover some remedial agent for subduing the febrile movements. Nearly all the so-called antipyretics which have lately come into use, unfortunately, possess the common property of overwhelming nerve centres when administered in adequate and continued doses. The artificial depression of the respiratory, the circulatory and thermic centres cannot be contemplated with indifference in patients struggling against the onslaughts of the fever poison. With all due deference to the favorable reports in regard to the fever treatment by these remedial agents, stronger proof than we possess is required to establish the reputation of their efficacy. If the elevation of the temperature and the acceleration of the pulse constituted the only essential elements of the fevers, and not merely their most prominent and constant symptoms, then the employment of antagonistic remedies might be reasonably considered to approach the character of a curative treatment. For the time being, it must be admitted that the search for a rational therapeutic in the infectious fevers will be governed by the effort to alight on some agent or agencies that will control the febrile movements. Perhaps our successors will be more fortunate than we are in the endeavor to discover the antidotal treatment of fevers. Hyperpyrexia is undoubtedly itself a source of danger, and deserves, for this reason, the most unremitting efforts to combat it with safety to the patient. Among all the measures adopted to this end, none seems to answer as well as the judicious abstraction of heat from the surface of the body. The promptness with which the cold-water treatment fulfills this important indication is undeniable, and if the harsh method of its employment be avoided, it can neither be said to prove meddlesome, aggressive nor hazardous. Those who favor a so-called active

treatment in acute fevers, should be reminded of the history of the treatment of pneumonia, which reads like a commentary on mischievous officiousness. We moderns are no longer swayed by the fear of momentary disaster to attack a pneumonia with all the therapeutical fierceness at our command. Trousseau candidly confessed that long ago "he was tempted to leave nature to bring to a favorable issue the disease against which we are all disposed to act so vigorously, but that he had not yet dared to act." Our exuberant materia medica is somewhat to blame for the fashion of piling drugs upon drugs, though it might be asked, What are they good for, if not prescribed? But inconsiderate medication is principally due to the false conception of what constitutes expectant treatment. The old physicians are often criticised for their crude theories of disease, and their easy faith in accredited remedies. As an offset, however, they taught certain practical maxims which no physician, even of the present day, can afford to neglect. They advised "to watch the complications, to attend to the secretions, to support the patient and to obviate the tendency to death." These excellent precepts contain the very gist of the expectant treatment. In view of the limits of our therapeutical resources in the acute fevers, such a plan of treatment seems the only rational one, and suffices to satisfy professional conscience. There is one part of it which is absolutely remedial and productive of positive good. This must be conceded to faithful and skillful nursing, including a well-ordered diet, and the strict observance of hygienic rules. The physician who has a profound respect for the recuperative powers of nature under favorable circumstances, will be extremely careful in the use of coercive measures. There is a conservative medicine as well as a conservative surgery.

It is rather late in the day to discuss the supreme importance of hygiene and sanitation, not only as prophylactic measures but also as invaluable adjuvants of therapeutics. Preventive medicine, which has now been fairly inaugurated, is the great glory of our profession in these days, an event which will be signalized as the dawn of a new epoch in the history of our art. The remarkable improvement which is sometimes witnessed in diseases of a fatal tendency, in consequence of a change of regimen, diet and general mode of living, is calculated to make a deep impression on the medical observer. Balneology teaches many an instructive lesson of the same import. Vital statistics also tell a plain story. After giving due credit to improved methods of treatment, it must be admitted that a great share in the reduction of the death rate in modern times is attributable to the ameliorated condition of the less favored population and the increased attention which legislation devotes to the comforts and health of the laboring masses. The decrease in the morbility of factory hands in the large and numerous establishments of England, according to the reports of the Registrar-general, is mainly due to the passing of laws that diminish the hours of labor and prohibit the employment of children at a certain age. No other influence had a more decided effect in bringing about a change of the factory laws of that country than the constant agitation of this subject by the medical press.

The reminder to attend to the *indicatio causa* comes now with a better grace than formerly, since microscopic anatomy and the achievements of physiology enable the clinician to fill up many gaps in the interpretation of symptoms. Text-books have long ago dropped their separate chapters on dropsies, hemorrhages, jaundice, essential paralysis, and the like. The work is still going on, to change clinical into anatomical diseases. But therapeutics has not kept pace with these conquests. Our power of knowing is vastly in advance of our power of doing. The whole stock of our materia medica, with very few exceptions, consists of remedies which are avow-

edly employed either to palliate, to subdue, or to evoke a symptom. The physicians of a past period thought they had a class of medicinal substances which influence nutritive changes. They called them alteratives, of which calomel held the first rank. Perhaps we have been too hasty in our disparagement of such a claim. Absolute causal treatment is, at any rate, the prerogative of surgery. Chirurgical art, in modern times, has wrested many an inch of ground from the domain of Medicine, but these conquests are limited to those portions of it which had rightfully belonged to Surgery. Specialism, at least in the departments of Ophthalmology, Gynæcology and Laryngology, would have celebrated far fewer triumphs had it not been for the introduction of novel surgical procedures. We can appreciate the satisfaction with which the neurologist contemplates now the use of electricity as an important acquisition to his meagre therapeutical resources. It is significant that the remedial powers of this physical agent are solely due to its action in *loco morbi.*

The general practitioner encounters his old foes now, under the designations of sclerotic, cirrhotic and other forms of degeneration, with far less confidence in his ability to cope with them than he did formerly. He cannot stand idly by until some lucky chance or a pharmacological wonder shall supply him with a specific, though he would probably give a whole volume on cellular pathology for a single compeer of quinia. He therefore continues to do precisely what his predecessors did under similar circumstances. He tries to relieve pain, to remove a dropsy, to check a hemorrhage and to improve a palsied limb, whatever may be the known or unknown cause. It is not only in cases belonging to the category of progressive and fatal diseases that a symptomatic treatment, or even a crude empiricism, is in its place, but necessities occur which urge the physician to strain the resources of his art for no other consideration than to afford momentary relief of only a single symptom. Traube's language on this subject is to the point. He says, "The endeavor to remove a symptom whose persistence endangers the life of the patient, or threatens to prolong the disease or increases suffering, is as rational as any other therapeutical intention, provided the purpose can be accomplished." Sometimes a favorable change is produced, or the course of the disease is favorably influenced, on successfully contending against a conspicuous symptom. Every experienced physician will coincide with this observation. Perhaps it is by a process which may be recognized as reflex action that symptomatic treatment not seldom brings a disease to a favorable issue. It is now generally believed that hydropathy chiefly owes its therapeutical effects to the reflex action of the vaso-motor nerves. There is an aspect of symptomatic treatment which may be conceived to go beyond its original intention. No valid reason can be advanced against the supposition that structural changes may be modified by remedies which exert a specific influence on the functional activities of organs.

Pathological theories, despite their fluctuations, will continue to control practice. The currency they gain in the medical world is generally accepted as the warrant of their truth. This will always be so, for theory and science are inseparable. Inexplicable facts do not constitute science. Post-mortem appearances become scientific facts in so far as they lend themselves to the construction of theories which profess to explain the seat and nature of the morbid processes that gave rise to them. The knowledge which is thus gained, although it may only be of an approximative kind, can be made available for practical use. Strange as it may sound, there is a surfeit of facts in medicine and a dearth of good working theories. Bacterio-pathology, which is founded on a fair induction, is the most noteworthy theory which the industry and ingenuity of modern cultivators of medical science has brought forth,

though there are signs ahead that indicate a tendency to give it too sweeping a significance. Theories of disease based on the results of etiological investigations are certainly of preëminent value. Beyond question, etiology is the most obscure chapter in the whole of medicine, but its successful study is of incalculable practical importance.

That nature often cures disease expresses an insipid truism. If we only knew how nature does cure. An acquaintance with her methods offers the chance of supplementing them when they are deficient, or to evoke them into action when they are not forthcoming. Physico-chemical investigations have disclosed many of the secrets of the *vis medicatrix naturæ*, and clinical medicine has always profited by the knowledge which they afforded. Many of the names applied by the old physicians to certain classes of remedies implied their supposed physiological effects. They had their excitants, depressants, eliminants, resolvents, revulsives and roborants. This classification is probably imperfect, but in principle it is sound. Every new insight into the nature of a physiological process brings into nearer view its pathological correlative and suggests a fitting therapeutics. Thus, the artificial preparation of peptonizing ferments and the adoption of an improved dietary is due to the better understanding of the character and function of the gastro-intestinal secretions. The tendency at the present day toward physiological medicines has not escaped the criticism directed against experimentation in place of bedside observation. But this criticism is untenable; for it is not only requisite to know the therapeutical properties of the remedies we constantly employ, but it is also highly desirable to increase our stock of useful medicinal agents, and this is best accomplished by experimentation. Moreover, it would conduce greatly to the honor and credit of medical science if the mystery surrounding the action of our specifics and empirical remedies were laid open to us. Were it not that the fallacy of the *post hoc* reasoning must always be taken into consideration, there would be no appeal from the dictum of personal experience in reference to the therapeutical value of any remedy. The extreme difficulty of entirely eliminating this fallacy practically throws this demurrer out of court. The license which is thus conceded to the assertory judgment of the individual practitioner is one of the weakest points in therapeutical science. Virchow has somewhere remarked that therapeutics continues to be the only department of medical science which is tolerant of rubbish. Things have much improved since these words were written. Systematic writers should now be released from paying their pious regards to the faded glories of many a drug that had enjoyed the suffrage of our predecessors.

At one time it was thought that the numerical method afforded the only trustworthy criterion of the comparative value of modes of treatment. Practice might then be reduced to the simple empiricism of selecting that therapy in a certain disease which had been found of greatest benefit in the largest number of cases. Science and art would occupy a very subordinate place in such a system of practice. The treatment, on the average principle, would reverse the rule of treating the patient and not the disease. The numerical method is, nevertheless, the only one for judging the rate of mortality under different plans of treatment. Apart from the variability of the extrinsic and intrinsic causes that influence the mildness or gravity of diseases, every distinctive group of diseases is characterized by a constancy of morbid conditions. This sameness of the pathological factors in all the cases of the group neutralizes, in a great measure, the source of fallacy which attaches to the numerical method. In practice, use is made, and properly so, of this consideration by preferring that plan of treatment which shows a low death rate. It is hardly conceivable that

scientific rules, however comprehensive and unassailable, shall ever supersede the display of tact and judgment of the skillful physician. The practice of medicine will never cease to be an art as long as pathological doctrines are incompetent to devise an adequate therapeutics. There seems to be an incompatibility between art, which is a personal, incommunicable gift or expertness, and science, which is founded on proof and demonstration. In reality, there obtains no such incompatibility, if, by art, we do not imply a sort of divination. There is no walk in life demanding a greater amount and diversity of knowledge than practical medicine. But it is not only this wide range of studies and the acquirement of an infinity of technicalities which the physician is called upon to make his own; the problems which present themselves to him for solution tax the highest powers of observation and physiological insight, and require a sure and prompt exercise of the logical faculty. Medical art in the hands of its brightest ornaments often assumes a character akin to the inspirations of genius.

After the address of Dr. Arnold, Dr. YGNACIO ALVARADO read the following paper on yellow fever:—

ON THE PATHOGENESIS OF YELLOW FEVER.

SUR LA PATHOGENIE DE LA FIEVRE JAUNE.

UEBER DIE PATHOGENIE DES GELBEN FIEBERS.

BY DR. YGNACIO ALVARADO,

Delegate from Mexico, Professor of Physiology in the National School of Medicine, Mexico.

As science now stands, every fact points to the belief that to a certain microbe must be undoubtedly ascribed the starting cause of the series of anatomical changes which originate those particular functional disturbances which are known clinically by the name of "yellow fever." Such a fact, by itself, is not sufficient to afford an amount of knowledge available for the checking of the diverse ailments present; it would be a valuable means for research into the prophylaxis of the malady, but is by no means the leading step in controlling the attending symptoms. The former means the prevention, the latter the cure, of the illness. No better means can be devised for attaining the latter than the knowledge of the real condition of the solids and liquids of the organism when under the action of the microbe.

The object of the present paper is to call the attention of the profession to certain facts relating to yellow fever as it prevails at Vera Cruz (Mexico), which have led us into the theory that *yellow fever is an auto-blood poisoning, either by the acid phosphate of soda of the same blood having been turned from a basic into the acid form, or by the phosphoglyceric acid set free from the lecythinal—by reactions in both cases that have been produced by the feeding of the microbe upon the constituents of the sanguine fluid.* The phosphoric compound, be it what it may, has not been introduced from without into the blood; it preëxisted in the liquid under a harmless form, contributing to the physiological and perfect composition of the blood, but has been rendered toxic by the microbe, which acts then as a true ferment, and while appropriating some of the blood elements for its own sustenance, causes some of the others to be noxious to human life.

The idea of yellow fever being a case of poisoning by phosphorus is certainly not a new one; it has been set forth by other observers who could not disregard the similarity between the symptoms of the two ailments—and, by the way, they meant phos-

phated hydrogen. But although this similarity is a striking one, this teaching has had no followers, as there are good reasons against it. For instance, how is it that yellow fever is an epidemic and infectious disease if it is only phosphorus poisoning? Why can a ship be contaminated to such an extent as to infect people traveling on it many months afterward? Is there any analogous case on record of phosphorus poisoning? Why is it that natives of Havana and Vera Cruz, as well as those who have had the illness, are exempt from contracting it again? Does phosphorus poisoning ever confer such an immunity? These objections are, indeed, unanswerable arguments, and on this account the theory of the phosphorus poisoning has been set aside as untenable.

Although the quantitative analysis of the blood is the only *direct* way for solving such a question, we did not intend to undertake it during our studies of the yellow fever at Vera Cruz, some ten years ago, because it is necessary for one who makes such analysis to be *fully* conversant with practical chemistry; otherwise, the conclusions reached would be unreliable.

But there is, nevertheless, another indirect way for *very reasonably presuming* the reality of the phosphoric poisoning of the blood, and this is, to appreciate *all* the facts that a post-mortem examination, the symptoms present during the life and the course followed by yellow fever, can afford to an unprejudiced mind, and then to compare them respectively with those attending phosphorus poisoning. If there is a favorable result in the said comparison, it must be considered as a circumstantial evidence of the correctness of our hypothesis.

It is not out of place to remember here, that the right way, and the only way recognized by science, for finding out the correct explanation of any phenomenon whatever, is to build up at once upon the observed facts a *rational hypothesis*, and to test its consistency or inconsistency by instituting as many clear and *direct* experiments as may be necessary to convince one either that the said hypothesis must be set aside as void or accepted as an acquired truth. In case such a way cannot be followed, the *indirect experimentation*—as it has been termed by Claude Bernard—must be resorted to; that is, the repeated *observation* of the phenomenon in its different phases and under diverse circumstances; and this must be so far conducted as to demonstrate either the accord or the discrepancy between the hypothesis and the phenomenon itself. *Direct* experimentation leads to *certainty; indirect* to a *probability*, more or less approaching to certainty.

Let it be taken for granted that the microbe of yellow fever feeds, as any ferment does, on the oxygen of the sugar of the blood, transforming the remaining elements of the sugar into lactic acid; that this acid acts upon the basic phosphate of soda, turning it from basic into acid, or upon the lecythine, setting free its phospho-glyceric acid, and we shall have then that in yellow fever there will be but two distinct and natural periods, viz., (*a*) one of *fermentation*, during which the lactic acid is formed; and (*b*) a second stage, in consequence of the first one, in which the phosphoric compound appears, each stage being evinced by the proper symptoms attending, respectively, the fermentation and production of the lactic acid and the poisoning by phosphoric acid. Finally, we shall have to consider the anatomical lesions met with in cases of phosphoric acid poisoning.

Now let us see what the anatomical lesions are in cases of accidental poisoning by the phosphoric acid, and let us compare them with those in cases of yellow fever.

We cannot do better than to quote on this subject the following in Dr. Jaccoud's words:—

"Phosphorus, as a *steatogene* poison, is by far more active than arsenic and antimony. After its absorption in toxic doses, it determines the fatty degeneration of the liver, kidneys, heart, diaphragm, muscles, lungs, the acute steatosis of these organs being

revealed by the ordinary symptoms; and thus jaundice, diffuse hemorrhages, delirium and coma, that evince the last stage of the poisoning, must be considered rather as effects of the fatty atrophy of the liver than as consequences of the direct action upon the blood or the brain."

In a greater or less degree the same lesions have been found in animals in experimental poisoning by lactic acid.

As to the morbid anatomy of yellow fever, the following are the lesions met with by us and by every physician who has had to make examinations of the kind:—

The gums have been found congested, red and swollen; the tongue swollen, red and ulcerated; the pharynx red and swollen; the mucous membrane of the stomach has a more or less red discoloration, its hyperæmia being noticeable by enlarged blood vessels, sometimes as red patches, and at others exhibiting an arborescent appearance; its thickness has always been found increased, and softening is not of unusual occurrence, its epithelial layer being then easily scraped off. It is not uncommon to meet with superficial ulcers or abrasions. On microscopical examination, the fatty degeneration of the walls of the stomach has been unmistakably found, the degeneration implicating the small arteries; hence, the ulcers and hemorrhages.

The volume of the liver is augmented, but its density is diminished. Its color varies from the light yellow, mustard or straw to the red orange, the yellow hue always predominating. Its consistency and cohesion are notably increased, and blood flows out in a very small quantity after an incision. The hepatic cells never fail to show fatty degeneration in the different steps of its process, and the mucous membrane of the gall bladder exhibits very analogous lesions to those of the stomach.

The reddish arborescent appearance of the vessels of the peritoneum, in some cases, the fibrous adhesions between coils of intestine, and oftener the presence of a viscid liquid in the peritoneal cavity, constitute the most usual lesions found in the peritoneum. The kidneys are enlarged, hard, and their surface exhibits brownish-red patches alternating with yellow and normally-colored zones. The hyperæmia sometimes predominates and the yellowish color in other instances. There are very often ecchymotic spots, and the fatty degeneration, both of its parenchyma and blood capillaries, is observed in many cases. Sometimes abrasions of the epithelium have been observed in the pelvices and calices.

Fatty degeneration of the heart has been found in the endocardium, with whitish patches thicker than the rest of the membrane; these lesions extending to the inner layer of the main arteries, the peri-arterial connective tissue of which is very commonly the site of a sanguineous extravasation all along the arteries, as a consequence of capillary embolism. A greenish or yellowish liquid fills the pericardial cavity, and sometimes whitish patches may be observed on the surface of the membrane.

The brain, too, undergoes fatty degeneration; the cavity of the arachnoid containing a great quantity of a reddish, serous liquid, which infiltrates the cellular tissue surrounding all the blood vessels of the pia mater, which presents a beautiful red appearance after the arachnoid has been taken off.

Furthermore, the poisoning by phosphoric acid causes the blood to be acid and diffluent, and the same alteration has been found by Griesinger, Davy and La Roche in fatal cases of yellow fever.

These are, very summarily, the lesions in the organs in cases of yellow fever, and, when compared with those of phosphoric poisoning, one must be impressed with the fact that there is a very essential and tangible feature common to both ailments, viz.: *A fatty degeneration throughout the body as the last stage of a very acute process of inflammation.*

We wish to dwell upon this condition of the organism, because this is the characteristic feature in the pathological anatomy of yellow fever; no particular and unique

lesion in a special organ, as happens in other maladies, can be pointed to; the only one, we say again, is a generalized and very severe phlogosis of nearly all the tissues, having a rapid course and ending in fatty degeneration. Such a fact gives a natural and plausible explanation of every symptom of the disease.

We come now to the symptoms, and shall try to show that there are but two natural periods in this illness, such a fact being in accord with our views, namely: One of *fermentation*, the only one to which the name of yellow fever is properly fitted; and the other, which is but the result of lesions arising from the first.

No matter how skillfully the divisions of the course of the malady have been made by other observers, all of them are simply artificial, the main reason of this statement laying in the fact that there are numerous instances in which the different alleged periods of yellow fever are missing. Were they natural periods, none of them would fail to exist in every case. But among the numberless symptoms attending yellow fever, there is one that never fails to appear, and this is, the changed temperature of the body. For this reason we have taken the abnormal temperature as the basis for the study of the malady, and have noted the symptoms that accompany each one of the three natural stages of the abnormal heat—*ascent, fastigium* and *descent*—and the facts observed have led us to the conclusion that all the cases of yellow fever must be included within two classes—those in which the malady ends at the fastigium of the abnormal heat, and then the convalescence sets in at once, and those in which the illness still goes on through the decline of the abnormal heat. The symptoms attending the first class are present in every case, and those attending the second one are restricted to a certain number of patients; that is, yellow fever has two phases of evolution: The first one necessarily exists in all persons attacked with it, and the second one is only present in some of them. Patients experiencing only the first all recover, except in case the two periods are indistinct, which occurs in the severest form of the attack. Those who run into the second period have generally a severe attack, proving favorable in some, ending fatally in others.

Before going into the specification of the symptoms of each period, let us say that poisoning by lactic acid gives rise to the following symptoms: *A sensation of being tired,** which causes aching in the body all over, dizziness, somnolence, and the symptoms attending the inflammation of the alimentary canal from the mouth to the rectum, and in the last stage of the poisoning all those corresponding to the congestion of the heart, liver, kidneys, etc.

The following are the symptoms that we have very carefully observed twice a day in about one hundred patients from 1876 to 1879, at Vera Cruz, while the abnormal heat of patients was in its ascent and fastigium. For brevity's sake we shall omit all these details, and other symptoms, too, that are not closely pertinent to the subject under consideration.

Among the many symptoms attending the invasion of yellow fever, *aching in the limbs and in the arms* has been one of the most prominent ailments from the very onset of the malady in 80% of all patients. This pain has been compared by the patients themselves with the sensation *experienced after protracted walking*, and, as we have been very cautious that our inquiries may not be suggestive of the said comparison, we are confident that the character of this aching is certainly that of a tired feeling.

That particular pain which is located in the muscles of the eye and increases when the patient is compelled to look upward, shares the same characters. In 88% of all

* It must be remembered that lactic acid is normally formed in muscles at the time of their contractions, and taken immediately in by the capillaries, and when it accumulates to such an extent as to make acid the muscles' reaction, there always appears then the *feeling of being tired.*

the cases it has been present, and in almost every one of them from the earliest period of the illness. It began at the same time as that in the limbs in 88% of favorable cases and in 83% of fatal ones; following its march in 76% and 56%, respectively, to these apply the same remarks that we have before made.

Finally, the lumbago—the excruciating pain that is so frequently present in yellow fever as to deserve to be considered as a characteristic symptom—has the same co-relation as the *tired feeling* of the limbs. In 95% of our patients it has occurred, and in 85% of them lumbago and pain in the limbs have started at the same time, both following the same course; that is to say, increasing and decreasing together and disappearing at the same time. There is, later on in the course of the illness, hyperæmia and inflammation of the kidneys, and such an occurrence accounts for the absence of the co-relation between the two ailments in all cases.

It is to be seen by these abstracts from our observations that a *unique* and *general* circumstance is the starting cause of all the disorders which are present at the onset of the malady. There are but two general conditions of the body capable of producing them, namely, the abnormal heat and the pathological condition of the blood. The first must be excluded, because, although in every instance abnormal temperature and general aching sensation have begun at the same time, they have followed afterward a different course in 69% of patients who recovered and in 94% of those who died; thus, the co-relation of cause to effect is not reasonably presumed here, and, consequently, the general condition of the blood is to be taken as its only cause.

Lactic acid poisoning gives rise to an inflammatory condition throughout the digestive canal, and this happens as well in yellow fever from the earliest hours of the malady. In 100% of our cases the gums have been noticed, a very few hours after the beginning of the illness, swollen, more or less reddish, lined with a whitish layer of *oïdium albicans*, and later on, ulcers on their free edges have appeared. The inflammation of the stomach, evinced either by pain, nausea, or vomiting, never failed to exist. The tenderness of the abdomen on pressure, elicited by movements of the patient, even by the weight of the bed-covers, and by the ingestion of liquids, have been observed in every instance of fatal cases and in 93% of patients who recovered. Nausea always attended the illness from its start, and vomiting, either mucous, bilious or black, has been noted in 86% of cases who recovered and in 93% of patients who died.

Dizziness and somnolence, that are among the morbid phenomena accompanying lactic poisoning, are the usual attendants of yellow fever, the former having occurred in all cases, and in 90% of them at the outset of the illness, and the latter, although not so frequent, was present in 53% of cases of recovery, and in 93% of patients who died, appearing later than the dizziness.

There is no patient, be the character of their attack what it may, either mild, severe or fatal, who fails to show, on the whole, during the ascent and acme of the abnormal heat, besides other symptoms, inflammation of the gums, pharynx, stomach and intestines, *feeling of being tired*, headache, pain in the eyes, lumbago, dizziness and somnolence; and it is to be remembered that identical symptoms are noted in cases of lactic acid poisoning.

We have stated before that when the abnormal heat has reached its maximum and begins its third stage—the descent—the yellow fever will either be definitely over then, or the symptoms will continue to go on, and some new ones will appear, although there is no abnormal heat; that in the first case there is but one period, *the fermentation*, which always exists in every instance of yellow fever, and, in the second case, the *consecutive* period sets forth, which, according to our hypothesis, will be a case of poisoning either by phospho-glyceric acid or by acid phosphate of soda.

Do the symptoms observed in the *consecutive* stage of yellow fever correspond to those of phosphoric poisoning? We are inclined to believe that such is the case, because

the latter symptoms are those of an acute inflammation of the digestive tract, liver, kidneys, heart, and, in general, of all the tissues, ending in fatty degeneration; and we have seen that such is the characteristic feature of yellow fever, and shall show, besides, that the symptoms present during the *consecutive* stage reveal that, although there was already a phlogosic condition of the main organs, this had been aggravated at the time the pathological temperature began to subside, which fact would not have happened were it not for the presence of a new cause.

The symptoms revealing an increase of the inflammation of the different parts of the alimentary canal in that circumstance, in our observations, have been noticed as follows: Recrudescence of redness and swelling of the gums, 7% in cases of recovery, and 52% in fatal cases; bilious vomiting, 53 and 45%, respectively. Furthermore, the black vomit, which evinces either ulceration—the advanced stage of phlogosis—or decomposition of the blood, or both, as is the most usual case, has been scarce in patients who recovered, and when they occurred, made its appearance during the acme of the abnormal heat in 20%, and during the descent in 80%; in fatal cases 40 and 57% have been, respectively, the figures. The most repeated and abundant instances of black vomiting occurred in 50% of cases of recovery and in 58% in fatal cases, in the said period of descent of the abnormal heat. An increase of the nephritis is noticed by the recrudescence of the renal pain, or by the augmentation of the quantity of albumen in the urine. The former took place during the descent of the abnormal heat in 19% of patients who recovered, and in 51% of those who died, and the latter in 46 and 71%, respectively.

Delirium, as one of the initial symptoms of meningo-encephalitis, has failed to be present during the illness in 72% of successful cases, and existed in 93% of fatal ones; and when it so happened, its increase took place while the heat went on subsiding in 33 and 86%, respectively. Coma, that attends an advanced degree of phosphoric poisoning, was in yellow fever of a very rare occurrence in mild cases, and in the fatal ones *always* appeared, as the rule (0.09 excepted), while the heat was decreasing.

We regret not to have always paid due attention to the symptoms revealing the angio-cardiac phlogosis—pain and murmur; and in the cases in which we did it (16) its maximum accompanied the maximum of the cerebral and nephritic complaints.

It is to be seen, therefore, by the foregoing figures, that in severe and fatal cases of yellow fever the phlegmasies of the diverse organs undergo a recrudescence, and some others begin to be inflamed after the abnormal heat has begun to subside, which fact can be ascribed but to the presence of a new agent acting like lactic acid. Such a condition can be fulfilled but by a phosphoric compound.

If our opinions are correct, then the experimental poisoning in animals by lactic and phospho-glyceric acids would reproduce, to a certain extent, the symptoms and lesions accompanying yellow fever. With this object in view, we undertook a series of systematic experiments, which personal circumstances compelled us soon to give up. Although very incomplete, they will add to the *probabilities* of the proposed theory.

We made in some dogs hypodermic injections of two grains of phospho-glyceric acid in one gramme of water, without noticing any abnormal phenomena. In some others an intra-venous injection of 20 centigrammes of lecythine, prepared from the yelk of eggs dissolved in 1.50 of sulphuric ether, and in some others the quantity of lecythine being ten drops to ten drops of ether, and in both cases with negative results, except in the latter cases, in which an abundant salivation and lachrymation immediately ensued upon the injection. In another dog 1 gramme of lactic acid diluted with 1 gramme of water was followed by no particular phenomenon during the eight subsequent days. We then had injected into the jugular vein of a small-sized dog 0.50 of lecythine, and immediately after, 0.50 of lactic acid. Up to 9 P. M.—the injection was made at noon—no particular symptom was observed, and at about 3 A. M. on the

following day the dog was dead. At noon a post-mortem examination showed the following: There was on the ground a natural stool; no vomited matter. Cadaveric rigidity very exaggerated; liver hyperæmic, natural consistency, red-brown hue, volume a little increased. Stomach completely emptied, its mucous membrane swollen and of a dark, very red, color; red ecchymotic patches in the duodenum, which was filled with yellow bile, and in the rest of the small intestine yellow and abundant bile, too, either alone or coloring an abundant mucous matter. Kidneys congested; bladder emptied and its mucous membrane dry. Heart of natural size, and its four cavities filled with a very dark blood, the consistency of which was a little greater than, but closely resembling, gummy syrup. No signs of endocarditis; reddish-yellow and swollen patch in the transverse portion of the aortic arch; nothing particular in the carotids. Brain very much congested; its pia mater of a very vivid red hue and all its capillary vessels enlarged and very numerous; nothing particular in the arachnoid.

It is clear that death was due to the conjoint presence of lecythine and lactic acid. This agrees with the observation of similar experiments, in which the death of the animals could not be referred to the influence of only one of those acids, and, therefore, corroborates, to some extent, our hypothesis. In support of this view additional observations may be advanced in evidence, namely, that the lesions of the post-mortem examination were of an inflammatory character; the mucous membrane of the stomach and of the small intestines, and also the internal surface of the aorta, showed an analogous morbid alteration, and it is presumable that the brain, liver and kidneys were implicated in a similar manner. The urine was suppressed, precisely as it occurs in yellow fever; the blood had the same aspect, and the cadaveric rigidity was as marked as in *vomito nigra*.

Having been led to think that lecythine extracted from the blood would be better suited for the purpose than that from eggs, we made the following experiment: Rabbit, of large size—Respiration, pulse and pupil normal; rectal temperature 40.1° (Centig.); gums of a vivid rosy color; palpebral conjunctiva red; ocular conjunctiva normal.

Aug. 29th, 12.30 P.M.: Hypodermic injection in the right side of 0.004 of sheep's blood lecythine, dissolved in 1.50 of sulphuric ether. 12.35: Temperature 40°, slight anæsthesia; respiration and pulse normal; pupil enlarged. 12.45: Temperature 39.7°, profound anæsthesia; pupil very much enlarged; pulse and respiration normal. 12.55: Temp. 39°; anæsthesia, the same. 1.05: Temp. 39°; pupil less enlarged. 1.15: Temp. 38.7°; slight anæsthesia; spontaneous movements; he tries, successfully, to walk; pupil the same. 2.15: Temp. 37.6°; anæsthesia disappeared; pupil, pulse and respiration normal; he appears depressed. 3.30: Temp. 37.1°. 5.45: Same. 8.15: Temp. 39.1°; gums of vivid rosy color.

Aug. 30th, 8.30 A.M. Temp. 38.1; ocular conjunctiva yellowish; gums redder than yesterday; depressed. 8.30 P.M. Temp. 37.4°; increased dullness.

Aug. 31st, 7 A.M. The rabbit lies dead on his right side; cadaveric rigidity (opisthotonos) very marked. Around the puncture made with the syringe the subcutaneous cellular tissue was red, and a greenish liquid permeated it to a great extent around. Mesentery red and its vessels filled with blood, and in some portions of it small ecchymoses appeared. Stomach considerably enlarged by very dry food, and so was the mucous membrane of the viscera, from which large portions of its epithelium can be scraped off by a light stream of water; no change in the color of the said mucous membrane, except at the level of its small curvature, where there is a patch of red hue; nothing particular in the intestines. Liver congested, showing very numerous red dots; its consistency and size about normal; the gall bladder very much distended by bile; kidneys somewhat enlarged and dark; the bladder contained 45 grammes of urine, which after being filtered was transparent, greenish, and nitric acid test revealed the presence of a small quantity of bile and a great deal of albumen. Lungs of a vivid red color. There was in the pericardium one gramme of a greenish liquid, in

which no trace of bile could be detected by nitric acid. Brain very much congested, and so was the pia mater in a great degree, the capillaries of which appeared very red and numerous, owing to its being filled with blood.

The above experiment shows that after the introduction of lecythine in the blood, the temperature fell, jaundice and albuminuria set in, inflammation of the stomach, peritoneum, liver and pia mater appeared in a very short time, and ecchymoses were produced, as happens in the *consecutive* stage of yellow fever.

In summing up we believe the deduction is warranted, from all that has been stated in the present paper, that there is circumstantial evidence in favor of the opinion professed by us, namely—

That during the ascent and acme of the abnormal heat in yellow fever, a true fermentation takes place through the presence of a microörganism in the blood, which takes the oxygen of the sugar for its own nourishment, and the other elements of the sugar are turned into lactic acid, and hence the symptoms attending this first stage of yellow fever.

That in case the fermentation has not attained a certain degree, the malady will have a favorable issue at the end of the fastigium of the abnormal heat.

That in the contrary case, the lactic acid will cause the phospho-glyceric acid to be set free from the lecythine of the blood, and there will then be a *consecutive* period accompanied by all the symptoms attending phosphoric acid poisoning; and, finally,

That when death ensues the cadaveric lesions will be those caused by fatal intoxication with phosphoric acid, viz.: generalized phlogosis throughout the body, causing fatty degeneration of the viscera.

We confidently hope that persons interested in the advancement of science and conversant with practical chemistry will take this matter into their hands, and by skillful analysis of the blood arrive at data by which they will be able either to reverse or affirm our opinion.

Dr. Walter B. Geikie, of Toronto, Canada, then read the following paper :—

CROUPOUS PNEUMONIA, AS FOUND IN VARIOUS PARTS OF THE DOMINION OF CANADA.

DE LA PNEUMONIE CROUPEUSE COMME ON LA RENCONTRE DANS LE DOMINION DU CANADA.

DAS VORKOMMEN DER CROUPARTIGEN PNEUMONIE IN VERSCHIEDENEN THEILEN VON KANADA.

BY PROFESSOR WALTER B. GEIKIE, M. D.,
Dean, Trinity Medical College, Toronto, Canada.

I do not for a moment propose to bring a subject so familiar as pneumonia before the Medical Section of this International Medical Congress. True, no disease attracts more attention, or is more widely known in both hemispheres, and on this account it occurred to me as desirable, in addition to my own observations, to obtain by correspondence, as far as lay in my power, some information regarding the prevalence and peculiarities, if any, of croupous pneumonia as found throughout Canada, from the Pacific on its western to the Atlantic on its eastern shores. It further seemed more than likely that a short paper referring to a subject so practical, and of such widespread interest as inflammation of the lungs, would be certain to elicit the views and experience of many members of the Congress, and in *this way* prove of great practical utility.

It would be out of the question to detain this Section by reading in detail either the queries submitted by me to various medical men throughout Canada, or the replies received to these. One main point desired was to ascertain the *frequency* with which the disease was met with in different parts of the country remote from each other, and the form or forms it is wont to assume under very varying climatic conditions.

From British Columbia, on the western coast, I learn (and may say that the information received thus far has been chiefly from the New Westminster District) that pneumonia is not of very frequent occurrence—that when met with, especially in the larger towns, it is usually an accompaniment of some other form of disease—in other words, that it is a *secondary* much oftener than a *primary* affection, and as the disease with which it is most frequently associated is typhoid fever, many of the cases are prone to assume a very low form. Acute cases, however, occur from time to time, but are said by my correspondents to be not nearly as common in that region as the complicated low-type form just referred to.

Coming eastward into the as yet very partially known and very sparsely inhabited regions of Alberta and Assiniboia, pneumonia is said not to be common. Query, Is this not because settlers are so few in these vast territories?

I am also informed that it has never appeared in these parts as an epidemic, as it is reported and believed to do occasionally in many older and more fully settled localities. Practitioners there find it a purely primary disease—an acute inflammation of the lungs pure and simple. It is important to bear in mind that in British Columbia, as well as over the entire Canadian northwest, reaching from the eastern side of the Rocky Mountains to the westerly limits of Ontario, malaria, which, wherever found, so largely influences every disease, is practically non-existent.

In all the vast tract just spoken of, pneumonia is met with more or less frequently in proportion to the *number* of people settled in particular localities. It is, as in almost if not in every other place, found to take the acute form in scattered settlements, and not seldom a lower form in towns and villages, particularly in those which are rapidly increasing in population; the explanation of this, I take it, is not far to seek. Population in American and Canadian communities often increases with great rapidity, while the carrying out of efficient sanitary regulations takes much time and, what is more scarce than time in all new places, a good deal of money. The fact now happily becoming more and more familiar, that as sanitary measures are perfected low forms of pneumonia and of all other diseases tend greatly to decrease, abundantly verifies this observation.

Coming still eastward through Ontario, pneumonia is found to occur frequently, and in an *acute* form, at certain seasons—chiefly toward and during spring, especially in rural districts. But, as one would expect, many cases present themselves in which more or less blood-poisoning co-exists with the local inflammation, giving them often an asthenic character. As we pass into the more southerly portions of Ontario malaria becomes a very important factor, not in pneumonia alone, but also in every other disease, modifying not the type only, but the entire course of the cases very considerably.

From districts more or less malarious I have received conflicting reports as to the frequency of pneumonia, but learn that a very large proportion, given by some as high as *two-thirds* of all the cases, tend to assume the low form. This is very markedly the case in some of our cities, in Toronto, for example, where, during the last winter and spring, pneumonia has been very prevalent. Owing to the particularly low form of many of the cases, an unfavorable termination has occurred in a much larger proportion than has been the case for some years past. It attacked not only the weak and broken down, but many young and middle-aged persons as well, who prior to the

attack had been vigorous, and of all ages, varying from fifteen to thirty-five years. Weakly and broken-down constitutions, and persons advanced in life, sank, in many instances after only a few days' illness, in spite of every effort made to save them. General and excessive prostration was its principal feature. According to some of my correspondents who kept an accurate record, the cases were so numerous that the disease, or, as some under such circumstances would call it, the specific fever accompanying the pneumonia, appeared to be contagious. For example, one of our most experienced hospital authorities, speaking from his own observation, says nearly all the cases he saw last winter and spring presented the same low type. He found several instances of two or three cases coming from one house, each case running just the same course, one often falling ill a short time after the other. It is a pity that the *exact* periods at which the disease began in these cases were not observed. In every one of them the pneumonia was very marked as well as extensive. From several other cities of considerable population a similar report might be given, especially of the pneumonia of last winter, both as to its frequent occurrence and the low type it assumed.

The asthenic form prevalent from year to year in our Ontario cities, where we do not have the very low winter temperatures reached in Quebec and in the Northwest, is very striking. I know that in Toronto, as in other cities on this side of the Atlantic, among the poorer classes exhaustion from overwork or underfeeding may and does exist, but happily only to a comparatively slight extent, and I feel, and sadly admit, that prostration of the system to a far greater extent is due to alcoholic and other excesses. But, making liberal allowance for *all* such cases, have *imperfect drainage, more or less impurity in the drinking water*, and *malarial* poisoning not *much more* to do, than all other causes combined, in giving rise to this particular type of the disease?

In the more northern portions of Ontario the pneumonia record from rural districts, villages, and towns is just what might be anticipated—the disease being often, in *strictly rural parts, acute*—but presents a much less active, and often even a *low*, form in lesser or greater centres of population.

Coming to the Province of Quebec, we learn that in Montreal, the most populous city in Canada, pneumonia is frequent, and is, as a rule, as my best correspondent informs me, *acute* in *form*. Unless in feeble persons, young or old, or among the intemperate, the *asthenic* forms of the disease are seldom met with. The very low form, thought by some to be contagious, on which some of my correspondents in Toronto and in some places west of that city have laid great stress, is said to be exceedingly rare in Montreal, and its presence there *as an epidemic* is strongly questioned.

As is the case throughout the entire Northwest, so malaria is practically unknown in the Province of Quebec; the small amount of it met with being in persons who have entered the province from malarious localities in the West or South.

There is little doubt in my mind that to this absence of malaria, as well as to a considerable similarity of climate, is due the fact that the pneumonia met with presents much the same characteristic features in these widely separated regions.

Coming still further eastward and seaward, we notice very briefly the disease in Prince Edward's Island. This little insular province presents in summer, in most parts, the very perfection of natural beauty, although, perhaps, the less said about it in winter the better. Pneumonia of an *acute* type is reported as frequent, more so during some seasons than others; some of my esteemed correspondents refer to the cases being at times so numerous as almost to justify the view that it prevails epidemically.

As in type, course and frequency of occurrence, pneumonia is just the same, as a rule, in New Brunswick, Nova Scotia and in the old colony of Newfoundland, with its appendage Cape Breton, as in Prince Edward's Island, it is needless to do more than mention that in *all* the provinces the form commonly seen in country parts is the

acute. Now and then, due, as elsewhere, doubtless, largely to local causes, cases are seen in towns and cities, of a very low form, which tax to the utmost the skill of the medical attendants.

In this paper I purposely omit any reference to the portions of the reports sent me as to the *theories held* regarding the nature of the disease, whether it is a *local* affection with symptomatic fever only, or a *specific fever* with a local accompaniment; neither do I speak of the treatment of pneumonia adopted in different parts of Canada.

To enter on these topics would make this paper altogether too long, and long papers, like too long sermons, are not at all consistent with the brevity of human life, and nearly always make listeners sleepy rather than interested. I may, however, perhaps be permitted to say here, that many authors, some of whom are very justly esteemed and have great weight given to their views by the profession, are, *on the one hand*, rather too brief and general in their remarks on the treatment of this disease; and, on the other, frequently do not, as it appears to me, bring into sufficiently bold relief the sound principles which underlie the largest success, and which are admirably laid down by Mr. Erichsen in his great work on surgery, where he treats of the management of inflammation in general (see Vol. i, last American edition, page 225). It seems unusual to refer to a work on surgery in a paper on a purely medical subject, but Mr. Erichsen's remarks are by no means seldom quoted approvingly by physicians. I will not detain you by quoting the passage in full. The author very strongly and properly objects to all inflammatory diseases (and pneumonia is one of them) being treated on any *uniform* plan, whether by depressants or by stimulation. As regards the management of cases of pneumonia, no remarks can be more practical or valuable than this, that so far as successful treatment goes, "it is of far greater importance to be able to estimate accurately the *constitutional condition* of the patient than to be able to form a minute diagnosis of the precise extent and depth of the local mischief." We therefore, in Canada, as elsewhere, use repressive means in one case of pneumonia, and stimulate more or less freely in another—or often in the same case, after judiciously repressing existing vascular over-activity for a short time, we may, indeed, if it be called for we *must*, support and stimulate to any required extent. This varying of the means to be adopted in particular cases at particular stages, calls for the exercise of the greatest judgment and all the knowledge we possess; but it is the only practice which can secure the best results to our patients and at the same time most redound to the credit of medical science. Such practice is no mere routine, but a strict following of medical science properly so called.

For the many answers to my queries from medical friends throughout Canada, I beg, without naming them—for this they might not like—to return my sincere thanks. I am fully aware of not having been able to gather anything specially new or at all striking from any quarter of the wide field gone over, and I did not expect to do so. But to make the inquiries and to get answers from so many parts of this extensive field interested me greatly, and I hope the subject may not prove altogether devoid of interest to those who have done me the honor of being present. I heartily thank the Medical Section of the Congress for their patient hearing of this paper, and have only to regret having had too little time at my disposal to make researches as exhaustive as I could have wished, over an area comprising many thousands of miles, stretching as it does across the widest part of the American continent, and presenting climatic and other differences great in proportion to its vast extent.

DISCUSSION.

Dr. T. B. Lester, of Kansas City, Missouri, thought that pneumonia is an infectious disease, sometimes caused by malaria or insanitary influences. He had, how-

ever, cases which did well under tartar emetic treatment. In apparently similar cases this treatment had no effect, owing to the existence of the malarial element, and which readily yielded to quinia. Again, in cases of septic origin, he succeeded better with milk and whisky.

Dr. H. D. DIDAMA, of Syracuse, N. Y., said: "The presence of certain microbes in pneumonia is not accidental. The disease can be produced by their introduction, as has been demonstrated. There are many types of pneumonia, just as there are many types of typhoid fever, but these diseases are caused by specific poisons."

Dr. JAMES STEWART, of Montreal, Canada, requested members who live in malarial districts to state whether in their experience they have met with many cases of pneumonia.

Dr. THEODORE H. BRYSON, of Egg Harbor, N. J., said that pneumonia is a rare disease in the part of the country where he practices, and where malaria is not prevalent.

Dr. WILLIAM J. WAUGH, of Philadelphia, Pa., thought it difficult to determine whether a disease is infectious or not in a solitude.

Dr. GEIKIE said, according to his observations, pneumonia is not more frequent, but a more serious disease when complicated with malaria, for the accompanying fever is of a low type.

SECOND DAY.

The Section again met on Tuesday at 3 P. M.

Mr. JOSEPH KÖROSI, Director of the Communal statistics of Buda Pesth, Hungary, read an abstract of his paper on the preventive power of vaccination. The paper is elsewhere printed in full in this volume, so that the abstract is necessarily omitted.

DISCUSSION.

Dr. C. A. LEALE, of New York, expressed himself greatly interested in the paper, especially for the valuable statistics, evidently the result of much patient labor. He referred to a commission which had been appointed under his supervision to inquire after the health of the sick children in New York, with the result that there was not a case reported which had had smallpox. This he attributes to the good virus and enforcement of vaccination among all classes. Many years ago he was called to investigate two cases of death alleged to have been the result of smallpox, but in which death was evidently due to erysipelas. This was rendered more positive, as others were vaccinated with the same virus without harm. He once saw a child that died, after vaccination, from secondary syphilis.

Dr. JOHN S. LYNCH, of Baltimore, Maryland, said: "It is absurd to deny the protective power of vaccination. Bovine virus will always be safe, and should be exclusively used."

Dr. WM. WELCH, of Philadelphia, said: "I believe vaccination possesses the power of absolutely preventing smallpox. During my hospital practice I met with over five thousand smallpox cases, and none of the number that were vaccinated took the smallpox. I think humanized virus answers all the purpose. As to its durability, it will run out."

Dr. W. M. WHITMARSH, of Bristol, England, then read the following paper:—

VACCINATION AND THE PASTEUR TREATMENT.

DE LA VACCINATION ET DU TRAITEMENT PASTEUR.

VACCINATION UND PASTEUR'S BEHANDLUNG.

BY W. M. WHITMARSH, M. D., M. R. C. P., M. R. C. S., LOND.

Many of you, no doubt, have visited the laboratory of Pasteur, in Paris, seen the operations on the rabbits which have died of rabies and the inoculation of the living ones, also the rabbits in their different stages of paralysis prior to death. The theory of M. Pasteur is that he discovered some five years ago a method by which he could render certain animals insane by inoculation beneath the meninges of the brain.

A live domestic rabbit is tied down to a board by its front and hind legs, a small quantity of chloroform is put on a piece of blotting paper and held to the rabbit's nose a few minutes; when it is entirely under the influence, a longitudinal incision is made through the scalp, in the median line, and the skull trepanned by an instrument invented by Pasteur for this purpose, and which you will find with the rest of his instruments used for this operation.

The next step is to cut off a piece of medulla oblongata of the brain (irrespective of size) from a rabbit which had died that day. Mix it with a little sterilized beef tea in a mortar, then warm and inject it under the meninges of the brain. The scalp is now put together and sown up, and in a few days it heals by the first intention. During the first period, six days, the rabbit eats fairly well, but on the seventh paralysis begins in the hinder legs, on the eighth it extends to the front, and on the ninth rabies is at its full extent and the rabbit ceases to eat, literally dying of starvation, which takes place on the thirteenth or fourteenth day.

I have entered into these details simply for the purpose of showing the treatment from the commencement. Now the inoculation on human beings is seldom made from the medulla oblongata, as M. Pasteur considers it too strong, but from the spinal cord. It is necessary before the inoculations are started on the human being that the spine of fourteen dead rabitic rabbits should be prepared, by keeping the cords suspended in a glass bottle containing caustic potash, which dries them; two rabbits have to be inoculated every day to keep up the stock.

M. Pasteur believes that the virus of rabies is a "living microörganism," and that, like some others, it produces in the tissues it invades an excretory substance, by which, when present in sufficient quantity, its own development and increase are checked, as are those of the yeast ferment by the alcohol produced on the vinous fermentation. In accordance with this theory, he thinks the spinal cord of animals that have died of rabies contains both the virus and the excretory substance, which practically may be deemed its antidote.

He believes, therefore, that by injections of an emulsion from such spinal cords into the system of animals bitten or inoculated with the virus of rabies, the antidote may be able during the period of incubation to arrest and prevent the fatal influence of the virus. But in order to avoid the possibility of injecting a still more potent virus, M. Pasteur holds that the virus in the spinal cord must be weakened, by drying the cords in a pure and dry atmosphere at a temperature of 20° C., in which drying the efficiency of the antidote may be reduced to a much less extent than the potency of the virus.

M. Pasteur asserts:—

By such drying this potency may be so reduced that an emulsion of the dried spinal cord may be injected without any risk of producing rabies; and this risk is in no measure increased by the daily injections of emulsions from cords dried during a gradually less number of days, and which, though more virulent than those first used, still contain a larger proportion of the antidote than of the virus.

The preventive measures first used by M. Pasteur were adjusted in the following manner:—

Days inoculating	1	2	3	4	5	6	7	8	9	10
Days during which the spinal cord had been dried	14	13	12	11	10	9	8	7	6	5

In consequence of some deaths among those who had been thus treated, M. Pasteur deemed it necessary, in cases of very severe bites and of persons bitten long before the treatment could be commenced, to increase the intensity of the treatment by more

speedily increasing the strength of the injections, by more frequent repetition of them, and by using on certain days spinal cords dried only on the third, second and first days.

Thus in September and October, 1886, he adopted the following formula :—

Days inoculating.....	1	2	3	4	5	6	7	8	9	10	11
Days during which the spinal cord had been dried.....	14, 13, 12	11, 10, 9	8, 7	6, 5	4, 3	2	1	6, 5	4, 3	2	1

In very severe and perilous cases this course was repeated three or four times. It was distinguished as the "methode intensive," but when it appeared to Pasteur that the rate of mortality was greater in this method than the former, he changed it for that which he now uses, and which may be thus represented :—

Days inoculating	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
Days during which the spinal cord had been dried	14, 13	12, 11	11, 10	10, 10	9, 9	9	8	8	8	7	7	7	6	6	5

In the report to the Local Government Board by the commission appointed to inquire into the Pasteurian treatment and rabies, they say that they know of *no deaths* from the first method. Deaths have occurred from the first method. Case of Berge, of Bordeaux. Bitten in July, 1886. The day was certified to by a veterinary surgeon. Three days after Berge was bitten, he underwent a complete course of treatment by Pasteur, by the non-intensive treatment, and on its completion returned to Bordeaux. On the 23d of January, 1887, he first complained, and died of hydrophobia at Hospital St. Andre on January 30th, 1887. That he died from rabies was proved by experiments made by Dr. Ore, at Bordeaux. The same dog bit another man. There are also other cases, but time will not permit me to go into them. But after the intensive treatment, deaths have occurred under conditions which have suggested that they were due to the inoculation rather than to the injection from the bites. They also reported that forty deaths took place up to April 30th, this year, but Pasteur published a table admitting eighty-five up to June 30th, 1887.

The patients assemble at 44 Rue d'Ulm, close to the Pantheon, Paris, and a very interesting group they appear to be. They remain in the waiting-room, are called into the consulting-room in proper order to have their names taken by M. Pasteur's representative, and Dr. Roux takes a history of their case. They then return to the waiting-room until all their cases have been taken down, when the inoculations commence. The gentleman, Dr. Roux, who performs the duties, is a qualified medical man; and here let me state, M. Pasteur never inoculates. He is sometimes present, but more frequently absent, as the laws of France will not allow him to take an active part in the operation, he being neither a physician nor surgeon, which probably accounts for the patients not undergoing any medical treatment or restrictions as to living. M. Pasteur is a first-class scientific chemist, but doubtless knows little of the art of surgery or science of medicine; and, as far as his operations are directed, he is constantly changing his methods, thereby showing that he feels somewhat doubtful of his standpoints. The operation is a very simple one; namely, injecting into the abdominal walls a solution of rabbits' spinal cord mixed with sterilized beef tea. For first cases bitten by actual contact, the patient is inoculated morning, afternoon and evening, for three days, and then at different times, for twenty-one days. The first

day from spinal 14 days old, 13 and 12; second day, 11, 10 and 9; third, 8, 7 and 6; and then once or twice a day up to twenty-one days. Those bitten through the clothes, once or twice a day; but this varies according to circumstances. The last day of attending the Rue d'Ulm, the patient has injected the poison, or the rabitic matter of the first day of the death of the rabbit, so that the plan of operation is from the weakest point, fourteen days of spinal cord, leading up to the first day. It is a pity Pasteur did not select the arm for inoculation, instead of the abdomen, for the children scream and appear dreadfully frightened at having their clothes removed. The explanation that Pasteur gives of his selection is, that it is less likely to set up inflammation, but he would find the arm a more desirable part of the body, and not more dangerous, if so much, as the abdomen. During the three weeks of operations, the patient undergoes no medical treatment whatever, nor is any notice taken of the diet; and frequently patients just operated upon are to be seen sitting down outside the cafés, drinking the most intoxicating liquor sold in Paris, namely, absinthe. M. Pasteur has brought his remedy before the public as a cure, and it is only right that it should be given a fair criticism. To be bitten by a mad dog is bad enough, but to receive injections of mad rabbits, extending over three weeks, when you have not been bitten, is too terrible to think of. M. Pasteur says time will reveal all things, and that so far he has not had sufficient time given him. Let us hope, in the end, he will be able to demonstrate to the medical profession and the public that he has made a discovery. At present, very few who have gone thoroughly into the matter have reason to believe so.

M. Pasteur's statistics are most unsatisfactory, and are certainly unfavorable to the operation, and show it is no certain preventive. The cases have to be thoroughly analyzed. It does not follow that those who rush to the operating-room in the Rue d'Ulm have been bitten by a mad dog. Dogs bite and men fight because they are annoyed or teased, but how many are really mad? Very few, indeed. The old remedy of applying nitrate of silver has quite died out, and in place of it a strong crystal carbolic acid is used, which destroys the lower tissues, which, if applied directly after a bite from a mad dog, will do away with the chance of the sufferer ever having hydrophobia. During the thirty-five years I have studied medicine I have had hundreds of cases under my care, of dog bites, but never a fatal one.

M. Pasteur states he has discovered a sure remedy, and that he has conquered a disease. The profession would be very apathetic if it took for granted such a statement. If the seat of the virus is known and proved by Pasteur to be in the medulla oblongata, why does he not more frequently inoculate his patients from that seat, instead of the spinal cord? And then, again, why require fourteen rabbits? Only contemplate vaccinating a child from fourteen other children as a remedy for small-pox! If it is a fact that rabitic poison is found in one rabbit, why not keep to one; and if the virus is too strong, reduce it 14 times, 13, 12 and so on, according to the day the patient requires it.

If the whole strength is not in one, how does M. Pasteur prove it to be found in fourteen? If it were necessary that fourteen microbes were required to produce a cure, and only one microbe could be found in one rabbit, then we should have scientific data to go upon. No one has yet discovered a microbe either in the brain or spine of the rabbit, although a medical man at Geneva states he thought he had, but afterward said he was not quite certain. Then, again, Pasteur has treated all sorts of cases, those bitten and those not bitten; he has also treated a number who have been said to have been licked by dogs; he has injected persons with rabitic matter who went merely stating they had been bitten, who went for the purpose of obtaining charity or exciting attention from the more wealthy who assemble there. M. Pasteur shows results simply caused by the large number who have rushed to him from all parts, some actuated by fear, others by curiosity. The bite of a healthy dog cannot

produce hydrophobia. It has been proved beyond all doubt, that dogs kept shut up in a room, and given but a small quantity of water, will at times go mad; and a gentleman from India told me last September, during the time he was attending Pasteur, that his dog bit him from this cause. Mothers, fathers, and others should think very seriously over the matter before subjecting themselves to this treatment, until some better results are shown than those now placed before the public. Two pug dogs were running about the laboratory, in Paris, during the time the dead rabbits were operated upon, and I asked if it was not dangerous; but M. Viola, the operator, said, "No; they can eat as much as they like, even of the rabitic medulla oblongata;" which proves, to my mind, that symptoms which the rabbits show can be produced, and have been produced, by injecting portions of the medulla, as was done by Signor Absell, who was employed by the Portuguese government. He reported adversely to Pasteur, and that the so-called hydrophobia in patients is brought about by the operation. To continue, M. Pasteur tells us that if a dog, or rabbit, or other animal, be bitten by a mad dog and die of rabies, a substance can be obtained from its spinal cord which, being inoculated into a healthy dog or animal, will produce rabies similar to that which would have followed directly from the bite of a rabid animal, or differing only in that the period between the inoculation and the appearance of the characteristic symptoms of rabies may be altered.

The rabies thus transmitted by inoculation may, by similar inoculations, be transmitted through a succession of rabbits with marked increase of intensity. But the virus in the spinal cords of rabbits, by drying the cords so that at the fourteen days it is inert, may be injected into a healthy rabbit without producing any bad effect.

By injecting a portion of medulla oblongata, as before described, beneath the covering of the brain, the animal may be made secure against rabies, whether the bite be from a rabid dog or any other animal. The duration of immunity from rabies is not yet arrived at. These, gentlemen, are the principal points in the treatment of Pasteur, so called.

To criticise the new discovery of Pasteur for hydrophobia in 1880:—

In the first place, Pasteur lays claim to have found out that by inoculating the medulla oblongata of a rabitic dog or rabbit into a healthy dog or rabbit it would become rabitic. Now this was known in 1828, and a year previous to 1880 M. Raynard had announced the transmission by the saliva of rabies from man to rabbits, and we believe those latter rabbits died of hydrophobia.

Pasteur states he found a special microbe in the blood, but he found the same microbe in the saliva of children who had died of common maladies. It was a new microbe not before discovered; this same microbe, however, is to be found in the vocal cavity of healthy human beings, and M. Vulpeau also established that the saliva of healthy persons can kill rabbits, the same microbe being found in them.

This fact was announced in 1844, in England, by Samuel Wright, Esq., M. D., Physician to the Birmingham General Dispensary. (Vide *Lancet*, Vol. i, 1844, p. 122.) Dr. Wright also states, p. 124, that animalcules are perpetually found in saliva, both healthy and morbid. We may therefore assert it, that Pasteur did not describe a new malady. Dr. Wright must therefore have the credit of having discovered this fact long before Pasteur announced it as his specialty. M. Dubone stated a theory in 1879, that the nervous system, and especially the medulla oblongata, were important factors in the development of rabies; and here it may be stated Pasteur only inoculated rabbits with the medulla oblongata and not the spinal cord; that the phenomena of rabies is evinced by morbid anatomy depends upon specular alterations to the medulla oblongata, but he does believe the virus travels along peripheral nerves to the central nervous sytsem. Dr. Dolan, of Halifax, England, has given great attention and study to the claims of Pasteur, and in the *British Medical Journal* of September 4th, 1886,

he is spoken of very highly in reference to his publication on rabies and hydrophobia. Dr. Dolan says he has been assured by M. Pasteur that a true remedy has been found, and that he has conquered a disease which has been for many ages the despair of medicine. We are justified in examining the grounds on which this claim rests. The profession would be untrue to itself did it accept the dictum of any experimentalist, no matter what may have been his past services, without submitting the results to the touchstone of reason.

We need not repeat M. Pasteur's experiments, to form an opinion on the merits of his method; time has solved part of the question.

The deaths after his preventive inoculation are the saddest corollaries we could have on the falseness of the basis on which the prophylactic rested. I may here briefly point out some of the objections.

Control experiments appear to have been neglected. M. Pasteur originated a disease in rabbits which he called rabies. The same kind of affection in rabbits can be produced by injecting almost any kind of diseased material into the same region favored by M. Pasteur.

In man the same symptoms as in hydrophobia can be produced by irritating the medulla, and in certain cases we find a disease simulating hydrophobia produced by a tumor or by cysticerci pressing on the brain. A case is reported to the practitioner, narrated by Dr. Dolan, wherein cysticerci did produce the simulative affection.

The number of deaths after being treated by the Pasteur treatment, up to June 30th of this year, as admitted by Pasteur, was 86, and several have taken place since. The report to the Local Government Board, after experiments at the Brown's Institution, London, was to the following effect: This report was only handed to the Local Government Board in July of this year, which was, that Pasteur has proved that inoculating a healthy dog from a rabid one, the dog would have rabies, but that the protective treatment had not yet been put into operation, and therefore nothing could be said respecting it, but if persons bitten by a mad dog went afterward to Paris the result of the treatment would be doubtful and uncertain. Now, considering that within the last six months Pasteur has again altered his treatment, as he did not consider the inoculation first put into operation sufficiently protective, therefore, those who have visited Paris for the operation prior to the alteration simply wasted time and money, and doubtless before long we shall hear of some other alterations made in the treatment by Pasteur.

Pasteur's great name as a scientific chemist carries weight. His previous work almost demands faith. The gods do not blame the gods; *it is for mortals to criticise and pick holes in the garb of the celestials.* The experiments made by Mr. Victor Horsley, secretary to the commission for inquiring into the Pasteur treatment, were far from numerous, and will not satisfy many continental experimentalists, like Frisch, Abreau, De Renzi, Amoorso and others, Sir James Paget, and other distinguished pathologists. And it shows how careful we must be before we can accept evidence supported by Sir James Paget, Lauder Brunton, and others, who only got their information second-hand; that is to say, simply what their secretary thought proper to tell them, he having performed all the operations, at which they were not present. The reason why the committee were sixteen months in giving in their statement, although frequently asked for it, was, that several members of that committee did not consider they had anything satisfactory to report upon, inasmuch as the great test of inoculating a human being with rabitic matter as a protective had not been tried. However, the secretary was anxious a report should be forwarded, and it was put to the vote accordingly, and I think he might be complimented on its fairness, as it simply goes no further than is generally admitted; he winds up his report by not recommending Pasteur's treatment, but says, "The consideration of the whole subject has naturally raised

the question whether rabies and hydrophobia can be prevented in this country. If the protection by inoculation should prove permanent, the disease might be suppressed by thus inoculating all dogs; but it is not probable that such an inoculation would be voluntarily adopted by all owners of dogs, or could be enforced on them. Police inspections would suffice if they could be rigidly enforced, but to make them effective, it would be necessary—

First, That they should order the destruction, under certain conditions, of all dogs having no owners, and wandering in either town or country.

Second, That the keeping of useless dogs should be discouraged by taxation or by other means.

Third, That the bringing of dogs from countries in which rabies is prevalent should be forbidden or subject to quarantine.

Fourth, That in districts or countries in which rabies is prevalent the use of muzzles should be compulsory, and dogs out of doors, if not muzzled or led, should be taken to the police as "suspected." An exception might be made for sheep dogs and others while actually engaged in the purposes for which they are kept.

These are examples sufficient to prove that by these or similar regulations rabies, and consequently hydrophobia, would be stamped out in this country or reduced to an amount very far less than has hitherto been known.

Now as regards the commission that went to Paris, the *Provincial Medical Journal*, in August, 1887, says: "Briefly speaking, the percentage of those who did not develop hydrophobia after the inoculation is small, and looked at by percentages the results are most striking. Here we find the weak point in the evidence; the committee themselves have dealt a blow at the system, perhaps the most severe that has yet been struck. Various percentages have been fixed; in a certain group the mortality is high, in another it is low. Averages have been fixed on, actually varying from 5 to 60 per cent., which shows there is no scientific data as yet on the subject, and all is to a certain degree arbitrary. The committee have taken the lowest estimate, 5 per cent. for dog bites, and 30 per cent. for wolf bites, a percentage which will hardly please the Pasteurians. Here we may dwell on a series of observations in the report which fully bear out all we have said in reference to the hydrophobia scare and to the unreliability of the evidence that the persons who went to Pasteur were ever in danger of contracting hydrophobia. We require no further evidence on the question to convince most people that our attitude has been perfectly justifiable."

Prof. Burdon Sanderson, Dr. Lauder Brunton and Mr. Horsley went to Paris and personally investigated the history of the ninety patients whose names appeared on Pasteur's list; they are average cases. Now let us mark the result. In 31 cases there was no reliable evidence that the dogs inflicting the bites were rabid, in the others the bites were inflicted through clothes; 24 were bitten on exposed parts by dogs rabid. We may dismiss the 31 bitten by the non-rabid dogs and those bitten through the clothes. The 24 cases are of interest. Here the committee made a calculation which requires an explanation. Taking the 90, the committee say 8 would have died had it not been for the inoculation. As they have already fixed a 5 per cent. mortality, we cannot see the justification for raising it; and as the 24 were only exposed, taking a mortality of 8 persons for the 24, gives a mortality of something like 32 per cent.

The 66 other patients out of the 90 were treated just as the 24 patients; they were injected, and it is a matter of congratulation that they were none the worse for the treatment. Here we have a part of the problem upon which light is needed. If the Pasteur virus was efficacious in eating up the pabulum in the case of the 24, who, according to the committee, were exposed to the danger of contracting the hydrophobia, what was the effect upon those who really did not require the injection? This, of course, opens out the whole question: What is the nature of the rabid virus,

and what is the nature of its antidote? It is not too much to say we know little of either.

We have also some errors creeping in as regards the table of cases sent in to the Local Government Board. In the first case, of Dr. Hughes, of Oswesty, England, the Commission state he was bitten by a rabid dog. But I produced a certificate from the veterinary surgeon stating the dog was not in any way mad, but only suffering from dyspepsia. The Enquiry Commission get out of this error by stating that the table was supplied to Professor Graucher from Pasteur's table, whereas nothing but absolute proof should have satisfied them.

In fact, the so-called cure or treatment of persons bitten by animals supposed to be mad, is nothing more or less than an experiment on human beings, fraught with danger and attended with conditions of uncertainty. People have become accustomed to the idea that the Pasteur treatment for dog bites is the best, and without rhyme or reason rush off to the scene of operations. But the weakness of M. Pasteur's position could not escape the observation of the keener-sighted and more logical men of science. As deaths have occurred, Pasteur has had a different explanation for each; but how can he possibly explain 815 cases in one year for his treatment? At length we find he offers an explanation which is equivalent to a confession of not knowing his position. He had previously not made his inoculations either sufficiently numerous or sufficiently strong; then the intensive treatment is introduced; but what becomes of the cases from India, Australia, Russia, England and America which he has treated in the simpler or modified process? Why, they have wasted their time and money, and are probably in a worse position than if they had never gone through the 14-rabbit process, for now probably they have had introduced into their blood a new disease, which may be termed Pasteurphobia. Not long after the intensive method had been put into effect, several deaths occurred among the English patients—Smith and Goffi, who died at St. Thomas' Hospital, and Wilde, of Rotterdam, occurred under such peculiar circumstances, that the medical men could not pronounce with certainty as to what was the cause of death. In the case of Goffi, who was bitten by a mad cat at the Brown's Institute, where the experiments were made on dogs, cats and rabbits, he went the same day he was bitten (after having the parts cut out and touched with carbolic acid) to Paris, where he underwent the intensive treatment for three weeks; he then returned to London; a week after he complained of symptoms of paralysis, was taken to St. Thomas' Hospital and died in a fortnight after, from exactly the same symptoms that the rabbits show after inoculation from a rabid rabbit. The medical men stated that they did not consider he died from the cat bites, but from the inoculation of Pasteur. Here was, indeed, a good chance for Pasteur to prove his antidote, from the fact that every precaution was taken prior to Goffi's being submitted to his treatment.

Dr. Clarke showed the close resemblance that existed between the disease of which they died and that produced by Pasteur in rabbits from whose spinal marrows the so-called "vaccine" is prepared. His letter, which appeared in the *Daily Telegraph*, was translated and reprinted in the Paris *Journal de Médecine*. The week following its publication, a case very similar to these two occurred in France, and was reported in the *Journal de Médecine*, whose editor pointed out the striking confirmation it gave to the opinion expressed in the letter, that the inoculations were accountable for the deaths.

Professor Peter gave the particulars of several deaths resembling the two English cases, and attributed them to the inoculations. The man Rebillac, a sailor, had been bitten six weeks before, and had undergone the intensive treatment. Four days before his death he was seized with pains, which radiated, not from the cicatrix of the bite, but, as in the case of Wilde, from the points where the inoculations had been made. He fell into a paralytic condition, had spasms of the throat, and was at

times unable to swallow liquids, and he died, like Wilde, of Rotterdam, foaming at the mouth.

A second case mentioned by M. Peter was that of a child bitten on October 8th, and inoculated during twelve days, beginning October 20th. On November 23d he was struck in the loins by a companion, fell ill 36 hours later, and died in 36 hours with spasms of respiratory muscles. There had been an impossibility of swallowing liquids. Nor were these two the only cases.

Since the last sitting of the Academy, notwithstanding "the attempts made by the authorities to conceal the facts"—from which we may conclude that the acknowledged death roll is considerably under the true figure—M. Peter had, he said, become acquainted with three new cases subjected to the intensive treatment; the first was that of a man, 47 years of age, who died at Dunkirk. He was bitten on August 8th by a mad dog, and not cauterized—thanks, said M. Peter, to the false security inspired by the Pasteurian pretensions. On December 29th, 112 days after the bite, he was taken ill, and died of the usual hydrophobia convulsions on the following day.

The second case occurred at Constantine, and closely resembled those of Wilde and Rebillac. The patient, a man aged 46, was bitten on October 12th. From the 20th he was treated by the intensive method. On November 20th pains appeared at the seat of the bite; on the 23d pain was felt where the inoculations had been made. He was sleepless; breathing became impeded by the accumulation of mucus in the bronchi. There was aversion to fluids; the next morning he "died." At the post-mortem examination (as in Wilde's case) the lungs were found "congested." This, Professor Peter contended, was a case of canino-experimental, or modified rabies, showing the inefficiency of the treatment and the combined effect of the two viruses.

The third case occurred at Arras. The patient was a man of 42, who was bitten on November 12th, and underwent the intensive treatment on the 17th. On December 10th he felt great pain at the seat of the inoculations, which extended from the loins up the spinal column. He was nervous and depressed, saying that he felt the same symptoms as at the time of inoculation, and felt very tired (the likeness of this case to that of Wilde is very striking). There was no pain at the seat of the bite or in the corresponding limb. On the 13th a doctor diagnosed lumbago, but a few days later changed the diagnosis to inflammation of the spinal cord. Difficulty of breathing, weight in the front of the chest, spitting, slow, jerky speech, interrupted by involuntary respiratory movements, convulsions in face, trunk and upper limbs—but not general convulsions—appeared successively, and the patient, completely paralyzed, died on the 17th. Professor Peter attributes this death to the rabbit-derived virus.

The *Lancet* says, "M. Peter's facts do not seem to have been contested. We have to ask for proof that the inoculations are of use to any one, and in its absence we deny that the method is therapeutic at all. On the other hand, we point to the English cases and to those adduced by Professor Peter, to show that the 'therapeutic' method has not failed to kill, *though it has conspicuously failed to cure;* and now 'Failure' has been written over the Pasteurian method by another hand."

Professor Von Frisch has tested M. Pasteur's results in the laboratory—for one set of vivisections is sure to find another!—and has not confirmed Pasteur's results. Here is Von Frisch's conclusion:—

Lancet, January 1st. "Most of the animals which were submitted to the preventive treatment after subcutaneous inoculations with rabitic rabies died of the disease, even when the period of incubation was thirty-four days."

"These experiments show," says Von Frisch, "that Pasteur's method of rendering animals refractory to rabies is not yet either sure or certain."

There is not yet a sufficient basis for the application in man of a preventive treatment after the bite of a rabid animal; moreover, it is quite possible that the preventive treatment, at any rate the intensive method recently recommended by M. Pasteur, may itself transmit the disease. This is exactly what was foretold before M. Pasteur commenced to experiment on human beings, and this conclusion was arrived at by the pure light of reason without the aid of vivisection.

Perhaps, now that Professors Peter and Von Frisch have pronounced the system a failure, on clinical and vivisectional grounds, and that Dr. Graucher, who performs the inoculations, virtually confesses that they and we are right, the people of Europe and America will open their eyes to see how they have been misled by M. Pasteur, who, while holding out to them the semblance of a cure, has really been carrying out a vast vivisectional experiment at the expense of their own blood.

In July last I was asked to attend a select committee of the House of Lords, and I told their lordships that some country, either England or America, should test the Pasteur treatment, by resorting to the same method as adopted some fifty years ago for smallpox, prisoners to have the option of meeting their sentence or, first of all, be injected with the Pasteurian mixture, and after three months to have a small wound prepared on the arm, and then the saliva of a rabid dog applied to it. This would test the so-called discovery, and if no deaths occurred after this, then M. Pasteur would deserve all the praise that could be bestowed upon him, but until that has been put to the test, people would act wisely in keeping as far away from the 14-rabbit inoculation as possible. I also stated that I considered hydrophobia had been fostered by the government by the compulsory muzzling of dogs with uncomfortable muzzles, thereby preventing their drinking water or even swallowing their saliva. I advised that the regulations in force in Germany and Vienna should be put into operation—all dogs to be registered; name, address, and registered number to be put on collar; when sick, dogs to be taken to a veterinary surgeon and obtain a report. In default, penalties to be enforced. I consider this precaution would meet the matter, and be far preferable to being inoculated with rabitic virus.

I will conclude by giving the report of a death at St. George's Hospital, which occurred on the 14th of August last—Martin Cahill, aged 29, the servant of an officer stationed at Portsmouth—after being bitten by a mad dog.

Lieutenant Day stated "deceased had been in his service for ten months. About June 14th, while staying at Templemore, County Lipp, two days, it was reported to me that the deceased had been bitten by one of his hounds. Deceased had been cauterized by Dr. Mitchell; there were two wounds on left wrist. The dog afterward bit two hounds, both of which went mad on June 22d, two days after the bite.

"By Dr. Mitchell's advice deceased went to Paris and was treated by Pasteur. Deceased was inoculated the first five days, in the stomach, by injection. After five days the inoculation was less perfect. After various inoculations deceased returned from Paris. The last time he saw deceased he appeared in good health."

Mr. Russell Coombe, house surgeon at St. George's Hospital, deposed "that deceased went to him on August 5th, and showed him one of his wounds which had not quite healed; he gave him a lotion. Up to the 11th of August he was treated as an outpatient, but hearing he had passed a restless night, attended with great pain, the witness ordered his admission into the hospital. On Friday he had a catching in his breath and was in great pain. He ordered him morphia, but he grew delirious and fought with the attendants. He calmed down after the morphia, but on Saturday night, the 13th, he began to bark and snap like a dog. When told not to do this he desisted, showing he was conscious of what he was doing. He had, however, to put shackles on his feet and leave him with male nurses. He grew more and more violent, and expired on Sunday morning, the 14th.

"He could not say what the cause of death was, but a portion of the spinal cord had been taken for the purpose of inoculating some dogs with it, in order to see if it would produce hydrophobia.

"The post-mortem examination of the deceased revealed nothing except the marks of inoculation in the abdomen. The symptoms he had described were not those of a typical case of hydrophobia.

"Communications had been opened with Pasteur respecting the case, but no explanation as yet had been received, but Pasteur must have used strong injections."

You will see, therefore, gentlemen, this case is simply analogous to that of Wilde and Goffi, and swells the deaths as admitted by Pasteur to ninety.

In conclusion, I might add that when Monsieur Pasteur gave me two rabbits for the purpose of carrying out his experiment in England, I was thoroughly unbiased as to his theory, and was wishful to prove or disprove it to the medical profession; however, credit of no mean order must be given to M. Pasteur for opening up this all-important subject and throwing increased light upon this particular branch, so that better care will be taken, in the future, of that ever faithful, true and life-lasting friend, the dog; and let us hope that when the protective treatment is tried on the human subject the result will be to immortalize the name of Pasteur.

DISCUSSION.

Dr. C. A. Leale, of New York city, said: As the chief medical officer of the St. John's Guild, for the care of the sick children of New York city, I have coming under my observation from eighteen to twenty thousand sick children annually. These children are sent to us from all quarters of the city, and all have been carefully examined by duly qualified physicians to certify that the child is sick. As these children are only the sick of the families, they represent, probably, at least one hundred thousand children from families of New York city and the surrounding places. I am also the chairman of the committee of the large Seaside Nursery at New Dorp, Staten Island, and therefore have had an opportunity of observing a vast number of sick children during the past five years, and I have not during that time seen or had a case of hydrophobia reported to me, although hundreds of these children have at different times been bitten by dogs. Twenty years ago I had charge of the largest class for the treatment of diseases of children in the city, and during five years had over five thousand children under my professional care, and during this entire time I did not see a case of hydrophobia.

Who has ever seen a mad dog or a case of hydrophobia in the human being, are questions I have put to at least one hundred New York physicians, who have answered in the negative. I am fully convinced that much harm has been done by unnecessarily exciting those bitten by dogs, and when we consider that a very large proportion of people have at some time been bitten by a dog, we may well imagine the results of such false alarms upon the nervous systems of the delicate. I therefore sincerely hope that the general profession will not, with our present knowledge, resort to Pasteur's methods of inoculation.

The Section then adjourned.

THIRD DAY.

The Section met on Wednesday, September 7th, at 3 P.M.

Dr. OUCHTERLONY, of Louisville, read the following paper :—

THE NATURAL HISTORY OF DISEASE.

DE L'HISTOIRE NATURELLE DES MALADIES.

DER NATÜRLICH VERLAUF DER KRANKHEITEN.

BY JOHN A. OUCHTERLONY, A. M., M. D.,

Of Louisville, Ky.

Deeply impressed by the importance of the study and its practical bearings in giving much needed solidity and permanence to medicine, both as a science and an art, nevertheless it is with no little timidity that I enter upon the consideration of the subject. For I am most anxious to avoid even the semblance of trying to disparage the science we love and revere, and to the study and practice of which we have devoted our lives. No one can more ardently admire, or more sincerely rejoice in the great achievements of modern medicine, than I do. But the interest and honor of science are not only advanced by reviewing what has been accomplished, but are perhaps promoted in even a higher degree by bringing out in bold relief whatever may be defective, and the means most likely to overcome it.

It seems to me that nothing could be more conducive to this end than a careful, systematic study of the natural history of diseases. This would include not only a study of their etiology and their nature, but also of their mode of development, course and termination, when left entirely undisturbed by the interference of art. The subject is one of great difficulty, not only on account of its vast scope, but also because of the often complex character of disease, and most of all by reason of the obstacles of extraneous origin which will inevitably beset the path and obstruct the progress of one engaging in such a course of investigation.

On looking around us, we find in every department of pathology unmistakable evidence of the all-pervading presence of law. According to law, the more or less orderly succession of morbid phenomena as well as of normal processes takes place. A knowledge of these laws reveals the natural history of disease—disease pursuing its natural course in the human body, undisturbed by remedial agencies, and following out its tendency to recovery, complete or partial, or to death. Even a superficial view will satisfy the observer that in practice at the bedside the physician does not generally manifest great trust in nature. His eagerness to relieve pain and restore health tends to make him forget nature's agency in the cure of disease.

But diseases themselves are perfectly natural, though not normal conditions of the living body, and the same power which called them into being may also not unreasonably be supposed to be in many instances adequate to their removal. The truth is, that

nature does possess far greater power in curing disease than is admitted by most of us, even in theory, and still greater than our practice would seem to imply.

These words do not arise from a want of appreciation of medical science, nor are they intended to dim the lustre of those brilliant achievements which have in our day so wonderfully multiplied the resources, and so vastly enhanced the power, of medical art. On the contrary it is because of a profound realization of the tremendous power of medicine to modify disease, that the necessity of being able to measure with more exactness the influence of our art seems so imperative to the writer.

But at the present time, when organic chemistry, both synthetically and analytically pursued, is so rapidly adding to the number of medicinal agents, and when the experimental study of their physiological action has given so much interest to their application in therapeutics, there certainly appears to be some danger of forgetting the importance of the study of the natural history of disease, which must necessarily be the basis of our estimates of all medication, and without which anything like a philosophical view of the effects of remedies and their influence in modifying disease is impossible. Indeed, our knowledge of the natural history of disease in general is very limited, and often quite unsatisfactory.

The natural course and termination of disease is frequently ignored. The question is rarely asked, What would have been the course, duration and final issue of the case, if no medication whatever had been resorted to? And we are all prone to regard our treatment as the most potent, if not the only factor, in effecting a favorable result. This error springs not from personal vanity or professional arrogance, but is simply one of the evils flowing from our ignorance of the methods and powers of nature in working out problems of disease. Most unfair would it be to discredit medicine because her progress has not been more prodigious in rapidity and extent. Let us reflect for a moment. What we to-day understand by the science of pathology is but of recent origin. How many diseases of frequent occurrence are there which at the beginning of this century had no nosological existence? Endocarditis, the whole group of renal lesions included under the generic title of Bright's disease, exophthalmic goitre, trichinosis, myxœdema, a number of parasitic affections, and many others. However, these will suffice for illustration. In regard to some of them, it may be said that, considering the inherent difficulties of the inquiry, there has not been time enough to round out our investigations into complete knowledge. But even if selection be made of diseases with which the profession has been acquainted from time immemorial, it will be found that their natural history is but imperfectly known. Pulmonary phthisis was long regarded as a pathological unit, and its anatomical and clinical varieties were unknown. It is little more than five years since Koch's great discovery of the bacillus tuberculosis was given to the world, and to-day its agency in the production of phthisis is not generally recognized.

The state of knowledge with regard to cholera presents a strong analogy to that of tuberculosis.

Croupous pneumonia (the very name was coined less than thirty years ago) was, until a comparatively recent date, universally regarded as a local inflammation, due to local irritation; and the accompanying febrile disturbance was believed to be secondary and symptomatic. The later view that it is a specific fever has been confirmed by the discovery of the pneumo-coccus. Its infectious character has been ably advocated and supported by ample and convincing clinical testimony. Yet on the floor of this very Section we have heard these discoveries ridiculed and denied by some, while they received only a half-hearted and qualified support from others.

In the therapeutics of this form of pneumonia such wide differences exist that they must be supposed to rest on opposite and conflicting views of the nature of the disease.

The cause of such multiplicity and divergence of scientific opinion is to be sought in our neglect of the study, and consequent ignorance, of the natural history of disease.

No candid and impartial person will deny that more energy and zeal in the prosecution of this interesting and important study are imperatively demanded at this time; especially when one reflects that, in spite of the wonderful developments of various other departments of medical science in our day on the subject of the natural course and tendencies of disease, the advance has been so slight that in many instances we find our knowledge here not far above that possessed by the profession half a century ago.

The only certain and safe escape from this labyrinth of ignorance, error, difficulty, and doubt, is through the study of the natural history of disease. For by this means alone can the practice of medicine be placed on a solid, firm and enduring foundation. This work can be carried on satisfactorily only by the coöperation of large numbers of medical men all over the world, directing their attention to the various morbid conditions which affect the human race. It must consist in protecting the sick from all extraneous sources of irritation and depression which might disturb the natural course of the disease; but in all other respects the patient must be left entirely to nature without any interference of art. When such observations shall have been carried on until a sufficiently vast and varied amount of material will have accumulated, then will be the dawn of a new era in our science, the brilliancy of which will dim the lustre of all that has gone before.

These observations must include patients afflicted with various diseases, as modified by age, sex, occupation, etc.; the duration of the malady, the events marking its course, and, lastly, the mortality and mode of death. We will then be in a position to judge with positiveness the value of any drug in shortening disease, preventing complications, or averting a fatal result.

Many obstacles stand in the way of executing such a plan. To some it will appear culpable and cruel to withhold from any human being the assistance of our art, and consign him to the exclusive care of nature. Others will be actuated by the dread of censure which might be incurred by such a course. Another class, who are possessed with the idea of the exclusive power of art in the cure of disease, will regard it as dangerous to trust to nature alone the chances of recovery.

The immense benefit to humanity and to science certain to accrue from a sufficiently extended series of observations of the kind and in the direction I have suggested, should of itself be an incitement to us to undertake the work.

But other arguments which will completely answer the objections that may be raised against it are readily found.

1. While we do not hesitate to subject the sick in our public hospitals and other charitable institutions to treatment with medicines the action of which is imperfectly known or entirely unknown; and while we consider such a course legitimate, it must be admitted that to withhold all medication in the same class of cases, and simply watch the operations of nature with the view of accumulating facts and thereby accomplishing more certain good, is even more legitimate and humane.

2. It must not be overlooked that nature, having inflicted disease, is also, in many instances, adequate to the cure. This is true not only of comparatively light ailments, but also of severe and, in their nature, grave diseases, such as malignant fevers, and occasionally of tuberculosis and cancer. Spontaneous recovery in the worst cases of fever, which appeared absolutely hopeless, has so often been observed, that its possibility must be beyond dispute. The possibility of spontaneous recovery in tuberculosis has been attested by such an array of well-authenticated cases that it is now no longer open to doubt. But it is when the patient has simply been provided with good

hygienic surroundings that this happy result has been most frequently observed. Hence the steadily increasing favor with which the profession regards the climatic treatment of this formidable enemy of our race.

The annals of medical literature furnish illustrations of the spontaneous cure of external cancerous affections, and I could summon the personal experience of surgeons of the highest character in further confirmation of its occurrence.

3. The strong tendency to recovery in acute affections is often admitted, and is an impressive admonition to us to reduce medicinal interference to a minimum in many cases; while in many others it constitutes an ample justification for allowing the disease to pursue an undisturbed and natural course, especially when greater certainty and broader knowledge are to be promoted thereby.

4. The character of self-limitation which we now know is possessed by many diseases, should be a warning to those who entertain exaggerated ideas of the results produced by their treatment, and an encouragement to those desiring to study the natural history of disease.

5. Were it not that the vis medicatrix naturæ is no imaginary power, but a living reality, it would be impossible to understand how it is that many quite feeble or absolutely inert medicaments could have obtained so high a reputation in the treatment of various and severe affections. To-day "Sage" is known to have no medicinal power, and is barely employed as a condiment, yet the learned Salernian of old wrote of it:—

> "Cur moriatur homo,
> Cui salvia crescit in hortis."

How great is the number of drugs which we can remember to have been lauded like the garden sage for imaginary virtues, but after awhile sunk, like it, into well-merited disuse. Yet, while in vogue, the journals of the day were filled with accounts of their powers and remedial effects. Yet all the time it was the Great, Silent Mother who wrought the cure.

6. The indisputable fact that recoveries take place from the same diseases under quite opposite plans of treatment, allows no other inference than that the recoveries often are due to nature alone.

7. When one recalls to memory how marvelously patients sometimes get well under the rude and pernicious medication inflicted by quacks and other uninstructed persons, one is forced to conclude that nature not only is adequate to remove the original disease, but also to overcome the artificial disease, not seldom superadded by the energetic ignorance of the practitioner.

I hope that the objections which may be urged against a more general movement toward the study of the natural history of disease have been shown to be without sound foundation; while the necessity for it has been duly set forth, and the benefits to be derived from it have been made clear and conspicuous.

It only remains to offer a few suggestions for carrying such a course of inquiry into practical effect. I am well aware that all physicians cannot, if they would, and should not, if they could, forego medicinal treatment in the management of all the sick under their care. Far from it. But even in private practice instances will occur where such a course would be both safe and proper, and it is most desirable that intelligent and truth-loving physicians should avail themselves of such opportunities for the good of science and of humanity.

(*a*) The greatest as well as the most favorable field of observation must necessarily be found in our large hospitals, and it is to be hoped that in them a certain number of patients may continually be set aside, in whom the natural history of their respective diseases can be studied with the requisite intelligence and care.

(*b*) The education of medical students should be directed toward repressing the

belief in the exclusive action of art in the treatment of diseases, and rendering more prominent the utility of studying their natural history.

Some additional facts might properly be exacted in evidence of my own labors in this direction. But this paper has already attained the fullest proportion admissible, and I can but say, in conclusion, that I hope in the near future to publish whatever shall have been accomplished in my own peculiar field of labor. Such observations as these carried on by a single individual accumulate slowly, unless the materials for observation be exceptionally abundant, and the opportunities for investigation be particularly favorable. In the meantime it must be conceded, and I congratulate myself upon the fact, that the present occasion is most auspicious for interesting a large number of medical men in a course of investigation which cannot fail to yield important results in proportion as these investigations are accurate and extensive.

Dr. THOMAS D. CROTHERS, of Hartford, Conn., then read the following paper:—

THE DISEASE OF INEBRIETY AND ITS TREATMENT.

LA MALADIE DE L'IVRESSE ET SON TRAITEMENT.

DIE KRANKHEIT DER TRUNKSUCHT UND IHRE BEHANDLUNG.

BY T. D. CROTHERS, M.D.,

Superintendent, Walnut Lodge, Hartford, Conn.

It is a fact of historic interest that inebriety was called a disease long before insanity was thought to be other than spiritual madness.

On an old papyrus found in one of the tombs of Egypt, dating far back into antiquity, was found a clear, explicit recognition of the disease of inebriety. Herodotus wrote, four centuries before the Christian era, that in drunkenness both body and mind are sick. Diodorus, Plutarch, Chrysostom, and Ulpian, the Roman jurist, all mentioned the disease of inebriety long before and after the Christian era. From that time down to the present century this fact has been mentioned by scientists and philosophers in almost every age.

By a strange shifting of events, insanity, which was supposed to be a spiritual affection until a comparatively recent date, is now studied as a physical disorder, while inebriety, which was mentioned as a disease twenty centuries ago, is still invested with the superstition of a spiritual origin. At last, within a quarter of a century, this great truth has been formulated and organized into the realm of practical science.

This is a discovery in science evident from the sharp contradiction which it has provoked, and the same opposition which every new truth must meet before it is accepted. In the first surveys of a new country, only a few general outlines are marked out, leaving wide gaps and stretches to be filled up by other and future studies.

In like manner the purpose of this paper is to point out some outline facts of the disease of inebriety and its treatment, and indicate not only the few facts known, but the wide ranges of the unknown awaiting future discovery and study.

When inebriety is seen from a scientific point of view, it is found to be controlled by laws which vary according to certain physiological, psychological and physical forces, of which heredity, environment, culture, nutrition, brain and nerve vigor, etc., are prominent.

When accurately recorded histories of many cases of inebriety are studied and compared, certain fixed ranges of causes appear, which seem to follow some regular order

of movement. The following is an outline of these causes: 1. Certain conditions of heredity, certain physical and psychical shocks, and nerve injuries in a large proportion of cases, are followed by inebriety. 2. Certain structural changes of the brain and functional perversions, certain disturbances of nutrition and nerve irritations, precede inebriety in many cases. 3. Certain unstable brain organizations, irregular retarded and defective brain developments, manifest in great feebleness or intense activity, are intimately associated as apparent exciting causes with many cases of inebriety. 4. Certain diseases seem to have a special predisposition to develop into inebriety with or without any exciting causes. These conditions are uniformly found either alone or combined in all cases of inebriety. The outbreak of inebriety varies widely, and is gradual and obscure at first, or sudden and precipitous. It may be marked for a long time, or be very prominent, with long, irregular halts. In the first group of causes, the inebriety may be of the dipsomaniac type, appearing suddenly. In the second group it is often concealed for a long time. In the third group, long halts and apparent recoveries occur. In the fourth group it begins suddenly and is of short duration. In the second and third groups inebriety is not unfrequently a symptom of insanity and general paralysis, and these diseases follow from inebriety. The craving for spirits is not the disease, but only the symptom or expression of the disease. It is the demand of the disordered governing centres and general organization for relief, and alcohol, as a narcotic, supplies this demand most perfectly. Often the first use of alcohol acts like a spark, concentrating and exploding the diseased tendencies which may have been forming or transmitted from the past. Also, it may give an impulse or impress to a feeble, unstable organization that quickly emerges into inebriety. In these cases a train of degeneration is started which will ever seek relief from alcohol. The more impulsive and intense the desire for spirits, the greater the degeneration. When the craving is paroxysmal with free intervals, the degeneration is central and profound. All these periodical and paroxysmal cases are closely allied to epilepsy; the free intervals may recur with uniform exactness in time and duration. In some cases the return of the drink paroxysm may be predicted with as much certainty as an eclipse of the sun, and the duration of the paroxysm rarely varies more than a few hours. In other cases, the paroxysms, both in length and free intervals, are irregular and uncertain, appearing unexpectedly and ending suddenly, in adverse conditions apparently. Thus, in some cases, the inebriate will begin to drink in the most opposite circumstances and surroundings, and will stop suddenly in equally adverse conditions, beyond all rational explanations to account for such acts. In certain cases, the appearance of health and the confident hopefulness of the patient are signs of the approach of a paroxysm. Great mental and muscular activity is often followed by an outbreak of inebriety, lower in degeneration than before. These strange and mysterious phenomena are undoubtedly governed by laws that will be known in the future. Such inebriates, like epileptics, have distinct nerve storms and explosions of morbid energies. This is literally a great dark continent of the most fascinating psychological interest.

When inebriety is developed, either continuous or periodical, there are distinct traces of a regular order of symptoms. Other diseases may complicate and cover up the order of the march, but sooner or later the same symptoms appear, following the same order. Thus, certain brain palsies of the higher centres, certain delusions and hallucinations, certain psychical manias and melancholias, certain morbid impulses and functional perversions associated with anæmias, hyperæmias and a great variety of complex symptoms. Sometimes these are masked with the appearance of health, but generally they are apparent.

A certain number of inebriates are morbidly conscious of their condition, and live in the thought of their wickedness and misery. Humiliation is coupled with a delirious faith of recovery; they are pleased to serve as examples for others. Exalted delu-

sions and delirious manias are always present. Another large class seem thoroughly unconscious of their condition. They have delusions of strength to recover, and believe all accounts of their danger or weakness exaggerated for the purpose of creating alarm in them. This sense of danger grows less and their faith in themselves increases in steady progression, on to death. Although the action of alcohol may differ widely in different cases, the same general symptoms mark all the stages of acute and chronic degeneration. Inebriety in America is becoming more and more distinct in intensity and maniacal symptoms. The use of alcohol is more precipitate, followed by mental excitement, delirium, nerve and muscle exaltation and depression. Increased culture and intelligence takes away the coarser and more repulsive symptoms of inebriety, but increases the maniacal and suicidal phases and mortality.

Inebriety is more concealed to-day than ever before, and more intimately allied with, and terminates more often in, pneumonia, Bright's disease, apoplexy and various organic and heart affections.

Inebriety is increasing, apparently, from the fact of the increased consumption of alcohol in this country, and the increased number of persons arrested for inebriety in our large towns and cities. The dipsomanias, intermittent and periodical inebriates are increasing, and the moderate drinkers are diminishing.

From a variety of evidence as yet in outline, it appears that inebriety moves in waves and currents, prevailing like an epidemic, then dying away.

This seems to be evident from the statistics of persons arrested for inebriety, and from comparative studies of the number of inebriates in different countries. From these data it appears that inebriety steadily increases up to a certain maximum point, then declines to a minimum number, resembling a tide-like movement of continuous ebb and flow. Some of these drink cycles have a period of about eight years. In Sweden it is fourteen years, in other countries it is less. The consumption of spirits in England was found to follow a similar ebb and flow movement of eight to ten years. These great movements seem to be beyond any range of causes that will explain their presence. They have been traced in cities and villages of New England, in many instances, where it is clearly evident inebriety prevailed at certain times to great excess, then died away, only to reappear again after a lapse of years. The evidence is steadily accumulating, pointing out these great drink cycles, which, like storms, gather and increase up to a certain period, then die away.

The great unknown forces and laws which regulate these epidemics await discovery. When they are known, then much of the mystery of the strange temperance revivals and agitations which spring up and sweep over the land, dying away as suddenly, will be cleared away. Temperance agitations have a strange, wave-like movement, evidently controlled by laws unknown. Who will doubt that they may not be the reactions of some high tidal-waves of inebriety, the backward swing of some great drink cycle? A physiological protest, and consciousness of the presence of some bright point of inebriety, which will recede back, and with it the agitation will die away?

These are but hints, and faint outlines of facts and forces that control inebriety, far above the roar of theoretical agitation and personal revolution.

Inebriety is a disease governed by certain fixed causes, which follow a certain order of events, and the evidences of these and other facts are clearly within the range of a more accurate study and a wider intelligence to remedy and prevent.

In the efforts to check and prevent inebriety, the interest and agitation are increasing far beyond all rational knowledge of the subject. The following are some of the theories on which the present efforts for cure are based. Moralists assume that inebriety is always a vice and sin, the only treatment of which is prayer and conversion.

Temperance men and reformers call inebriety the result of a weak will power, and urge the pledge and the force of societies as the only real cure.

The temptation of saloons, and the facility of procuring spirits, are supposed to be active laws to be broken up only by prohibitory legislation. Ignorance, in childhood, of alcohol and its effects as a cause, is supposed to be remedied by forced instruction in the common schools concerning the nature and character of alcohol.

The theory most widely believed is that alcohol is a sin and crime, to be cured by punishment and suffering. It has been stated that in this country, for 1886, over three hundred thousand inebriates were arrested for inebriety and punished by fine and imprisonment. Yet it is the unanimous testimony of all competent observers that such means for restraining inebriety not only fails, but increases the very disorder it seeks to remove. Every inebriate sent to jail for inebriety is made more unfit for temperate living, and further removed from health and capacity to recover.

He goes to jail with impaired nutrition and degenerate brain and nerve force. He is suffering from brain anæmia and structural changes, with mental exaltations and depressions. In jail the quality and quantity of food are ill adapted to restore and build up the weakened organism. The hygienic influences are wanting, the mental influence is bad, and the associations and surroundings still further depress and weaken the mental vigor necessary for restoration. The only compensation for this is the removal of the alcohol, which is often a small factor in the treatment.

All this, and much more, can be read in the anæmic faces, tremulous eyes and depressed movements of inebriates in jails and prisons. Happily, the great delusion of inebriety being a moral disorder, a vice, a sin and crime to be punished, is passing away. For ages the phenomena of nervous diseases and insanity were moral disorders, to be remedied by law and church forces. These remedies, applied most vigorously, made no impression; insanity and nervous disease steadily increased. To-day, history repeats itself in the punishment of inebriates and the efforts of church and society to stop inebriety, while the army of inebriates goes on increasing.

As long as the causes are unknown, the remedies will fail.

All exact study of the facts indicates that the disease of inebriety is both curable and preventable. We must realize that inebriety is a dangerous disease, not only to society, but to the victim, requiring restriction of personal liberty and legal guardianship, under medical care in special surroundings. Like victims of contagious diseases, the inebriate should be quarantined, and forced to come under treatment. Society should not tolerate their presence, or permit them to injure themselves and imperil others by their acts. Dr. Bellows said long ago: "Inebriates will eventually be restrained in hospitals and treated medically the moment their liberty becomes dangerous to society. The terms of their confinement will be limited by the conditions and cure of the disorder. Society gains nothing by holding any man prisoner who is fit to be at large; liberty and human rights gain nothing by allowing any man to be at large for a moment who is destroying himself, his family and his neighbors."

Facts and experience show that this can be best accomplished in workhouse hospitals, where restraint and all other appliances can be gathered. These hospitals should be military training asylums, where medical care, restraint, occupation and general treatment can be applied for long periods. They should be divided into three classes—the first for the chronic incurables, the second for recent cases and the middle classes, and the third for private and wealthier patients.

The first and second classes should be built from the moneys received from the license fund, and supported by the same fund and the labor of its inmates. The first class of these military hospitals for the chronic cases should be placed in the country, on large farms, where various occupations can be profitably carried on. They should be managed by a board, with an expert superintendent, the same as in all State charities.

The government should be of military exactness, and every condition of living and surrounding regulated with steady uniformity. Each case should be studied, and a course of treatment pursued that would build up both the mental and physical organization; also rest, change, occupation, in exact surroundings, where every defect is antagonized and every element of health encouraged and strengthened.

The pauper inebriates who are now sent to jail, and the law inebriates of all grades, should be committed to these hospitals for long periods of not less than five years. On apparent recovery within this time they should be permitted to go out on parole, to be returned at once on relapse. Each one should be employed, and, if possible, self-supporting.

Practically, the final cure of persons in this class might be limited to exceptional cases, but the gain to society by housing and treating these poor victims would exceed all computation.

The second class of hospitals should be smaller and built in the same way, and conducted on the same plan of exact military care and medical treatment, for persons of limited means, who could pay some part of their expenses, and recent cases, who would come voluntarily or by force, under this restraint, for from one to three years' commitments. The same system of parole and occupation should be employed.

The third class should be private hospitals, under the same general management and plan. Rest, medical care, change and occupation should be suited to the needs and demands of all who could command them. They should be built by private enterprise and be under the general care of the State. Benevolence would endow these places, and the special means of treatment would accumulate rapidly.

The great principle of treatment in all these hospitals would be to quarantine and house the patient, and treat all the conditions which enter into the disease as well as the disease itself. These are outlines of the hospitals to be built in the future, the practical success of which is demonstrated in many ways.

The inebriate hospitals of to-day are struggling in this direction, amid difficulties that would crush out any other work not founded on great principles. The legal power of restraint is limited to months instead of years. Nearly all these hospitals are wanting in location, surroundings and means of support, except from the income of patients. Means and appliances for occupation and thorough medical care are wanting in many cases. Public sentiment and friends of patients will not sustain the physician in his efforts and methods of treatment. False pride in the friends of patients keeps them away from all hospital treatment until they are beyond reasonable hope of restoration; then, as a last resort, they send them to such places, expecting some extraordinary change. These are only a few of the great difficulties which beset all hospitals for this class, and yet unmistakable evidence indicates a large percentage of positive cures. All authorities agree that over twenty per cent. of all cases treated by these most imperfect means and appliances are cured. The nature and character of inebriety are so largely unknown that its treatment will vary with the hospital and its management. Enthusiasts and non-experts occupy the field in many cases, and urge methods of treatment that a wider intelligence would condemn. There are three classes of hospitals for inebriates in this country. The first recognize the disease of inebriety and aim to treat it on thoroughly scientific principles. The second class treat these cases as half disease and half vice, giving prominence to moral means over physical remedies. The third class are the *specific physicians*, who adopt special remedies, empirical and unfounded on any sound knowledge of such cases. The treatment, like the study of inebriety, is passing through the stage of empiricism; yet with all the confusion and ignorance which, of necessity, surrounds these efforts, the actual results far exceed the most reasonable expectations. It may be safely said that no medical field presents more difficulties for the student, as it stretches along that frontier land of sanity and insanity;

and no medical field promises grander results in the redemption of the race, and requires higher order of talent, than the study and treatment of inebriety.

These are some of the teachings of science, which are like headlands far above the mists and fogs of theory. From these points we shall discover the sources and forms which develop inebriety and indicate the means of cure and prevention. From this standpoint the possibility of restoring the inebriate and stamping out inebriety is only limited by our want of knowledge of the laws and means to accomplish this end.

Pausing on the confines of this almost unknown realm of mental disease, and looking over into the darkness of theory, and temperance agitation, and *empirical* efforts to know and cure this disorder, we are assured that here, as elsewhere, the same eternal reign of law and order exists. Here we shall find the same causes and effects, the same circumstances and conditions which are active in developing the epileptic and the insane, the inebriate and the criminal, all following some unknown lines and obeying some unknown forces as fixed and eternal as that which governs the planets.

Along this border line science is gathering her forces, and like the problem of the stars its mysteries can only be solved along the line of accurately observed facts.

DISCUSSION.

Dr. WILLIAM F. WAUGH, of Philadelphia, said: "I believe in the remedies which were mentioned by Dr. Crothers, and that asylums for inebriates often cure their patients. But these establishments exercise an evil influence on their patients, in consequence of the idle habits which they foster. These persons become indolent and careless, and depend in the future on the kind offices of their sympathizing friends. The inmates of these asylums should be made to do physical work and kept at some useful employment. Too much reliance is placed on remedies and mental influences, and too little on work."

Dr. E. CUTTER, of New York, said he had more confidence in the moral treatment of inebriates, and in the religious influence which could be brought to bear on these unfortunate people.

Dr. CROTHERS replied: "There is not one plan of treatment. Inebriety should be considered a disease, as much as typhoid fever. We are as capable of caring for the inebriates as clergymen for the sinners. The methods of faith cures and conversions are precarious and unreliable."

The Section then adjourned.

FOURTH DAY.

The Section met in morning session on Thursday, September 8th. Dr. SEMMOLA, of Naples, Italy, gave a brief statement of his experiments on the Pathogenesis of Albuminuria, and submitted the following summary of his conclusions :—

NOUVELLES RECHERCHES EXPERIMENTALES ET CLINIQUES SUR LES CONDITIONS PATHOGÉNIQUES DE L'ALBUMINURIE.

NEW INVESTIGATIONS, EXPERIMENTAL AND CLINICAL, ON THE PATHOGENESIS OF ALBUMINURIA.

NEUE UNTERSUCHUNGEN, EXPERIMENTIREND UND KLINISCH, ÜBER DIE PATHOGENESES VON ALBUMINURIA.

PAR LE DR. M. SEMMOLA,

Professeur à l'Université de Naples.

CONCLUSIONS DU MÉMOIRE.

1. L'albuminurie dite physiologique ne peut pas être considérée comme telle, parce que dans l'état vraiment normal typique les principes albuminoides ne sont pas déstinés à être éliminés hors de l'organisme. Il s'agit là toujours d'un état pathologique ou s'il vous plaît d'une condition toujours anormale. Elle sera très petite elle n'empêche pas de se croire en bonne santé, mais l'élimination de l'albumine à travers l'urine démontre toujours qu'il existe un petit désordre entre la recette et la dépense des albuminoides.

2. Les expériences faites jusqu'ici sur l'augmentation de la pression sanguine ne peuvent pas être concluantes, parce qu'elles sont basées sur des graves désordres fonctionnels produits sur d'autres organes que les reins.

3. Les plus graves maladies de cœur à leurs périodes non compensées, tandis qu'elles produisent des stases rénales constamment, ne sont pas toujours accompagnées d'albuminurie. Il doit donc exister une autre condition pathogénique de l'albuminurie.

4. En augmentant la pression sanguine générale avec la transfusion du sang avec un animal de la même espèce l'on obtient hémoglobinurie et quelquefois même une vraie hématurie ; mais pas l'albuminurie seule. L'augmentation de la pression sanguine en conséquence est incapable à elle seule de produire l'albuminurie.

5. L'augmentation de la pression sanguine par l'injection dans la jugulaire d'une quantité de sang défibriné et dans la même quantité du sang transfusé produit en même temps hémoglobinurie et albuminurie. Cette dernière en proportion considérable. Il est donc évident que la condition discrasique des albuminoides comme condition pathogénique de l'albuminurie est la vraie cause de la filtration albumineuse par les reins ; qui sont forcés de faire sortir de l'organisme tout ce qui est inutile et même dangereux au maintien des fonctions.

At the conclusion of Dr. Semmola's remarks, Dr. R. SHINGLETON SMITH, of London, read the following paper:—

SOME POINTS IN THE TREATMENT OF PHTHISIS, MORE PARTICULARLY THAT BY INTRA-PULMONARY INJECTION.

DES INJECTIONS INTRAPULMONAIRES DANS LE TRAITEMENT DE LA PHTHISIE.

EINSPRITZUNG IN DIE LUNGEN BEI DER BEHANDLUNG DER PHTHISIE.

BY R. SHINGLETON SMITH, M. D., LOND., F.R.C.P.,

Physician to the Bristol Royal Infirmary, England.

Since the discovery of the bacillus tuberculosis physicians have been keenly alive to the possibility of some more active and successful interference with the natural history of a disease for which the vis medicatrix naturæ does so little. The recent writings on phthisis display a more hopeful spirit, and even encourage the idea that some germicide may be introduced to a sufficient extent to modify bacillary growth, and so attack the root of the pathological process; true it is that no elixir vitæ which will exterminate the disease has been found, nor can we hope that such a specific will be discovered; nevertheless, the work which has been done in this direction since the last International Congress gives us encouragement to continue it, even although we may not unfrequently be disappointed in our results. At Copenhagen, in 1884, Professor Jaccoud stated, with emphasis, that no good result had yet been derived in the direction of treatment as an outcome of the discovery of the bacillus of tubercle. We may now fairly ask the question whether, at the present time, this statement can be maintained; certainly, if no other good had been received, the greater care and attention which our phthisical patients receive, ought, in itself, to be an advantage to them. It is impossible that we should take that care in dealing with a disease while we look upon it as hopeless, or treat it with success unless we can establish a scientific basis on which to direct our efforts.

I do not propose to enter into the general treatment of this disease, either hygienic, dietetic, or climatic; neither do I propose to enter into the question of the surgical treatment of cavities in the lung by aspiration or drainage; neither can I give a résumé of all the work done in this direction of the medicinal treatment by injection, either into the rectum or the cellular tissue of the body generally, or into the parenchyma of the lung itself. As regards rectal injections by the method of Bergeon, I know that this method has been adopted extensively by American physicians, and has been written of with favor, more particularly by Prof. H. Wood.* My own experience has not, however, given me sufficient encouragement to persevere with it. I have here records of three cases in which this method was carried out in every detail, the carbonic acid being made to pass through the bottled eau de Bonnes, and four litres of gas injected twice daily for several weeks, but without the slightest result on the course of the disease. I have not adopted the method suggested by Prof. Wood, of giving sulphuretted water by the mouth, at first half an ounce, then an ounce of this saturated solution in a tumbler with two or three ounces of carbonic acid water, three to five times daily; if sulphuretted hydrogen can be shown to have an effect on this disease, this method would appear to be superior to the disagreeable one of injection via rectum.

* *Therapeutic Gazette*, April 15th, 1887.

It will probably be generally admitted that in iodoform we have a drug which does in some way or other modify the course of the disease, and sometimes completely arrests it. Prof. Semmola, of Naples, at the International meeting in 1878, first directed attention to the utility of iodoform in lung disease, and since that time the evidence in its favor has been steadily accumulating ; a series of cases reported by myself at Copenhagen in 1884, showed that an absolute increase in body weight, increased appetite, diminution of cough and expectoration, and general improvement may be expected in many cases where patients are tolerant of this drug. Of the many other drugs in use, such as creosote, terebene, arsenic, no one is more generally of service than iodoform, or its more recent substitute iodol, and there appears to be now abundant evidence to establish its utility. Whether this depends on its action as a germicide, or whether it modifies nutrition in some other way, possibly by favoring fibrous growth and cicatrization, it is not possible to say; but the clinical evidence of its activity is the main ground for its use, and is not shaken by the recent discussions in Germany as to its feeble germicide powers.

The fact that this drug has proved itself of use when given internally, led me to prefer it to any other in carrying out the method of intra-pulmonary injection first used by Prof. Pepper, of Philadelphia, and since advocated by Beverley Robinson, of New York.

Dr. Ransome, of Manchester, has also used iodoform in solution in ether, olive oil, and oil of eucalyptus in four cases, and his results resemble my own, in that the practice was found to entail no great risk and appeared to be of benefit. My results in a series of cases were published in the *British Medical Journal*, October 31st, 1886, and since that time, in many other cases, similar injections have been used, but with only very partial success. Recently two cases have occurred in which severe pain and extensive pleuritis followed the injection of a solution of iodoform in oil of eucalyptus, and so much so that neither patient would consent to a repetition of this mode of treatment. The insolubility of iodoform interposes a difficulty in its administration by injection. I have adopted sound methods ; the solution in ether (1 in 8) is perhaps the best, and has only the disadvantage that the ether, by causing temporary giddiness and head disturbance, makes the patient timid as to its results. The solution in oil of eucalyptus (1 in 8) I have altogether discarded because of its irritating properties, and I should now prefer to use a solution in vaseline oil (1 in 60). The quantity to be introduced locally has usually not exceeded one grain ; the local introduction of this quantity seems likely to be more efficacious on the local patch of diseased tissue than when a much larger amount is taken by the digestive system and diffused throughout the blood.

More recently attempts have been made, especially by Meunier, of Lyons, to introduce various antiseptic drugs into the cellular tissue by hypodermic solutions in vaseline oil. Eucalyptol (5 parts in 20), eucalyptol and iodoform, carbon bisulphide, or turpentine, have been used with success.* Lefaivre also considers that eucalyptol, hypodermically, is a powerful anti-bacillary, penetrating everywhere, and not easily eliminated.† The eucalyptine (Le Brun) is a specially purified preparation of this kind, from which good results are said to have been obtained by a series of injections of one gramme daily for 20 days ; certainly this fluid is well tolerated. I have seen no evidence of any irritating properties, and neither have I seen any good results from its use ; it has had no influence on temperature, or appetite, or cough. The two cases in which I have used it pursued the ordinary downward course of phthisis in an acute form.

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* *Bull. Gen. de Thérapeutique*, January 15th, 1887.
† *Gazette des Hôpitaux*, May 12th, 1887.

I have endeavored to combine iodoform with these solutions of eucalyptol in vaseline oil; by the aid of heat eight grains of iodoform may be dissolved in a mixture of two drachms of eucalyptol and six drachms of the oil. I have also used a similar solution of iodol, twelve grains in one drachm of eucalyptol, and seven drachms of petroleum oil. Both of these solutions have given rise to some irritation when injected in the cellular tissue in quantities of twenty to thirty minims. The solubility of iodol in ether is much greater than that of iodoform; one ounce of ether, pure, will dissolve fifty grains. If, then, iodol is of equal utility with iodoform, its greater solubility gives it a decided superiority over the latter for purposes of injection. This substance, iodol, C_4I_4NH, tetra-iod-pyrrol, contains 89 per cent. of iodine, whereas iodoform contains 96 per cent. The absence of odor and taste from the iodol should more than compensate for the slightly smaller properties of iodine, which, in both cases, is in a form from which the iodine is easily liberated, and appears in the urine as an alkaline iodide. I have given both drugs internally, but have not quite satisfied myself that the iodol is equal in therapeutic value to the iodoform. Accordingly, I have not yet used the ethereal or vaseline solutions of iodol for injection into the lung. One advantage these drugs possess has been pointed out by Professor Pick, of Prague, who maintains that the excretion of the iodide is much more gradual than is the case with iodide of potassium, traces of iodine being found in the urine five days after the cessation of iodol, but only two days after the cessation of iodide. Another preparation of iodine, Lugol's solution, has been frequently used for injection into the lung, more particularly by Beverley Robinson, of New York, and Dr. J. B. White* has reported very successful cases treated in this way. It has always appeared to me that a solution containing free iodine must of necessity be irritating to the cellular tissue of the lung. Its effects when injected into serous cavities and into cellular tissues scarcely justify one in running the risk of setting up similar irritation in the lung substance.

Another fluid, mercurial bichloride, has been suggested by Professor Riva, of Pavia. He endeavors to inundate the parenchyma of the lung so thoroughly with the fluid as to destroy all the bacilli present. As much as 40 to 50 c.c. of a one to three thousand solution was injected at one time. In upward of a hundred injections no accident occurred. The number of bacilli in the sputum notably diminished, though in no case did they entirely disappear.† I have on one occasion only injected a mercurial solution into the lung; on this occasion about half a drachm was injected into a condensed patch in the left upper lobe, below the clavicle. The immediate effect was intense pain and evidences of pleuritis, which did not subside for days. This is not surprising, when we know how irritating the mercurial bichloride is, if injected into the cellular tissue hypodermically, and I should not be disposed to repeat any injection into the lung if the fluid injected was not well tolerated in the subcutaneous connective tissue. Injection into cellular tissues may of itself be of service. Of this we have some evidence, but we need more. But it appears to me that the substance injected is likely to be far more effectual if brought into intimate contact with the lung substance, and provided it does not set up great irritation there. I propose in future to always carry out this practice of ascertaining the effect of any fluid on the cellular tissue before injecting into the lung.

It seems scarcely possible to carry into practice the idea of injecting subcutaneously such substances as will be eliminated by the lungs and in the process of elimination will destroy any bacterial growths found there. The quantities so injected must necessarily be small, and their effect on the lungs infinitesimal. There remains, therefore,

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**New York Medical Record*, May 22d, November 13th, 1886.

†New York Medical Record, April 23d, 1887.

no alternative; if the substance is to reach the diseased spot in sufficient quantity, it must be injected from the outside and diffused through the parenchyma.

Experience has shown that these injections are not especially hazardous. A little cough, some local irritation and pain, occasional slight hæmoptysis, and a few other such results, have been recorded. In two of my cases there was considerable pleuritis with effusion, and hence one would hesitate to inject when the patient is going on well without. If iodoform, creosote, terebene, and other kinds of treatment failed to arrest a localized patch of tuberculous lung, then one or more injections, repeated as often as possible, may accomplish what milder treatment has failed to do. One would naturally hesitate, except under conditions of gravity, and where less energetic treatment has failed. Injections are rarely required and are often not justifiable, but in suitable cases they may accomplish what nothing else can do, and do much, not only in destroying the bacterial element of the diseased patch, but in aiding reaction to complete the process of cicatrization.

In answer to the question, *What is the best fluid?* I shall be glad if those who have had experience will give us their results. Time and experience are necessary for a solution to this question, and many workers, over a wide area, are necessary before any decisive answer can be given. Possibly something better than iodoform or iodol may be found to give the best results. If these substances be used, then I am disposed to expect better results from the vaseline oil solution than from the ethereal solution which I have usually employed.

Dr. PAVY then read the following paper:—

ON DIABETES.

SUR DIABÈTES.

DIABETES.

BY F. W. PAVY, M. D., F. R. S.,

Of London, England.

Diabetes has always been regarded as an incurable disease, and although it has elicited much study, there are still many points open for investigation. The nature of the affection may be stated in very precise terms. It is simply a faulty assimilation or a faulty disposal of certain elements of our food. If we look to the food of man we find that the chief elements of it are nitrogenous matter, fatty matter, or carbohydrates; that is to say, the large group, consisting of grape sugar, cane sugar, starch, dextrine, etc., behave in the system the one exactly the same as the other, so that it should be better to employ the general term.

In diabetes there is no difference in the behavior of carbohydrates; starch behaves the same as dextrine. It is this group of principles in connection with which there is a faulty disposal or a faulty assimilative action. The disease consists essentially of that. A healthy person, for example, takes one or the other of the carbohydrates, and it is lost sight of in the system. We know nothing more in regard to it, and we assume that the processes of life are such as to lead to the transformation of this carbohydrate in such a manner that it shall be susceptible of utilization in the system. That is the case with the carbohydrates; that is the condition of health; but what is the condition of diabetes? In diabetes one of these carbohydrates may be taken, and in proportion as the carbohydrate is taken, so is sugar eliminated in the urine. We may therefore say, and it is merely the expression of a fact, that in diabetes there is a want of

assimilative power; there is a want of the power to dispose of what carbohydrates may be taken into the system as food. Thus far, I think, we have to deal with facts.

In diabetes the sugar reaches the general circulation in a manner that it should not do. In a state of health analysis shows that only a trace of sugar exists in the contents of the general circulation, in the small proportion of 0.6, 0.7, 0.8 per thousand, so that when a carbohydrate is taken as food, it is stopped before it reaches the general circulation. The sugar exists to a large extent in the general circulation. In proportion to the severity of the case will be the quantity of sugar contained in the general circulation. The carbohydrate which is taken as food does not stop before reaching the general circulation, but in proportion as it is taken, so does sugar appear in the urine. The sugar appears in the urine in proportion as it exists in the blood, and therefore the urine may be said to be the index of diabetes.

The amount of sugar in the urine stands in direct relation to the amount of sugar in the blood. Sugar cannot be possibly kept in the circulation; it will not remain there, but it will make its appearance in the renal secretion. Now I have at present only given expression to facts that may be observed.

But why does sugar thus get into the general circulation in diabetes? That is the important point. I think it is generally admitted that the liver constitutes the assimilative organ of the carbohydrates. It is the liver that stops the sugar or the carbohydrate in its passage to the general circulation. The sugar is so changed by the liver that it is not permitted to arrive in the general circulation. Sugar in the organism is transformed into glycogen, and leads on to the production of fat. Its office may be demonstrated in the lower animals. If you want to fatten an animal quickly, feed it on an abundance of sugar. This, I think, demonstrates the use of sugar in the production of fat. The liver, then, I think, is a steatogenic organ, transforming the sugar into glycogen, and afterward into fat.

It seems to me that this is what occurs in health: the carbohydrate absorbed from the intestines is stopped by the liver, converted into glycogen and then into fat, instead of being passed through the organ and appearing in the general circulation. If, however, it passes through the organ, we have diabetes. But what is at the bottom of this faulty process? It seems to be a wrong condition, apparently arising from a faulty condition of the venous blood. The liver is differently placed from other organs in its large supply of venous blood and its proportionately small supply of arterial blood; and the blood which reaches it should be in good venous condition. If it be not in a good venous condition, we have the chemistry of the liver immediately altered, and sugar appears in the urine.

This alteration of the portal blood may be produced in a number of ways; it may be produced by diseases or by experiment. In the first place, by the injection of defibrinized arterial blood into the portal circulation. In a very short time after arterial blood, from which the fibrin has been removed, has been introduced into a vein of the portal system we find sugar in the urine. The blood may be rendered saccharine by over-oxidizing the systemic blood. Experience—not only my own, but those of others—has demonstrated this: that the carrying on of respiration in the lower animals more actively than normal, the heart's action continuing, will lead to the production of saccharine urine. If oxygen exists in the portal blood to the extent to which it ought not to exist, you will have saccharine urine.

How does this apply to diabetes? A vaso-motor paralysis of the arteries of the body will produce that condition. If, as may be witnessed, we have a vaso-motor paralysis of the vessels of one side of the head and neck, we find that the region becomes distended, not with venous blood, but with semi-arterial blood. As the result of such, the blood will arrive at the liver without being fully di-arterialized: in other words, in an imperfect venous condition. If the arteries of the

chylopoëtic viscera become enlarged, so that the blood in them does not become perfectly di-arterialized, the chemical action of the liver becomes changed so as to permit the carbohydrates to pass through it, and get into the system, and so charge the general circulation with sugar. The worst forms of diabetes I have met with are those in which there is a dilated condition of the vessels of the mouth. For the production of diabetes, it is only necessary to have a dilated condition of the vessels of the chylopoëtic viscera. But the worst cases of the disease, as I have just said, are those in which this dilated condition of the vessels due to vaso-motor paralysis has extended from the chylopoëtic viscera and visibly involved the mouth. In these cases we have the presence of an exceedingly red tongue.

Again, puncture of the floor of the fourth ventricle, that celebrated experiment of Bernard, leads to the presence of sugar in the urine. Now, what did Bernard himself observe in connection with the animals thus experimented upon? A dilated condition of the chylopoëtic viscera.

One of the first points to attend to in a case of diabetes is to test the sugar. Sometimes discordant opinions are expressed with reference to cases. One physician, for instance, will say that the patient is suffering from diabetes, while another physician will say he is not suffering from the disease. Sometimes that depends on the test made; at other times it depends upon its being a mild case of the disease, which presents varying conditions under the influence of food. If the patient has partaken freely of carbohydrates, sugar exists in the urine, and, if examined then, it is to be detected. Then he goes to another physician after he has partaken largely of the carbohydrates, or perhaps after a fast, and there is no sugar present.

We want a test that will give us a decided and reliable indication. I believe that the most reliable test for sugar in the urine is the copper test. What is ordinarily used is known as Fehling's solution. But there is this objection to Fehling's solution, namely, that it is apt to get bad after being kept, because, if kept for a long time, it will throw down a precipitate; and again, the stopper of the bottle, unless used often, is apt to become fixed, and when it is wanted it cannot be got out. Some time ago I came to the conclusion that it would be well if we could get the Fehling's reagents prepared ready for use in a solid form. I found, however, that when thus prepared the reagents rapidly deliquesced and decomposed. Here, however, is a pellet which I have had prepared, in which the difficulty has been overcome. It is composed of the copper sulphate, Rochelle salt and caustic potash. It must be made in a certain way, with the materials in an anhydrous state. The sulphate of copper is to be placed in the die first, then some Rochelle salt, next the potash, and finally, some more Rochelle salt to complete the mass. If, now, we dissolve one of these pellets in a little water, we have first produced the greenish color of the copper; but later, as the potash is reached, it becomes blue, as is characteristic of Fehling's solution. If these pellets are kept in well-stoppered bottles they will keep for any length of time. And there is this advantage about the pellet, that when it does become bad from the absorption of moisture, it becomes so bad that it cannot be used, and therefore there is no danger of its leading to error. It will, if exposed to the atmosphere, become altered, but it then turns black, so that the change is readily recognized, and it cannot be used. If now to this solution a little liquid containing sugar is added and made to boil, the oxide of copper becomes reduced to the state of suboxide, just as in the ordinary testing with Fehling's solution. These pellets are now considerably used in England.

I do not think that a case of diabetes can be satisfactorily managed unless a quantitative analysis of the urine is made, and the amount of sugar contained in it is determined. I do not consider that a person can get along satisfactorily without knowing the amount of sugar that is being eliminated by the kidneys any more than he can get

along in a case of thoracic disease without knowing, by the aid of the stethoscope, exactly the amount of disease existing there. In my own practice I desire that two specimens be brought to me—one passed in the evening and the other upon rising in the morning. By an examination of this kind we can discover errors of diet in the patient that would otherwise escape. We do not need to ask the patient at all what he has been eating; we can tell him. Errors of diet can be detected at once. We can not only detect that the patient is not following instructions, but we can tell at what meal the error has been committed. Under ordinary circumstances, if the person is passing sugar (of course, if he is not passing sugar you cannot tell anything about it), we may find in the night urine a considerable quantity of sugar, and in the morning none at all. The sugar in the night urine has been derived from the carbohydrates taken during the day. The morning sugar has been derived from the blood during the night, and, therefore, in the interval of digestion, and thus may contain no sugar. The urine of food may be quite different from the urine of fasting.

I remember a patient, whom I once treated for some time, coming to me with a bottle of night urine containing an unexpectedly large amount of sugar. I could not account for this sudden increase of the amount of sugar in it. I got him to enumerate the articles of food he had taken the evening before. Among the articles enumerated he named blanc mange, but he had frequently taken that before without injury. I told him to inquire, and that in all probability he would find that it had not been made in the usual manner, and upon doing so he learned that it had been made with corn flour (corn starch) instead of in the manner that had formerly been done, viz., with isinglass and cream. In another instance I was able to tell that she had taken her breakfast in bed. I found in this case that the night urine contained no sugar, while the morning urine was loaded with it. She had arisen late, and the only way of accounting for these conditions observed was on the supposition that she had eaten before rising, and this I found was actually the case.

Now, we want some means of easily and precisely determining the amount of sugar contained in the urine. This can be done as follows: As we know and have just seen, in testing in the ordinary manner with the copper solution the suboxide of copper is precipitated. In the method that I shall show, instead of this, the solution remains clear and becomes colorless, and the sugar is estimated from the amount of liquid being examined that is required to decolorize a given quantity of the solution. The solution is made with the sulphate of copper, Rochelle salt, caustic potash and water of ammonia. Into a given quantity of this the liquid containing sugar is dropped. It is best in testing urine to dilute it with twenty or thirty parts of water in order to make the test more delicate. The diluted urine is placed in a graduated burette, from which it is dropped into the ammoniated copper solution after the latter has been heated to the boiling point, letting it flow drop by drop until the color has entirely disappeared. The dropping of the liquid into the test solution is guided by means of a screw adjustment affixed to the tube, which can be set so as to permit the escape of forty, fifty, eighty, one hundred drops per minute. The beauty of the test is that the exact terminal point of the reduction can with the greatest surety be determined, for there is no precipitate to obscure the view of the reduction. As the saccharine liquid drops into the boiling test solution, the color is gradually disappearing and the liquid remains perfectly clear.

Sometimes albumen is found in addition to sugar in the urine of the diabetic. Not unfrequently, when the patient first comes under observation, he has a considerable quantity of albumen in the urine, and after he has been under treatment for the disease for a while the albumen disappears. This will permit me to notice a convenient test for albumen. This consists of citric acid and the ferrocyanide of sodium, the ferrocyanide of sodium being used because it makes a looser pellet than the ferrocyanide of

potassium, and, therefore, more quickly dissolves. There is this advantage about it, that as a clinical test nothing more is required. If albumen is present, there is a precipitate, and if there is a precipitate, albumen is present. It is also a test of an exceedingly delicate nature. It is a test that requires nothing to check and nothing to corroborate. It can be carried about in the pocket like a pencil case or pocket knife. The method of using the test is the following: The citric acid pellet must be used first. It very speedily dissolves in the liquid containing albumen. Sometimes the citric acid will bring down a precipitate of uric acid, or it may be of oleo-resinous matter; but as it is not the citric acid which is the test, we do not rely upon that. If the citric acid brings down a precipitate of uric acid, a dilution of the urine will dissolve it. On the addition, now, of the other pellet, consisting of ferrocyanide of sodium, a definite and reliable precipitate of albumen is produced. This test has now been out several years; so far as it is known up to the present time, nothing, under the circumstances, will occasion a precipitate with the pellet of ferrocyanide of sodium besides albumen, so that it is a test which may be relied upon.

I will speak now of considerations bearing more particularly upon the disease itself. There are different grades of intensity, probably as marked in diabetes as in any disease we have to deal with. Let us start with a healthy person. Even a healthy person has not unlimited power of assimilating the carbohydrates, but the first step toward disease is where the assimilative power is below the normal. This kind of person will not, under ordinary circumstances, pass sugar in his urine. If, however, he partake freely of preserves or other articles of food containing large quantities of sugar, he will pass saccharine urine. Persons partaking moderately of food containing hydrocarbonates also pass sugar. They may take carbohydrates to a certain extent without showing evidence of abnormality, but as soon as the limit is passed sugar will appear.

Age influences largely the complaint. Severe cases are in young subjects, mild cases are in old subjects, and the more advanced the age the better the prognosis. I know of no disease in young subjects that is more grave. The ordinary length of life in young subjects afflicted with diabetes may be said to be about two years. In middle-aged or elderly persons the prospects, happily, are of a different nature. If they follow proper management, they can keep the disease under. The unsatisfactory cases are in the young, the satisfactory in the elderly.

The age at which the disease is most common ranges between forty and sixty years. Preparatory to the meeting of the British Medical Association, a few years ago, I went through my case-book and tabulated 1360 cases, and I found that the cases occurring between forty and sixty years of age made up forty-six per cent. of the whole. Sometimes the disease commences in mild form, and may run along for several years before it is recognized. The way of knowing this is to notice that saccharine urine leaves white spots on articles of clothing, and in micturition the trousers are apt to get splashed. The "boots" at hotels are able to recognize diabetic guests. They find the spots on the legs of their trousers very difficult to brush out. I have had patients to come to me who were able, when asked, to hunt up old trousers on which these spots were found, and could remember the annoyance occasioned by them for a long time before. Such are the grounds for being able to say that the disease may have existed for some time without having been recognized.

It runs in families to a considerable degree. I do not say that it is hereditary in the same way as gout and phthisis are; but it runs in some families in a striking manner. I was asked to see a patient suffering from diabetes, who belonged to a family of five, the eldest of whom was not more than eighteen or nineteen. The mother had died of diabetes, and the grandmother had died also of the disease. One of the children was brought to me, as I have said, and sugar existed in the urine. I desired to

have a specimen from each of the remaining children. In four of the five the urine was saccharine.

There is one point with reference to the disease to which I would like to call especial attention. It is only somewhat recently that my own attention had been fixed upon the matter, and the number of cases I find to be affected in the manner I am going to mention is striking. The remark does not apply to young subjects, but to persons beyond the middle period of life. I used to come across persons who complained of pain in the legs, put down as cases of gout or rheumatism; and I took it as simply coincidental to the diabetes, without anything important in it. But I so frequently met with it, that my attention became aroused, and now I find many subjects of diabetes beyond the middle period of life thus affected. There is also more or less ataxia. It is not exactly the pure form of ataxia that is seen in locomotor ataxia, because I have noticed that these persons can stand, maintaining their balance fairly well with their eyes shut. Yet they walk with some difficulty. They cannot properly maintain their balance. They feel often compelled, in walking, to pull themselves together and make an effort to walk steadier, lest persons behind them may think they have been taking too much. Such is the experience related to me over and over again.

Then, again, with this there are usually associated some anæsthesia, hyperæsthesia and various forms of paræsthesia. Persons feel as though they are walking on pebbles. The flesh is tender, so that when the leg is grasped it gives pain. This occurs most frequently in the legs; sometimes, however, it affects the upper extremities also. Then there is an aching of the bones. The patient complains of it, especially at night, in bed. The condition appears to be due to peripheral neuritis.

Treatment.—In young subjects, all we can do is to endeavor to stay the disease for a time. It cannot be cured. At an early stage of the disease in young subjects, the regulation of the diet removes the sugar, and the patient thinks he is cured. Sometimes a patient has been brought to an extreme state of emaciation and weakness by a sudden and severe invasion of the disease. Placed under proper treatment, the change appears like a resuscitation. He goes on getting better, his hopes are raised, and he thinks he is well. But, unfortunately, this is not the case. It is a progressive disease. It is a disease which seems to progress in the same way as muscular atrophy or locomotor ataxia, but its advance is uneven. As it advances, the diet and other methods of treatment which succeeded in removing the sugar first no longer do so; the patient now loses ground and becomes weaker and weaker. At first the symptoms and the excessive flow of urine can be kept down, but the power to keep them down is soon lost, and finally something occurs to throw the patient off his balance and to lead up to diabetic coma, which is the ordinary mode of death in these cases.

It would be a hard and oppressive life to devote one's self only to the treatment of diabetes occurring in young subjects. But the success obtained in treating the disease in patients of a more advanced age compensates, in a measure at least, for the failure in young persons.

We must avoid feeding the disease, and while doing this, endeavor to convert the wrong action of assimilation into a right one. I lay the greatest stress on diet. I do not think that we can get along in the management of these cases without strict attention to diet. There must, then, be the proper food provided. It is easy enough for the patient to avoid taking some articles of food, as sweets, pastry, puddings, potatoes, etc., but it is not so easy in the case of bread. What he may take may be summed up as consisting of any kind of meat, fish, poultry and game, with eggs, butter, cheese, the various forms of green vegetable food, and a prepared substitute for bread. Milk should only be taken to a limited extent. Some authorities have recommended the free use of milk. But milk contains sugar, and milk sugar behaves in the system

exactly as any other form of sugar. I have found that where milk has been taken in large quantity, the sugar has been kept up in the urine, while, when the quantity of milk was reduced, the state of the urine improved. Certainly, it is a hardship for a patient to abstain from bread, and something else of a palatable nature should be substituted for the article prepared from the wheaten flour. I do not think bran, which is often used, is a good substitute, because it contains from 40 to 50 per cent. of carbohydrates and very little else that can be digested and applied. Gluten is better, but it contains a considerable amount of starch. If the gluten is good, it contains only from 30 to 33 per cent.; but I have found some specimens containing 70 to 80 per cent. Perhaps there is no article of food better suited to the diabetic than the almond. There is nothing which seems to supply him better with what is wanted. The almond, rich as it is in nitrogenous material, rich as it is in oily material, is just the article of food to meet his requirements, and palatable products as a substitute for bread may be prepared from it.

Medicinal Treatment.—As far as my experience goes, nothing contributes so much to arrest the disease as opium, morphia and codeia. It is difficult, of course, to say, when treating a case with one of these remedies in conjunction with diet, whether the good result is not solely the result of dieting. But my own experience is to the effect that my practice, ever since I have been giving these, has been much more satisfactory than it was before I gave them. Clearly these agents appear to exert some power in controlling the disease. It is my custom, when I get a patient of forty-five years of age, to put him at once on small doses of opium, morphia or codeia, and gradually increase. The sugar in the urine diminishes and usually disappears. After awhile I permit the patient to take a little bread—say two ounces a day. If he takes this without a return of sugar, I let him gradually increase the quantity until he takes four or six ounces a day. Then I say to him, you had better let well enough alone and stop at this, knowing how readily harm may be done by going beyond what can be taken without occasioning the passage of sugar. As long as sugar does not exist in the urine, there is nothing to interfere with a healthy state being maintained. The patient is, to all intents and purposes, in a natural condition as regards his general health.

DISCUSSION.

Dr. Herrick, of Cleveland, remarked that it had afforded him much pleasure to listen to Dr. Pavy on the subject of diabetes. He desired to ask one question with regard to the treatment of the disease. Dr. Pavy had stated that he looks on the sugar in the urine as the index of the disease; yet, in speaking of the pathology of diabetes, he referred to it as an incapacity for the assimilation of saccharine material in the circulation of the liver. Does the doctor give anything to aid in the assimilation of this principle?

Dr. Pavy replied, that at the beginning of the disease, when the patient was dieted, the sugar in the urine diminished, and after the treatment he had described was continued for awhile the sugar entirely disappeared. You make the test of allowing the patient to eat a little bread. When this can be given without producing saccharine urine, he looks on it as an indication that the assimilative power has become greater.

The President inquired whether the observations of Dr. Pavy would lead him to agree with the statement that diabetes is more frequent in the Hebrew race than others.

Dr. Pavy said that he coincided with this statement, and not only that, he also found the disease in the Hebrew is more amenable to treatment.

Dr. TRUAX asked whether death in diabetes is due to acetonæmia, as has been stated.

Dr. PAVY replied that he is not a believer in acetonæmia. In response to a question by Dr. Holt, of Vermont, Dr. Pavy stated that, although the bromides had been extensively used within the last few years, he had not much faith in them in the treatment of diabetes.

Dr. STOCKMAN then read the following paper :—

ON THE ETIOLOGY OF PHTHISIS.

SUR L'ÉTIOLOGIE DE PHTHISIE.

ÜBER DIE ETIOLOGIE DER LUNGENSCHWINDSUCHT.

BY R. W. PHILIP, M. D., F. R. C. P. E.

In the short time at my disposal, I shall endeavor to give a brief résumé of a somewhat extended investigation with regard to the etiology of phthisis.

First of all, I must premise that the scope of the present paper is hardly indicated with sufficient strictness by the words "The Etiology of Phthisis." It is not my intention to discuss the morbid anatomy of the phthisical lesions, nor the dependence of the phthisical process on the presence of tubercle bacillus, nor the important questions of heredity and of climatic and other influences, which figure so largely in the etiological chapter.

For the present, I start with an acceptance of the doctrine of the unity of the phthisical process and of the immediate dependence of the process on the presence of the bacillus. The rigidly exact observations and experiments of Koch and others have, in my judgment, placed this beyond doubt. I prefer, at least, not to raise the question now. But, in spite of the comparative fullness and clearness of our knowledge in these lines, it appears to me that we are far from a rational conception of the actual cause of death from phthisis. It was with the view of elucidating this higher etiological problem that the present investigation was undertaken.

A glance through the literature of the subject reveals how seldom the attempt has been made to solve the problem, how comparatively seldom, indeed, the question has been raised. When the matter has been discussed, explanations have been offered which may be classified roughly under four heads, viz.: (1) Progressive asthenia. (2) Loss of hæmatosis. (3) The lighting up of fresh inflammatory foci. (4) The absorption of waste products.

Now, I have no desire to depreciate the value of these as integral factors in the process. My contention is that, in view of the comparative regularity of the clinical phenomena and in the light of more recent work, they do not afford sufficient explanation. Each of them was fully discussed prior to the discovery of the tubercle bacillus; and Jaccoud, more especially, has the credit of emphasizing the importance of the fourth, namely, the absorption of waste products. Since the announcement of the tubercle bacillus, comparatively little has been added in this direction, though the features and clinical course of an ordinary case of phthisis, and those of experimentally induced tuberculosis, are well defined and strikingly similar.

What, then, is the *modus operandi* of the tubercle bacillus in leading toward death?

Its fatal propensities cannot, I think, be regarded as merely irritant or privative. In all probability, they are attributable to a power possessed by it of elaborating new products, which are afterward absorbed.

Before explaining on what facts I base that statement, I ought to mention that Dr. Hermann Weber has hinted at the probability of such elaboration and absorption. In the Croonian Lectures (1885), Dr. Weber speaks of "the chemical poison which probably is originated by the development of the tubercle bacillus in the tissues in an analogous manner, as, according to Gaspard, Pannur, Billroth, Burdon Sanderson and others, a powerful chemical poison—sepsin—is developed in the process of septicæmia." I am not aware, however, that up to the present time any attempt has been made to treat the matter more seriously. Whether this supposititious product or products be secreted by the bacillus, or be elaborated from the tissues which it infests, raises another question, which must be discussed later. It is enough, meanwhile, if we recognize the probable dependence of these new products on the presence and action of the bacillus.

Such a process of elaboration or secretion has its analogue in the more evident varieties of fermentation which have been studied by Pasteur, Schützenberger and others, for example, the alcoholic, the lactic acid, the butyric acid and the ammoniacal. More particularly the view appears to me substantiated by the following weighty evidence: The association of special forms of microzymes with special forms of fermentative action has been conclusively demonstrated by Pasteur and a large school of subsequent observers. A distinct variety of fermentation as certainly follows the admission into a suitable medium of a given microbe as the exclusion of the same microbe excludes the possibility of its occurrence. Further, the rearing of pure cultivations has shown that different effects are obtained—though some of the observations in this direction are open to question—and, certainly, marked differences in the rate of growth are observed, according to the constitution of the medium in which the cultivation is attempted, while certain organisms are most exclusive in their selective affinities. Moreover, if the same medium, say Koch's gelatine, be utilized for the cultivation, in different tubes, of different micro-parasites, the effects produced on the medium are very different in the several instances. Even in the gross, such differences, for example, in the rate and amount of liquefaction in the production of certain gases, are marked. And it is in the highest degree probable that careful examination of the medium, after cultivation has been carried out for some time, would show important alterations in its chemical constitution, as occurs in the better known forms of fermentation. In other words, the living organism has the power of disturbing, or rather, in order to the preservation of its own life, the organism is compelled to disturb, the molecular arrangement of the elements in the medium of cultivation.

These considerations open up a wide and promising field for investigation. This appears to me the aspect of bacteriological observation which is pregnant with most important results. In illustration of this, the work of Pannur, Selmi, Gautier, Krieger, Bergmann and Schmiedeberg need only be cited.

In practically applying this hypothesis to the problem of phthisis, I directed my attention first of all to urine. The results obtained, which have been given elsewhere, were not sufficiently definite in character to warrant their citation here. Examination of portions of the diseased organs, or of their glandular appendages, was abandoned, as it was found impossible to have these sufficiently fresh to avoid the objections that would inevitably assail successful results so attained. This led to the adoption of the sputum as the *materies morbi* for investigation, and that on the following, among other, grounds:—

(1) The sputum is the constant accompaniment of the morbid condition, and stands in a peculiar relationship to the diseased organs.

(2) It is accessible in large quantity, fresh, and therefore, as much as possible, free from such contamination as might be supposed to introduce fallacy.

(3) It has been shown that the maximum amount of the contagious element resides in the sputum.

(4) Having regard to the conditions of growth of the tubercle bacillus, it seems likely that the muco-purulent secretion is a peculiarly good medium for cultivation.

(5) It has been proved that tubercular sputum retains its virulence for months.

(6) The presence of the tubercle bacillus can be comparatively easily determined, while, with greater care, its relative abundance in different specimens may be gauged.

(7) The sputum can readily be subjected artificially to similar conditions outside the body as within the chest.

(8) Much of the experimental work already carried out with reference to tuberculosis has been done by the subcutaneous and intra-venous injection of unaltered phthisical sputum. (Villemin, Chauveau, Biffli, Veza, Semmer, Tappeiner, etc.)

(9) Collateral evidence, from the side of other ptomaine investigations, seems to imply that the ready access of oxygen to the centre of ptomainic production aids considerably in their rapid and abundant development.

After approaching the subject in a variety of ways, with a remarkable constancy of results, I thought it best to institute a series of experiments, with extracts obtained from different phthisical sputa, by such methods as could be least open to objection in respect of complications introduced from without.

Method.—The sputum was carefully collected in a clean vessel, preferably a closed jar with central hole for the entrance of the expectorated material, such as is used in some of the Edinburgh Royal Infirmary Wards. In the selection of the patient the greatest care was exercised. (*a*) Only such cases were made use of as showed undoubted signs of advancing phthisis. (*b*) No case was accepted where the temperature chart did not record a more or less persistent elevation. (*c*) After the first two or three examinations it was found best to restrict the selection to subjects where possible impurities from smoking were absent.

Similar care was taken in the selection of the sputum. (*a*) The sputum was rejected when any foreign admixture was present, such as vomited materials. (*b*) It was rejected when saliva was present in appreciable amount. (*c*) The reaction of the sputum was tested, and only such admitted as gave an acid or neutral reaction. This last condition was found always associated with a peculiar odor, which may be regarded as *sui generis*. (*d*) The presence, and approximately the relative abundance, of the tubercle bacillus was in every instance ascertained.

The sputum, thus carefully collected for 12 or 24 hours, is at once subjected to further examination. Its bulk is measured, and three volumes of rectified spirit are added to it. The mixing process is carried out *guttatim*, so that the separation of the elements of the sputum may be rendered complete, and the admixture made as intimate as possible. If the sputum be neutral or but faintly acid, a trace of tartaric acid is added to the rectified spirit previous to mixing. The whole is transferred to a Florence flask. Its mouth having been protected by a fine muslin rag, the flask is placed in a Koch's steam sterilizer and exposed to a gentle, moist temperature of 36°–40° C. for 20–24 hours. At the end of this time the fluid is carefully filtered, first, once or twice through fine muslin, and then three or four times through filter paper, till the filtrate runs off perfectly clear. Its volume is then measured, and the whole evaporated down in open beakers to $\frac{1}{15}$ of its bulk (*circa*). This reduces it to the consistence of a more or less muddy extract, varying in color according to that of the original sputum. The latter part of the process is conducted slowly, with the view of driving off all remaining trace of spirit, and to prevent the escape of more volatile products. The extract thus obtained was utilized for injection.

With regard to its constitution, it must be observed that it is as pure an extract as

can well be obtained of the carefully selected sputum. The only additions made are measured quantities of faintly acidulated rectified spirit. This in the process of slow evaporation to $\frac{1}{15}$ its original volume was presumably entirely given off, so that in observing the results we have to deal with the effects of a fairly purified extract of phthisical sputum, *i. e.*, sputum minus the coagulable elements, separated out by the addition of the rectified spirit and the after process of filtration. It should be mentioned further, that the extract, when properly prepared, is most unstable, and being extremely liable to the attack of fungi, breaks down in the course of a few days, giving rise to new products. The extract, therefore, was never used for experimental purposes after it had been prepared for three or four days.

Four series of experiments were conducted with the extract so obtained.

(1) To observe its effects on the system generally.

(2) To observe its effects on the circulation, *i. e.*, on the cardiac rate.

(3) To test the antagonistic effects of certain drugs, especially atropine, as regards the system generally.

(4) To test these antagonistic effects as seen more especially in the cardiac rate.

It is impossible here to give details of the numerous experiments conducted under these heads, but the general results may be summarized.

SERIES 1.

(*a*) *On Frogs.*—Thirteen experiments, carried out with varying quantities and under a variety of conditions, yield results of striking uniformity, and point to the presence in the extract of a toxic principle, or of toxic principles of considerable potency. The results differ only in degree, a progressive increase in the intensity of the symptoms being observable with the increased dosage. The general line of symptoms is that of the gradual development of voluntary motor depression. In no instance was a stage of excitation traceable. This condition of depression appears, in part, explicable by a toxic influence exerted on the higher centres. This is evidenced by the general character of the depression, by the sluggish nature of the movements, while coördination remains little affected, and by contraction of the pupil. The spinal cord appears to be unaffected, the reflexes remaining normal throughout in the less severe cases, and in the graver being unaffected till later on.

(*b*) *On Mammalia.*—In mice it was found possible to induce distinct symptoms with .3 c.c. of the extract. These symptoms resembled in general character those observed in the frog, and passed off gradually in the course of an hour or two. With increased injection the intensity and duration of the symptoms were correspondingly increased. As in the frog, the scope of the symptoms suggests implication more especially of the higher centres. There was the same early appearance of gradually advancing depression. This, as before, was not preceded by any trace of excitation. In the course of ten minutes the animal invariably became quieter, the stage of quiescence passing on to more or less complete passivity and disinclination for movement, according to the amount injected. In the lighter cases, this was recovered from. In the more severe cases, it deepened into death, or death followed after more or less complete approach toward recovery. In addition to these symptoms, common to frogs and mice, certain well-marked phenomena were observed. Among the more striking of these should be noted fibrillary twitching of the surface of the body and convulsive movements of the trunk and limbs. Regarding changes in the respiration, it has to be borne in mind that the estimation of the rate of breathing is always difficult in mice. The general impression, however, was that after the preliminary excitement there remained a certain increase in the respiratory rate, to be followed later, when symptoms were sufficiently prolonged, by retardation. In those

animals which died after prolonged symptoms, anorexia was a conspicuous feature, while water was drunk freely.

In rabbits comparatively large quantities of the extract were required to produce urgent symptoms. On economic grounds this line of experimentation was less systematically carried out. So far as they go, the results obtained were in strict accord with those just detailed. Of greater interest, however, in the case of the rabbit, was the effect of daily repeated small doses. Thus, for example, two rabbits were fed on measured quantities of oats and water, and their weights registered for some days until the daily register became fairly constant. The same conditions were continued, with the addition that once in the twenty-four hours each animal received subcutaneously small injections of the extract. Presumably, as a result of this, their weight progressively decreased by amounts varying from ½ oz. up to 1 oz. *per diem*, and the amount of food consumed was reduced to one-half, and on one occasion to one-quarter the amount previously consumed in the corresponding time. After some days the system appeared to grow more tolerant of the morbid material, as it was found necessary to increase the dose to produce the same effect. At the end of ten days the injections were discontinued, and the weights, without increasing, remained almost constant for a week or two. Then a gradual progression downward, apart from fresh injection, was observed, each animal continuing to lose a fraction of an ounce daily until death. It appears likely that the early loss of weight was due directly to the action of the morbid product, which doubtless led to loss of appetite, etc. This is evidenced by the daily loss of weight, corresponding with the dates of injection, and by the return to a more constant condition when the injections were stopped. The later progressive loss of weight, apart from injection, is more difficult of explanation. We may suppose that, following the earlier injections, a condition of marasmus developed. In neither of the rabbits was there found, on *post-mortem* examination, the slightest trace of caseation, to which rabbits are prone.

SERIES 2. EFFECTS ON THE CIRCULATION, *i. e.*, ON THE CARDIAC RATE.

A considerable number of experiments were conducted under this head. They prove conclusively the presence of a powerful cardiac depressant. In each instance the fall is striking. Where large doses were used it is remarkable; the cardiac rate being reduced under the influence of the extract from 44 per minute to 18 and even 14 in the course of four hours. Coincident with the decrease in weight, a marked lengthening of the diastolic in relation to the systolic phase was evident. These results, taken along with those of Series 4 (*infra*), imply, I think, that the depressant action on the heart is produced through the medium of the inhibitory fibres, and not by direct action on the cardiac ganglia.

SERIES 3 AND 4.

It is convenient, in this brief summary, to combine the results obtained in Series 3 and 4. In each it was endeavored to neutralize the ascertained depressant effects of the extract by the exhibition of presumably antagonistic drugs. For the present, I limit myself to the results obtained with atropine. The double series yield results in remarkable consonance with those obtained in the earlier series. In the first place, they afford strong corroborative evidence as to both the general systemic and the special cardiac effects of the extract. But, in the second place, they prove that the combined exhibition of atropine undoubtedly modifies these results in a striking manner. Of this there is evidence in all the experiments, the degree to which such modification is produced varying with the relative quantity of the antagonistic principle. Most perfect antagonism was produced by the combined injection of $\frac{1}{60}$ milligramme sulphate of atropine with .6 c.c. extract. Under such conditions, the general systemic effects, easily produced both in frogs and in mice by .6 c.c. extract, were almost completely absent,

while the cardiac rate, which .6 c.c. sufficed to depress considerably, remained practically constant. The effects were similar whether the atropine was exhibited simultaneously with the extracts or at varying intervals before or after. The antagonizing influence of atropine is most strikingly demonstrated in those experiments where the injection of the extract preceded that of the atropine by a measured interval of time. In such cases the effects of the extract were, first of all, well defined, and gradually declined on the addition of the atropine. Similar results, though less striking, were obtained when the atropine preceded the extract. It should be added, that in every instance where counter-experiments were made with atropine, the extract was first tested, with the view of establishing its physiological action.

This experimental record is necessarily too brief, and doubtless is open to much criticism. But the results at my disposal, which I hope to publish in more extended form, appear to me to justify the statement that from the tubercular sputum there is separable one or more products possessed of well-marked toxic properties, these toxic properties being more or less completely opposed by atropine.

The remaining question is, In how far this poisonous principle is dependent on the presence of the bacillus? Might not such toxic effects be produced by extracts obtained from other sputa besides those strictly bacillar? There is, unfortunately, no time to give in full the grounds for my statement, and I must content myself with stating categorically my belief, formed on experimental grounds, that the presence of the bacilli is casually related to the poisonous product obtained from the sputum. I incline, also, for similar reasons, to the belief that there is a relation traceable between the toxicity of the extract and the abundance of the bacillar elements discoverable in the sputum.

On the line of absorption and the therapeutic indications, regarding which I had proposed speaking, I must not dwell.

But it may be convenient, in closing, to tabulate shortly the chief points which have been discussed.

(1) In view of the work of Koch, it is impossible to avoid admitting that a causal relationship exists between the tubercle bacillus and the phthisical process.

(2) The mere predication of this relationship is not sufficient in explanation of the clinical facts and the generally fatal termination of such cases.

(3) The usually received explanations of the *modus moriendi* in phthisis are insufficient.

(4) It appears probable that the lethal influence of the bacillus is due to the production thereby of certain poisonous products.

(5) Clinical and experimental evidence appear to indicate that the morbid secretions from the respiratory surfaces afford a good medium for the growth of the tubercle bacillus, and, presumably, for the elaboration of such products.

(6) Such a product is separable from the carefully selected and prepared sputum.

(7) This product is possessed of well-marked physiological properties, being eminently toxic to frogs, mice and other animals.

(8) The toxic properties of the product are, speaking generally, depressant.

(9) More particularly, they include a marked depressant influence on the heart.

(10) This depressant influence seems to be exerted through the medium of the cardio-inhibitory mechanism.

(11) The toxic action of the product is more or less completely opposed by atropine.

(12) The amount of the product which may be separated appears to bear a distinct relation to the abundance of the bacillar elements present.

(13) Absorption of the poisonous product most probably occurs by way of the lymphatic circulation.

EXPERIMENTAL RECORD.

SERIES 3.—Effects of Extract on Circulation, *i. e.*, on Cardiac Rate.

EXPERIMENT XXIII.

(*Condensed Record.*)

Injection of .9 c.c. prepared Extract into posterior lymph sac of medium-sized healthy Frog (R. temp.). Temp. of room = 15.5° C. Heart rate = 46 per minute.

TIME.	RATE.	REMARKS.
P. M.		
12.15	46	
*12.20	46	
12.25	49	
12.30	48	
12.35	46	
12.40	44	
12.45	42	
12.55	42	
12.57	56	Diastole lengthened in proportion to systole.
1.3	28	
1.10	22	
1.15	21	
1.30	21	
1.35	20	Diastole still longer.
2.0	20	
2.10	19	
2.40	19	
3.0	18	
4.0	14	
5.30	14	Killed in moribund state.

* Injection of Extract.

EXPERIMENT XXVII

(*Condensed Record.*)

Injection of .6 c.c. prepared Extract into posterior lymph sac of medium-sized healthy Frog (R. temp.). Temp. of room = 10.5° C Pulse rate = 30 per minute.

TIME	RATE.	REMARKS.
A. M.		
11.0	30	
*11.2	30	
11.5	31	
11.7	31	
11.10	29	
11.15	28	
11.25	28	
11.30	26	Diastole lengthening.
11.35	26	
11.40	25	
11.50	24	
11.55	23	
P. M.		
12.5	23	Experiment discontinued.

* Injection made.

EXPERIMENT XXVIII.

(*Condensed Record.*)

Injection of .3 c.c. prepared Extract into posterior lymph sac of medium-sized healthy Frog (R. temp.). Temp. of room = 10.5° C. Heart rate = 28 per minute.

TIME.	RATE.	REMARKS.
A. M.		
11.0	28	
*11.5	28	
11.8	29	
11.15	29	
11.20	28	
11.25	27	
11.30	26	
11.40	25	Lengthening of diastole.
11.45	24	
11.55	24	
12.0	25	
P. M.		
12.10	26	Frog beginning to grow restless.
12.20	27	Effects appear to be passing off.
12.30	27	

* Injection made.

SERIES 4.—Effects of Extract on Circulation opposed by Atropine.

EXPERIMENT XXXVIII.

(*Condensed Report.*)

Injection of .6 c.c. prepared Extract + $\frac{1}{50}$ milligramme Atropine Sulphate into posterior lymph sac of large, lively Frog (R. temp.). Temp. of room = 15.5° C. Heart rate = 44 per minute.

TIME	RATE.	REMARKS.
P. M.		
12.35	44	
*12.40	44	
12.45	44	
12.53	48	
12.55	46	
1.0	46	
1.8	44	
1.15	42	
1.25	43	
1.35	43	
1.45	42	
2.0	42	
2.5	40	Frequent struggling.
2.10	39	
2.20	39	No marked change in diastole.
2.30	39	
2.40	39	
2.50	39	
3.0	39	
3.15	39	
3.45	39	Struggling frequent.

* Injection made.

EXPERIMENT XLII.

(*Condensed Report.*)

Injection of .6 c.c. prepared Extract into posterior lymph sac of medium-sized healthy Frog (R. temp.), followed in 35 minutes by injection of $\frac{1}{50}$ milligramme Atropine Sulphate. Temp. of room = 13.5° C. Heart rate = 34 per minute.

TIME.	RATE.	REMARKS.
A. M.		
11.0	34	
*11.5	34	
11.10	35	
11.15	33	
11.20	32	
11.25	31	
11.30	29	Evident signs of depression; diastole lengthening.
11.35	28	
†11.40	28–27	
11.45	30	
11.50	33	
11.55	32	
P. M.		
12.10	31	Struggling frequently.
12.20	31	
12.30	32	
12.40	32	
1.10	33	
1.20	34	

* Injection of Extract.
† Injection of Atropine.

EXPERIMENT XLI.

(*Condensed Report.*)

Injection of $\frac{1}{50}$ milligramme Atropine Sulphate into posterior lymph sac of medium-sized healthy Frog (R. temp.), followed in 25 minutes by the injection of .6 c.c. prepared Extract. Temp. of room = 14° C. Heart rate = 34 per minute.

TIME.	RATE.	REMARKS.
P. M.		
12.40	34	
*12.45	34	
12.50	34	
12.55	34	
1.0	34–35	Struggling.
†1.5	34	
1.10	33	
1.15	34	
1.20	34	
1.30	34	
1.40	34	
1.45	33	
1.55	33	
2.0	33	
2.10	33	
2.15	33	

* Injection of Atropine.
† Injection of Extract.

The following paper was then read by the Secretary :—

SOME CONSIDERATIONS ON THE PATHOGENESIS OF DISEASES IN WOMEN.

QUELQUES CONSIDÉRATIONS SUR LE PATHOGENESIS DES MALADIES DES FEMMES.

EINIGE BETRACHTUNGEN ÜBER DIE PATHOGENESE DER FRAUENKRANKHEITEN.

BY WILLIAM B. NEFTEL, M. D.,

Of New York.

At the beginning of my medical career I was in charge of a ward for consumptives (tuberculosis of the lungs) in the large hospital attached to the medical school in St. Petersburg. At that time the absolute incurability of tuberculosis was a firmly established dogma, and any attempt to cure the affection or even to arrest its progress would have brought odium upon the investigator ; certainly, he would have been considered ignorant of scientific medicine, especially of morbid anatomy and pathological histology.

Indeed, since the researches of Rokitansky, the progressively destructive character and the constitutional nature of tuberculosis were well known to every educated physician, and against that generalized destructive process, it was thought, medical science and art must always remain powerless. The utmost that could be done for these unfortunate patients was to alleviate their sufferings by narcotics, and to diminish the colliquative excretions, at the same time carefully watching the progress of the disease to its fatal termination. The greatest care and study were devoted to the physical examination of these patients, and the diagnostic details had to be verified or corrected at the inevitable autopsy. It is true that at the post-mortem sometimes a characteristic cicatrix would be found in the lungs, thus indicating the healing of a former cavity, or, what was still more suggestive, such cicatrices would be found occasionally in the lungs of persons who died of other diseases, and in whom therefore a spontaneous cure of tuberculosis had undoubtedly taken place at some previous period of life. But such cases were considered simply as exceptions to the rule. And yet the truly scientific physician knows that there can be no exception in nature, and if there exist only one case of cure of tuberculosis, a similar result can be obtained in any given number of such cases, provided all the conditions are known under which the so-called exceptional case got well.

Entirely under the influence of the prevailing notions with regard to the incurability of tuberculosis, I met a case in private practice which, by its peculiarity, seemed to throw some light on the etiology of this disease. In a large family, all the members of which were remarkably strong and healthy and in which tuberculosis had never existed for generations, one married daughter, twenty-nine years old, and mother of two children, was afflicted with pulmonary consumption. This patient was endowed, apparently, with the same robust constitution, and even the same features, as the rest of the family ; she was brought up in the same locality and under the same healthy sanitary conditions, and yet was the only member of the large family affected with tuberculosis of the lungs. Her disease was diagnosticated by competent physicians, and the diagnosis corroborated a few years later, at her death.

This patient was for a time under my observation, and gave a detailed account of all the anamnestic data, and even explained the cause to which she attributed the origin of her disease. When a girl she was remarkable for robust health. During several

years, while living in a fashionable boarding school, she wore, day and night, a tightly-laced corset, in order to have a small waist, and was admired for her beautiful figure. She continued to keep on her corset, even while asleep, after having left school, and gave it up only after marriage. Already at school she became pale, and often troubled with a dry, hacking cough, which, after marriage, became more troublesome, and at times was accompanied with febrile movements and increased expectoration. After the birth of her first child the symptoms of pulmonary consumption became manifest. The disease progressed slowly, and at the age of thirty-three the patient died in an advanced stage of general emaciation.

This case, by the simplicity of the conditions under which the disease originated and developed, could be compared to the exactness of a pathological experiment on animals. Indeed, the patient was in perfect health, and without any predisposition to tuberculosis, like all the other members of her family, but became tuberculous after a special condition had been introduced—a condition entirely absent in the others—viz., tight lacing. The positive result of this experiment appeared to me at the time a complete demonstration of the fact that a steady, methodical compression of the chest and of the upper portion of the abdomen, continued for a certain length of time, can produce tuberculosis in a perfectly healthy person without hereditary predisposition to the affection.

Of course, there was no difficulty to find a hypothetical explanation of the fact in accordance with the prevailing theoretical ideas. The compression interfered with the free circulation of blood in the lungs, producing stasis, chronic inflammation and exudation. The latter, according to the existing blood crasis, could become either organized or else transformed into pus or tubercles, etc.

I was determined to test this hypothesis by experimenting on animals, when working in Virchow's Pathological Institute, in 1860. I will give here only a few examples taken at random from my old notes, as all the other experiments present almost identical results.

EXPERIMENT 2. May 22.

I applied a bandage, producing moderate pressure, around the chest and abdomen of a well-nourished black rabbit.

May 23. The rabbit manifests a high degree of dyspnœa, and the conjunctivæ are congested, urinary secretion exceedingly scanty.

May 24 and 25. The symptoms increased in severity; respiration very frequent and superficial; conjunctivæ dark red, forming a thick chemotic wall around the cornea.

May 26. The animal was found dead.

At the autopsy the internal organs, especially those of the abdomen, presented an intense venous congestion; there was dilatation of the right heart and arterial anæmia of all the parenchymatous organs.

EXPERIMENT 3. May 29.

A bandage, with very slight pressure, was applied around the chest and abdomen of a well-nourished white rabbit. The symptoms of dyspnœa, etc., developed more slowly, but in a few days reached the same climax as in the preceding experiment, and June 11 the rabbit was found dead, with the post-mortem appearances as in experiment No. 2.

EXPERIMENT 11. June 12.

A bandage, producing considerable pressure, was tied around a large rabbit.

June 13. The animal breathed with great difficulty, and the bandage was taken off.

June 14. Bandage reapplied quite loosely.

June 23. The rabbit appeared much emaciated, though the morbid symptoms were not acute, and on.

June 29, was found dead. Autopsy: Considerable emaciation of the whole body, arterial anæmia, dilatation of veins and of right heart; in the lungs nothing unusual.

The fatal result was uniform in all the rabbits experimented upon, even when the bandage of the chest and anterior portion of abdomen produced but a slight pressure.

I then tried to entirely exclude pressure, and applied the bandage very loosely. Under this modification the acute symptoms would not appear, and some of the animals lived to the end of the session (August), when they were killed, mostly by Claude Bernard's piqûre; in some, however, death occurred spontaneously. In all of them the autopsy showed *arterial anæmia* and *venous stasis*, with dilatation of the veins and of the right heart. Especially the liver was always greatly congested and enlarged, but nothing of special interest could be discovered in the lungs. Only once a generalized pathological process was found, closely resembling, if not identical with, tuberculosis.

I tried the same experimental method on dogs in Berlin, and continued it in Dr. Harley's laboratory, University College, London, but with a still less satisfactory result. It was almost impossible to keep the bandage in position on dogs, unless made of plaster-of-Paris, and even then the animals would gradually succeed to loosen it to such an extent that a positive effect could not be obtained.

EXPERIMENT 8. May 30.

A plaster-of-Paris bandage was applied, rather loosely, around the chest and anterior portion of the abdomen of a dog. The animal manifested no morbid symptoms.

June 14. The bandage became perfectly loose, and had to be reapplied.

July 4. The dog was found dead. At the autopsy a rather moderate degree of arterial anæmia and venous stasis in the internal organs. At the lower end of the spleen a small infarction the size of a pea. The small intestines were spasmodically contracted. As this was the only case of spontaneous death in a dog after the application of a bandage, I suspected accidental poisoning in the laboratory. All the other dogs were killed in various ways at the end of the session, the autopsy showing in those where the bandage was efficiently applied general anæmia and dilatation of the right heart, with venous congestion of the parenchymatous organs, but in no case was there any special affection of the lungs.

I finally abandoned these experiments, partly because they did not corroborate my original expectation to produce tuberculosis by compressing the chest, but more especially in consequence of Villemin's * researches, which proved that tuberculosis could always be produced by inoculation with tubercular substances. Villemin thus diverted the entire question into a totally different direction, which ultimately led to the discovery of the bacillus tuberculosis.

On account of the negative result of my experiments with regard to the etiology of tuberculosis, I have not published them, although they offer some positive results in other respects. They demonstrate the deleterious effects of a continued compression of the chest and abdomen in animals, even when applied in a moderate degree. It produces general arterial anæmia and venous stasis in the parenchymatous organs, especially in the liver and other abdominal organs.

As regards the question of tuberculosis, the important result obtained by Villemin was soon threatened to be entirely lost by the subsequent work of numerous investigators, who, with faulty, unscientific methods, apparently succeeded to prove that tuberculosis can be produced by inoculation, not only with tubercular substances, but with

* Villemin. "Cause et nature de la tuberculose, etc." *Comptes Rendus*, LXI, 1886. *Gazette Hebdom.* Nos. 48, 49. Villemin's views met with much skepticism at the time, but at present they appear quite prophetic; for instance, the contagiousness and specific nature of the organic poison of tuberculosis multiplying itself in the system, etc.

almost any substance, organic or inorganic. Fortunately, the admirable researches of Cohnheim * proved conclusively that tuberculosis is a specific infectious disease, caused by a distinct living microörganism, present only in genuine tubercular substances, but its actual demonstration was left to Robert Koch. The brilliant researches of the latter show, beyond any possibility of doubt, that the bacillus tuberculosis is the real and only cause of tuberculosis, that without the bacillus, tuberculosis is impossible, and that inoculation with the pure culture of the bacillus always produces tuberculosis. The effect of Koch's discovery on the profession was overwhelming. Not only the diagnosis of the disease became always positive and easy, but it was generally expected that, with the knowledge of the etiology of the disease, the prophylaxis and the treatment of tuberculosis would become successful. In this respect, unfortunately, the general expectation has not been realized, and matters remain exactly as they were before Koch's discovery; now, as before, the fifth part of the population die from tuberculosis.

Moreover, although the bacillus tuberculosis is omnipresent, and hence every one is equally exposed to the infection, yet only certain persons of predilection, those with a so-called phthisical habitus, or *under certain conditions*, are liable to become affected with pulmonary consumption. In other words, besides the bacillus *a certain predisposition on the part of an individual is requisite in order to become consumptive.*

Thus we are practically brought back to the same condition of things which existed previous to the discovery of the bacillus, inasmuch as the primary factor in the production of the disease is predisposition.

So far we are able to form only hypothetical suppositions as to the nature of this predisposition to phthisis. For instance, a person may be considered predisposed when the respiratory mucous membrane is in a state of chronic irritation, and both the local and general resistance are diminished.

The interesting researches of Metschnikoff † and his phagocytic doctrine lend experimental weight and evidence to this hypothesis.

Again, the so-called phthisical habitus—a peculiar, faulty formation of the chest, with weak respiratory muscles and general debility—is justly considered a predisposing cause of pulmonary consumption. Under these circumstances, the lungs not being thoroughly ventilated, the stagnant air and the slow pulmonary circulation afford a favorable condition for the settlement and proliferation of the bacillus tuberculosis.

Perhaps the development of tuberculosis in the case above stated could be explained in a similar way. The prolonged compression of the chest by tight lacing in the perfectly healthy person, seriously interfered with the normal pulmonary ventilation and circulation, and thus produced the same conditions which are favorable for the development of consumption in persons with a congenital habitus phthisicus.

I therefore maintain, though merely as a hypothesis, that compression of the chest by tight lacing, and continued for a certain length of time, constitutes a *predisposing cause* of pulmonary consumption.

As already mentioned, my experiments upon animals show that a prolonged compression of the chest and abdomen invariably produces general anæmia and venous congestion of the liver and other abdominal and pelvic viscera.

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* Cohnheim's first experiments, conducted with B. Fraenkel, which, owing to imperfect methods, had led to the erroneous assumption that tuberculosis was produced by the absorption of inspissated pus, were, later, refuted by himself.

† E. Metschnikoff. "Ueber eine Sprosspilzkrankheit, etc." "Ueber den Kampf der Phagocyten gegen Krankheitserreger." *Virchow's Archiv*, XCVI. "Ein Beitrag zur Phagocytenlehre, etc." *Virchow's Archiv*, CVII. Also "Fortschritte der Medicin," etc.

Analogous results are frequently met with in human pathology. I often had the opportunity, especially in Berlin, to be present at the autopsy of women in whom the liver presented the characteristic malformation known as corset-liver, and of which some illustrations are given in the classical work of "Frerichs on Diseases of the Liver." In accordance with our present state of knowledge, the liver plays an important part in the hæmatopoëtic function, and not only in the formation of blood, but also in its destruction. It is evident, therefore, that the distorted shape of the liver produced by tight lacing must necessarily interfere with its normal functions, and also that a continuous disturbance and obstruction in the system of the portal vein must act injuriously upon the other abdominal organs. Hence the impairment of blood formation, of circulation, of digestion, and the engorgement of the internal sexual organs; the latter condition being the predisposing cause of obstinate chronic affections of the uterus and ovaries.

The so-called corset liver is accompanied by a changed shape of the bony framework of the chest, the ribs assuming a wrong direction, in which they become permanently fixed.

For the sake of brevity, I will not draw clinical pictures of the morbid conditions induced by tight lacing and improper modes of dressing, so much the more, as every medical practitioner is well acquainted with these daily occurrences. It seems necessary, however, to call the attention of the profession to the following experiments which have a direct bearing upon the subject under consideration. Schreiber* was able to produce experimentally albuminuria in persons whom he subjected to compression of the chest. After moderately compressing the chest in young and perfectly healthy persons, even during one and a half minutes only, the urine always contained albumen in quantities varying from a trace to two per cent., according to the length of time the compression lasted. The albuminuria continued after the cessation of the pressure from one to thirteen hours, the urine containing occasionally, besides albumen, hyaline casts, red and white blood corpuscles. Schreiber attributes the albuminuria following compression of the chest to stasis in the pulmonary circulation produced by the diminished respiratory expansion of the lungs and by diminished negative thoracic pressure. This condition leads, secondarily, to stasis in the renal blood vessels, with consecutive alteration of the renal tissues.

There can hardly be a doubt in the mind of the medical profession that women's dress, especially the pressure exercised by it upon the chest and abdomen, must necessarily produce an injurious effect upon their health. Indeed, it would be quite impossible for any one unaccustomed from childhood to the female wearing apparel to bear it for a single day without great discomfort. Only by a constant habit of wearing it a certain tolerance is created, owing merely to the fact that the body adapts itself to the compression, by gradually changing the natural shape of the internal organs, and even of its bony framework, just in the same way as the feet of the Chinese women become distorted by methodical compression. The last comparison is by no means exaggerated, and it can be truly said that the deformed feet of the Chinese women do less harm to their owners than the distorted viscera of our women.

In view of this grave source of discomfort and disease, it is hardly worth mentioning the minor inconveniences of women's dress. Any one, for instance, who would try to wear several hours daily their high-heeled shoes would soon become troubled with backache and general fatigue. I believe a certain kind of headache and facial neuralgia, induced by neuritis, is sometimes caused by exposure of the head, covered only by a fashionable bonnet, in a northern winter.

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* "Ueber experimentelle am Menschen zu erzeugende Albuminurie," *Archiv für exp. Path. und Pharm.*, 1885, III, and 1886, IV.

That such an uncomfortable mode of dressing unfits women for continuous and often hard work is self-evident.

It seems strange that women, even those of great intelligence, do not modify their costume more in accord with hygienic requirements, comfort, æsthetic taste and common sense. Female fashions, as we all know, have their headquarters in Paris, where they are invented by a clique of uneducated dressmakers, totally ignorant of hygiene and devoid of æsthetic taste. From this principal centre the fashion instantly spreads to all European courts and to America, and soon reaches the remotest parts of the civilized female world. All must submit to the dictates of fashion, which is the most absolute of despotisms, and against the decision of which, no matter how absurd and injurious, an appeal is impossible.

That a sanitary question of such magnitude, affecting injuriously half of the population of civilized nations, and capable of transmitting morbid tendencies to future generations, is of vital importance, and deserves immediate attention, will be admitted by every physician.

I believe the only competent authority capable to remedy the existing evil is the International Medical Congress. After having tried in vain to influence the opinion of my own patients, among whom were a number of intelligent ladies, in favor of a healthy reform in the way of dressing, I now take the liberty of proposing to the International Medical Congress to appoint a commission to investigate the whole question of dress for women, and to report at the next meeting of the Congress. Such a commission might consist not only of physicians, but also of pedagogues, of artists and of cultivated ladies from different countries, and would have in view, besides the health and comfort, the æsthetic taste of the sex of which beauty and modesty are the distinctive characteristics. The work of the commission could begin with an inquiry into the different national costumes, which might offer some practical hints, as their usefulness has already stood the test of centuries. Above all, however, the commission could take into consideration the dress of the classical antiquity as the ideal of simplicity, grace and comfort, which, perhaps, would become again a perfect model, when adapted to the changed requirements of modern life and to the various climatic differences.

The inauguration of the much-needed reform in the dress of women by the International Medical Congress, assembled in America, would have a peculiar significance. America has already powerfully contributed to reform in the household affairs of women by the invention and introduction of the sewing, washing and other machines, utensils and appliances which have relieved women of so much tedious work, and have saved time now devoted to physical, intellectual and social culture.

The Section adjourned.

FIFTH DAY.

The Section met on Friday, September 9th, at 11 A. M.

Dr. STUBBS then read the following paper :—

THE RATIONAL TREATMENT OF DISEASES OF THE RESPIRATORY APPARATUS.

LA TRAITEMENT RATIONNEL DES MALADIES DES ORGANES RESPIRATOIRES.

DIE RATIONELLE BEHANDLUNG DER KRANKHEITEN DES ATHMUNGSAPPARATES.

BY GEORGE EASTMAN STUBBS, A. M., M. D.,

Professor of Surgical Pathology and of Clinical Surgery in the Medico-Chirurgical College of Pennsylvania.

I feel considerably abashed at my temerity in undertaking to present, in the presence of so many learned and eminent masters of our art, the result of my studies and experience in regard to so large and important a class of diseases as those of the respiratory apparatus, notwithstanding the sincerity of my desire and purpose to do what little I can to advance the medical art.

I am aware that my presentation of this subject must necessarily be brief, and it may, very likely, have nothing or little of newness about it, and yet every physician ought to bear witness in respect to the rational treatment of a class of diseases which are accountable for the death of about one-fourth of all who die. Thus, according to the returns of the Registrar-general of Great Britain for 1880, we find that during twenty-five years, 1850 to 1874, his calculations show, that 5038.7 per million of persons living died of zymotic diseases, while no fewer than 5840 died of diseases of the respiratory organs (including phthisis and excluding the organs of circulation); moreover, these two classes of diseases, the zymotic and the respiratory, were chargeable with almost half of all deaths from every cause, including accidents.

The respiratory apparatus, filling up the greater part of the thoracic cavity, is made up of these several tissues, viz., cartilaginous, fibrous, muscular, mucous and glandular, each one of which is peculiarly adapted for its portion of the work required of the whole system. Besides filling up the cavity proper of the thorax, the larger portion of this apparatus, viz., the lungs, are so neatly adjusted in pleuræ and chest walls, for expansion and contraction, that their thin apices are habitually raised in full expansion an inch or two above the first ribs, and the lower border descends as low as the eleventh rib.

By the happy blending and disposition of these several tissues in larynx, trachea, bronchi, bronchioles, infundibula and vesicles is formed the great sponge-like system by the contraction and expansion of which the blood becomes oxygenated.

This wonderful expansibility and contractility of the respiratory system is its great

characteristic, and when we study more closely the arrangement of cartilaginous fibres, muscular and glandular structures within the respiratory area, as well as the movability of the chest walls in several directions, and of the diaphragm below, we are filled with admiration at the adaptability of these small structures to the end in view.

The large and extremely minute distribution of vessels, both of the venous and the arterial system, and of the lymphatic system as well, not only in connection with the parts of larger calibre, but also in connection with the smallest cells, clearly indicates to us the possibility of affecting somewhat even the smallest cell, if taken in time, by constitutional methods.

The point to which I desire to call especial attention, however, while studying the anatomy of the parts, though not new, is yet important to the conclusion to which I have been led, and that is the great patency of the air tubes, on account of the large use to which cartilage and elastic tissue of an extremely thin character is put.

We are told by very competent observers that the cartilages, in the shape of thin laminæ and of various forms and sizes, are scattered irregularly, not in the larger tubes, where they are easily made out, but even in tubes the diameter of which is but one-fourth of a line, beyond which point the tubes become wholly membranous. Now, if we only bear in mind that this dimension is about the same as that of our customary script as made with a steel pen, or, to make my meaning still clearer, rather less than the diameter of an ordinary pin, then there will be presented to our minds a better idea of the great minuteness of these air conductors.

The fibrous coat and the longitudinal elastic fibres, also, are further extended into the smallest branches of the bronchioles. The muscular coat of unstriped muscular tissue forms annular layers, extending to the smallest tubes also, and the mucous membrane, covered with columnar ciliated epithelium, lines all the tubes from trachea to air cells, and even in the ultimate air cells it is continued, though becoming much thinner and more transparent.

Elastic tissue makes up a considerable amount of its deepest layer, and lymphoid tissue composes a portion of its superficial layer. As mentioned, the surface of all the mucous tissue is lined with a layer of columnar, ciliated, epithelial cells, the vibratile movement of whose ciliæ is directed in such a way as to pass on to the proximal ends of the tubes the mucus, thus favoring expectoration. The ciliated epithelial cells are supplanted, however, in the air cells themselves, by the squamous variety of cells.

Besides the large amount of expanding and contracting tissue within the air passages, it should be remembered that the lungs themselves are covered by a delicate layer of elastic tissue, sometimes described as a distinct coat, under the name of the second or inner layer of the pleura. I need not halt further to speak of the nervous or vascular supply, the delicate lymphatic system, or the racemose glands of the mucous membrane. But it should be of importance, in making our prognosis as to any one of the diseases affecting the lung cells, for us to bear in mind that the ultimate cells are collected, like families in the state, into lobules, each lobule having its own vessels, and that, further, each cell possesses its own proper plexus of capillaries, so that it may be possible to limit a disease of a single cell or lobule.

The blood and air, it is true, are not in actual contact, but the walls of the cells and of the capillaries are so delicate in structure that they oppose no obstacle to a free interchange of *gases*, by which the blood is oxygenated and freed from carbonic acid and watery vapor.

It is well for us to note, also, the minute distribution of the lymph capillaries in connection with the lobules, by means of which cell depuration goes on.

Pathologically considered, the organs of respiration range naturally into two groups, distinguished by differences both of structure and function. One has to do with the lungs, in which oxygenation of the blood takes place by its chemical interchanges

with the air, the other is the system of air passages connecting the lungs with the outer air.

This system, comprising nose, larynx, trachea and bronchi, may be regarded pathologically simply as hollow passages lined with mucous membrane. And the morbid changes they meet with arise from changes in this membrane.

It is not contemplated in a short paper such as this must be to discuss the treatment of diseases affecting the organs of smell or of the voice, or even, most, numerically, of the so-called diseases of the organs under consideration, which are of less danger to mankind or which result in few or no deaths, but only in discomfort or suffering, but rather would I spend the little time allotted to me in considering the general pathological conditions found in the whole system, and then, more in extenso, those graver diseases which are, perhaps, more an "opprobrium medicinæ" than any other class of diseases.

Among our commonest light affections are those of the mucous membranes of the air passages, when that membrane, which, in the natural state, is nearly white, may take on any of the shades between that and the deepest red, or it may even become almost purple when deep congestion takes place. The free discharge of mucus and lymph is peculiar to this membrane, and serves the double purpose of an aid to the proper function of the parts as well as a protection from external noxious influences. This membrane, placed over the "submucosa" and the loose texture of the movable connective tissue, is very permeable, and thus easily gives passage to white blood corpuscles. In this membrane may occur either congestive or passive hyperæmia or hemorrhages. In them are also found degenerations, atrophies or hypertrophies. The power of repair is exemplified in the rapid restoration of lost epithelium; this, however, cannot take place unless the underlying fibrous structures are complete. If these are destroyed, they must first be renewed by granulation. The simpler epithelial catarrhs may, later, develop into those of a purulent character.

There are many causes of inflammation of the mucous membranes of the air passages, and consequent grave disease to important parts of the system. Thus, we may find the parts affected through hereditary influences, by blood contamination, as in tuberculosis or syphilis, by too cold or too hot air, by irritant dusts, vapors, gases, and it is asserted that in pneumonia, as well as in tuberculosis, microbes are found. That some of the dusts do permeate the lungs has long ago been proved by particles of carbon being demonstrated even on the outer surface of the lungs, and by the discoloration of the lungs of coal miners, and by other colored particles being found there. That is what we ought to expect from the anatomical formation of the whole breathing apparatus. Now, what ought to be our treatment of those graver, more mortal diseases which do so much to swell our death lists, such as pneumonia and phthisis?

In an experience of twenty-four years of practice, both in army and civic hospitals, as well as in the practice peculiar to city physicians, I have, of course, used various modes of treatment both in pneumonia and in phthisis, but now I find that my experience has caused my lines of treatment to crystallize into certain general principles, to be carried out as is demanded by each special case.

Of course, every physician is called to treat certain cases of tubercular and other grave lung disease, where so great changes have occurred that his interference can have but a feeble effect, except to buoy up the patient with hope, prolong his days somewhat and relieve his sufferings. As to the larger number of the remaining cases, I believe that a judicious carrying out of the principles of counter-irritation, support and internal topical treatment can be made to accomplish much. Take the principle of counter-irritation in acute pneumonia, for example: I have used many modes of counter-irritation, but have settled down for years into the opinion that the best counter-irritation is that which affords heat and moisture—a hot, continuous sweating of the parts affected—

such as can best be accomplished by, say, half a dozen thicknesses of thick Canton flannel wrung out, after being thoroughly soaked in hot mustard water, until no more water runs, and then laid as hot as possible over the parts, and covered by a dry compress of from four to half a dozen thicknesses of wool flannel, with oiled silk over all, to exclude the air. There is a right and a wrong way even in applying a compress, but if, *first*, each layer is ready and the parts ready to be exposed for the compress by a mere pull, then everything will be accomplished "cito que et jucunde."

As to support, I include under this term both medication and alimentation, medication must be according to the necessities of each case. In most cases, both in pneumonia and phthisis, I use quinine; sometimes I use the muriate of ammonia, sometimes the wine of ipecac, as an expectorant; but where pain is present and an expectorant is required, I often use Dover's powder. For blood, I employ the best animal foods, but I advise my patients to use much milk and cream. I take care of the digestion and assimilation at all times. As to internal treatment, if my studies and experience have taught me anything, it has been that, in accord with the anatomical, pathological and etiological ideas advanced in the first part of this paper, the local treatment of the parts should be by such medication as will *reach the parts*. This can best be done by the atomization or vaporization of medicated waters by means of atomizers. No doubt many are in use, but I have found Codman and Shurtleff's, of Boston, Mass., answered my purposes, and I have used it with great advantage in cases of chronic bronchitis and incipient phthisis.

As to medicated waters, I have used many, but I find a saturated solution of copaiba balsam exercises a peculiarly happy, healing effect upon these cases, though carbolic acid and iodine are also very useful. By its constant, often-repeated use, I am positive I have cured cases that otherwise would have succumbed.

No doubt in a little while we will have more definite data as to the greater or less advantages of highly-heated vapors and sprays, and other means, such as the use of gases, for reaching minute parts of these organs, but if by these crude ideas I shall have strengthened your faith in this mode of treating diseased parts of the respiratory organs, then I am content.

At the request of Mr. Körosi, a committee was appointed to examine and report upon his statistical paper on the "Preventive Power of Vaccination." This committee made the following report :—

To the First Section of the Ninth International Medical Congress, Washington, September 6th, 1887 :—

In connection with his paper on vaccinational statistics, read at the meeting of September 6th, Mr. Körosi, of Buda Pesth, presented also a number of documents referring to the well-known and much-quoted statistics which were published fifteen years ago by Dr. Keller, the chief physician of the Austrian State Railway. These statistics were among the first which dealt with the influence of age upon smallpox mortality. The most astonishing result of these statistics was that, by omitting children under one year of age, both from the vaccinated and from the non-vaccinated, no influence of vaccination was to be observed, as there died among the vaccinated people 13¾ per cent., and among the non-vaccinated 13¼ per cent. But for some ages there was to be found even a greater mortality among the vaccinated. For instance, the deaths between ages from four to five years among the vaccinated were 20 per cent., but from among the non-vaccinated only 9 per cent.; so that these data appeared to prove not only the uselessness of vaccination, but even the danger of it.

Let us add, that these statistics, especially in consequence of the well-known disci-

pline of the service of the said railroad company, as also in consequence of the very careful arrangement of the schedules, were much praised. The anti-vaccinators, especially Lorinser, of Vienna, and Professor Vogt, of Berne, Switzerland, declared them to be the most carefully prepared and the most trustworthy of vaccinational statistics. These data were also quoted everywhere when vaccination was to be attacked, and even in the German Parliament the leader of the opposition, Reichsperger, quoted these statistics in order to combat the German vaccination act of 1874. These statistics have thus done, and still do, much harm to the cause of vaccination.

Director Körosi, having undertaken a critical review of all the statistical methods which were used up to the present time in defence or in attack of the preventive power of vaccination, had also resolved to examine some of the important anti-vaccinational papers, step by step, to pursue each quoted statistical fact back to its original source, and to ascertain in this manner its reliability. Among those papers was included also that of Keller. When Körosi undertook this troublesome work of investigation, which occupied him for some months, and necessitated an extensive correspondence and compelled him even to make journeys, he had no suspicion that these statistics had been falsified, and he expected that he would be obliged to acknowledge their exactness, exceptional as they were. But the result of this investigation terminated in a quite unlooked-for development.

Körosi, having addressed himself first to Dr. Keller to allow him to come to Vienna to revise the original schedules, found that Dr. Keller had died not long before. He went, however, to Vienna to look into the matter. Here he learned from the successor of Dr. Keller, Dr. Reumann, that Keller had retired from office two years before his death, and that he had taken with him all these official papers. Keller having died in the city of Klosterneuberg, without family, Körosi sought for the documents, but in vain, as all the property had been delivered to kinsmen residing at Prague. Körosi addressed himself now to these, but learned that no statistical papers had been found among his effects. It is then probable that Keller had himself disposed of these important documents.

The correctness of these statements has been verified by us by examining the letters of Dr. Reumann, of the burgomaster of Klosterneuberg, and of Professor Erben at Prague, the latter having made inquiries of the heirs of Keller.

Director Körosi, having thus far been baffled in his search, addressed letters to all the railway physicians who had furnished, in 1872-3, statistics to their chief at Vienna, asking them if they had perhaps duplicates of their statistical reports. Out of nineteen physicians still living eight were able to send duplicates, and Director Körosi was thus enabled to reconstruct the railway statistics of 549 cases of those reported in Keller's brochure.

Before presenting to the Section the results of our labors, we desire to say some words as to the accuracy of the original reports. Far from being perfectly accurate, it must be admitted that they are the very opposite, and that in the following respects:—

1. The alleged superiority of the Keller's statistics was ascribed to the circumstance that, according to the circular order No. 30,593, 1872, of the Vienna office, the physicians had to note during the epidemics of 1872-3, in each case of smallpox, not only whether the patient had been vaccinated or not, but also whether he had been re-vaccinated, if he had smallpox before, or if vaccination could not be ascertained, and besides all this, the age, and this with great exactness. Thus, for instance, in case of sucklings under one year the number of months. Now we have had in our possession this circular No. 30,593, and have found that its date is toward the end of the year, that is, the 19th November, 1872. How could these physicians have furnished all the required statistical data concerning the persons who had been treated during the time before this ordinance had been published, especially when we take into consideration that the

working people on the railways represent a very fluctuating population? The required data could have been possible only if the register of patients had contained columns for indicating these data. But we have had in our hands a duplicate of these older registers in the handwriting of Dr. Borbely, chief physician of the Hungarian lines of the said railroad company, and we can affirm that they contain no column for these data, and that consequently this extract of the register of Pesth, containing all the cases of smallpox which had occurred there in these two years, shows that not in a single one of these cases the fact of vaccination, re-vaccination, etc., had been noted.

2. The fact that the physicians knew very well that the chief Medical Officer was an anti-vaccinationist renders it not unreasonable to infer that they acted under pressure. We have also seen the letter of one of the physicians, who confesses that, "inter nos sit dictum, the data were prepared in conformity to the taste of their chief, whom he knew to be opposed to vaccination." We can thus state that the much praised source of the Keller reports has been found a very impure one.

But even these inexact statistics furnish a proof in favor of vaccination. The data reconstructed by Director Körosi lead to the following results: Of the vaccinated, died 8.82 per cent.; of the non-vaccinated, died 19.23 per cent., that is more than double the number of the former.

From the paper read by Körosi in Section One we learned that in nineteen Hungarian hospitals, where the registration was exact, eight times more died of the non-vaccinated than of the vaccinated; but the incorrectness of these railway statistics caused this advantage of the vaccinated to be reduced one-half, while under the hands of Dr. Keller this advantage was reduced to zero. How was this accomplished? We beg to tender you the explanation of this fact. Keller had actually altered the statistics of his physicians, which he should have only compiled.

Let us give only one instance:—

The railway company is proprietor of a great mining colony, called Steierdorf, in the southeast of Hungary. The physician of this colony, Dr. Pichler, sent to Director Körosi a duplicate of his statistics, according to which there had died out of the vaccinated 3.8 per cent., but out of the non-vaccinated 34 per cent., that is, nine times as many. In Dr. Keller's paper we find it reported thus: There died among the vaccinated 4 per cent., among the non-vaccinated 20½ per cent.; so that the difference is simply quadruple. This result was produced by raising the number of deaths in the column of the vaccinated, and by changing in the same direction the number of the patients. For Dr. Pichler reported that among 38 not vaccinated, 13 died—34 per cent., and Dr. Keller changed this in the following manner: Among 68 not vaccinated 13 died—20½ per cent.

From the correspondence submitted to us, we find that Körosi informed Dr. Pichler of the great discrepancy between these two statistics; he called his attention to the fact that in the trial, Keller *versus* Jenner, Keller ought to be impeached for falsification of statistics; that he (Dr. P.) would be in the witness box before the tribunal of an International Congress; that he should therefore revise his registers once more, and perhaps he would find some mistakes. We have seen the answer of Dr. Pichler, dated from Steierdorf, the 4th March, 1887, in which he says that he did not know anything about the paper of Keller; and declared that his own data are true, and accord entirely with his registers of sick and dead. Besides this, it may be mentioned that Dr. Pichler also had sent, in 1873, his statistical reports to the county authority at Lugos, so that the possibility of errors is entirely excluded from his own data.

But your committee have also had in hand the answers of all the other physicians of the railway company, in all eight letters. We are convinced that in each of these, without exception, Dr. Keller had changed the genuine data, and that always in such a manner as to increase the mortality of the vaccinated, and to diminish that of the non-

vaccinated. We further aver that in some cases, also, as in that of Olmutz, where all the patients had been vaccinated and all recovered, Dr. Keller simply omitted all mention of the report and of the facts; for we have sought in vain for the statistics of Olmutz in Keller's brochure.

In conclusion, we are forced to declare that the statistics of Dr. Keller have been found by us to be false; that these statistics are an unpardonable effort to mislead public and scientific opinion, and that henceforth no weight should be attached to them, having been proved by us to be entirely incorrect.

JOHN A. OUCHTERLONY, M.D., Chairman.
Professor Principles and Practice of Medicine and Clinical Medicine in the University of Louisville, Ky.

THOS. B. LESTER, M.D.,
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A. B. ARNOLD, M.D.,
President of Section of General Medicine.

Sir JAMES GRANT, of Ottawa, Canada, then made some remarks on the subject of Diphtheria:—

DIPHTHERIA AS OBSERVED IN THE OTTAWA DISTRICT DURING THE PAST TWENTY-FIVE YEARS.

DIPHTHERIA COMME OBSERVÉ DANS LA DÉPARTEMENT D'OTTAWA DURANT LES VINGT-CINQ ANNEES PASSÉES.

BEOBACHTUNGEN ÜBER DIPHTHERIA IM DISTRIKT OTTAWA WÄHREND DER LETZTEN FÜNFUNDZWANZIG JAHRE.

BY SIR JAMES GRANT, M.D., F.R.C.P., LOND.

During the past twenty-five years two epidemics of diphtheria have been observed in Ottawa and the surrounding country. In the spring of 1862 diphtheria of a malignant character became epidemic in both city and country, and the death rate was exceedingly high, notwithstanding all the care and attention that could be bestowed upon it. In 1879, a second epidemic, of a much milder character in every particular presented, in which, owing to its peculiar type, the death rate was much lower, and, in fact, it gave way more readily in every particular to local and constitutional treatment. So far I have observed but three forms of this disease—the mild or catarrhal, the inflammatory form and the malignant form. In the latter, treatment in the great proportion of the cases appeared to be of little avail. The physical character of Ottawa City and the surrounding country is most conducive to health, having the great Lawrentian range of mountains on the north, with intervening sections of country on the Gatineau, Blanche and Le Lievre rivers, well watered and possessing good agricultural capacity. The city of Ottawa rests on a limestone basis, elevated considerably above the level of the Ottawa river, and remarkably well situated, as far as health is concerned, being well drained and supplied with an abundant flow of pure water during the entire year. Notwithstanding these important facts, our city and the surrounding country for an area of over one hundred miles suffered considerably from these two epidemics of diphtheria.

Prior to 1868 the disease was not known in this section of the country. I have not observed this disease to exist more in the winter than the summer months, and in those winters marked by a large snowfall and dry, bracing weather, the ratio of disease is usually smaller than in mild winters with moist weather. Those sections of the Gatineau visited by diphtheria are well drained and the inhabitants careful as to the principles of sanitation. Diphtheria I consider as a blood disease, with a determination to the throat first and the nerve centres afterward. Under these circumstances I have modified my ideas as to its treatment, and for some years past have used mustard baths vigorously once each day (a tablespoonful of mustard to an ordinary hot bath) until such time as the parchment throat membrane gradually becomes detached and thrown off. In addition, the throat is brushed two or three times daily with glycerine and tinct. ferri ʒ ij to glycerin ℥ j, the system well supported with milk, beef tea and occasional stimulants, as required. The usual mixtures given internally consisted of potass. chloratis, tinct. iron and liq. ammon. acet., as suggested by Sir Morrell McKenzie, and with modifications as circumstances dictated. Under this plan of treatment the success was most marked, particularly when the disease was attacked in its incipient state. The poison of the blood is deflected from its point of determination, the throat chiefly, and thrown off by the great emunctory, *the skin*. Again, the simplicity of treatment places it within the control of the country classes, where the disease, as a whole, is most prevalent. From careful observation in hospital and private practice, I have come to the following conclusions :—

1. That diphtheria is a blood disease, with a determination chiefly to the throat, forming a specific membrane.

2. That it prevails more in moist than in dry weather.

3. That during the winter season, the greater the snowfall the less the appearance of this disease.

4. That owing to its secondary interference with the ganglionic nervous system (particularly cardiac), great care is necessary in locomotion for several weeks after the disappearance of the throat disease.

5. That the most careful isolation should be introduced on the first appearance of the disease.

6. That hilly districts in the country are preferable to the seashore as convalescent resorts, in diphtheria.

7. That children giving evidence of muscular atrophic conditions as secondary results from this disease, should not undertake ordinary school work until quite restored to normal muscular vigor.

Dr. Grubb, of London, read the following paper :—

A NEW AND IMPROVED FORM OF UREOMETER.

UNE NOUVELLE FORME IMPROUVÉE D'UREOMETRE.

EIN NEUER UND VERBESSERTER UREOMETER.

BY DR. ALFRED S. GRUBB,
Of London, England.

This instrument was devised by Dr. A. W. Gerrard, F.C.S., of University College, London. It was designed to enable the practitioner to estimate promptly, easily and accurately the proportion of urea. In this apparatus all calculations have been rendered

unnecessary, the various sources of error having been obviated or allowed for. The reading on the graduated tube may thus be taken to give at once a correct rendering of the percentage of urea contained in a given specimen of urine.

The apparatus consists of a glass tube about 12 inches long, fixed in a wooden socket. This is graduated to represent the percentage, and is subdivided into tenths. Below, it communicates by means of an india-rubber tube with a reservoir arranged so as to slide up and down parallel with the graduated tube. This is to hold the displaced water. In the bottle which accompanies the apparatus is placed a test tube containing exactly 5 cubic centimetres of urine (indicated by a mark), and 25 c.c. of a saturated solution of hypobromite of sodium is then carefully poured in. The various stoppers are then firmly adjusted; the reservoir, filled with water, is pushed upward until the level of the water in the graduated tube is at zero, and, finally, the loose tube at the top of the latter is clipped, and so secured. The bottle containing the urine is then gently tilted so as to mix it with the hypobromite of sodium, and when the reaction is complete (in about two minutes), it is immersed in cold water to avoid any separation by heat, and the reservoir is lowered until the two volumes of water are again equal. The result may then be read off at once.

The principle is that of the decomposition of urea by bromine into CO_2H, water and nitrogen, the amount of the latter being taken as the indicator.

The whole proceeding does not require more than three minutes to carry through, and no technical knowledge is required to work it.

The Section then adjourned.

AFTERNOON SESSION.

The Section met at 3 P. M., and the following paper was read by title—the author being absent—and referred to the Transactions :—

MALARIAL HÆMATURIA.

HEMATURIE PALUDIENNE.

MALARIA-HÄMATURIE.

BY W. O'DANIEL, A. M., M. D., PH. G.

This very peculiar type of malarial disease is comparatively new in the Southern States, and elsewhere, so far as we are aware.

From the best information we can obtain, a well-authenticated case had not been known, at least in Georgia, until 1868, but since that time, in sections intensely mala-

rial, its annual visitations are constantly and distrustfully expected in the autumn months, and in localities subject to malignant forms of malarial fevers our expectations have been fully—we regret to say painfully—realized, because of the great fatality attending the worst forms of this fever.

That this particular phase of disease did not appear until about 1868 has been established by written evidence, now in our possession, by medical gentlemen of eminence and distinction in Georgia, experienced, too, in the treatment of malarial disorders in all of their different forms and stages.

As this form of disease is unknown in non-malarial districts, literature, although immensely important, is very scarce. When this malarial complication first appeared, the symptoms so closely resembled epidemic yellow fever as to be so considered by physicians of ability and experience in some localities.

Close and patient study, however, of the causation, etiology and pathology of this complication soon educated the discriminating medical mind to accurately diagnosticate cases of this character with absolute correctness and precision. The unchecked ravages of malaria (so-called) frequently cause in certain localities favorable to its production, both directly and indirectly, a diseased state of the human system, upon which supervene maladies very fatal to its unfortunate victims, and the disease in question is a striking illustration of this fact. No matter whether the cause be cryptogam, spore, germ, bacteria, miasm, bacillus malariæ, or whatever else now unknown to the scientific and well-educated medical mind, we are satisfied that there is an invisible something that causes these changed conditions, and that they do occur, and occur only where we have heat, moisture and vegetable decomposition in abundance; for it does take an overdose of this poison to produce the hemorrhagic condition.

We generally find these morbid conditions near low, damp, marsh and bottom lands, which are subject to frequent inundations and overflows during the summer months. Such lands, since the war between the States, have in many places been neglected; that is, have not been properly drained by ditching for agricultural purposes and sanitation, as before the war, when forced labor was more efficient than the unreliable hired labor of to-day. Therefore, we are of the opinion that this new morbid state is purely malarial, and is caused by a superabundance of malaria, which we did not have two decades ago. This may be considered hypothetical; but why so? When we have had in these very locations intermittent, remittent and bilious fevers from time immemorial, without this dangerous complication, now so disastrous to health and destructive to human life; or, if you please, this excess of vegetable decomposition may have brought forth a new germ; hence, the new type. We are not much inclined to the latter theory, however, because we regard the hemorrhagic condition a symptom rather than a disease *per se*, for the reason that it is always the result of malarial poisoning, and never occurs except in anæmic patients, whose blood corpuscles have been disintegrated by malarial complications. In districts where this superabundance of malaria exists, the blood becomes impoverished and deficient in some of its normal constituents, and perhaps retaining others that should have been eliminated, and this changed by the absence of some ingredients, and the presence of others not properly belonging to it. That it is, strictly speaking, no longer blood. While this life-sustaining fluid is in this abnormal and poisoned condition, caused by malarial influences and neglected chronic relapsing intermittents, this peculiar illness quickly develops, and with these malarial complications the lives of those attacked are dependent mainly upon the malignity of the disease and the vital powers of life to sustain them. We have seen death ensue, to all appearances, just as a woman dies from post-partum hemorrhage; or death from accidental hemorrhage. The impression upon the constitution and upon the circulation is apparently almost identical. Frequently those attacked with hæmaturic fever actually die so suddenly, in a state of congestion and

malarial stupor, as to leave no time for human effort to assist nature in bringing on reaction, and thus the end comes, apparently, from sheer exhaustion and for want of normal blood. Of course, there is in the system an ample supply of this abnormal fluid just described, but the toxic changes have rendered it unfit as a life-sustaining and vitalizing fluid.

In such cases we are almost persuaded that if ever transfusion was indicated, immediate recourse to it should be had. We do not desire, however, to be understood as recommending transfusion as a common remedy in malarial hæmaturia. It is now a fact well established, that only enfeebled and broken-down constitutions, from repeated attacks of intermittents and their usual recurrences, have very nearly always been the victims, and the severity of the disease always in a degree commensurate with the intensity of previous sufferings and amount of exposures.

The uninterrupted and constant prey of this invisible malarial element upon the nervous system and upon the spleen and liver, and, indeed, upon the whole organism, will inevitably, sooner or later, so sap the constitutions of those within easy reach, as to cause them to succumb to diseases which would otherwise be controlled.

How often have we sorrowfully and hopelessly witnessed this sad spectacle, despite the best efforts and skill of the most distinguished and learned disciples of Æsculapius. The frequency of these troubles could be lessened, and sufferings ameliorated, and possibly averted, by properly observing and enforcing the laws of hygiene, and adopting in time the necessary prophylactic measures, thereby preventing attacks of intermittent, remittent, bilious and pernicious fevers. If preventive means are not employed, the systems of those in malarial districts will eventually become so saturated with this morbid element, so dangerous to health, as to finally culminate in the speedy development of what is now known to medical men in malarial sections as malarial hæmaturia, or malarial hemorrhagic fever, a disease which, in appearance, closely resembles epidemic yellow fever, and would be so designated by any intelligent physician unacquainted with malarial hæmaturia, or, if you please, malignant bilious fever—for, in our judgment, it is really nothing more.

In the severest and most virulent form, it is almost a symptom of dissolution. Then, with these facts before us, it is all-important that the premonitory symptoms of the various forms of malaria should receive much more prompt and timely attention than is usual, even by competent practitioners of medicine, in order to prevent the insidious and often unsuspected inroads upon the system, and before the work of degeneration is so far in excess of that of reparation as to weaken the vital powers of life to such an extent as to render recuperation hopeless, even when patients are under the care and observation of medical attendants of unquestioned skill and acknowledged ability. Much more can be accomplished by way of preventing this fearful malady than in relieving patients already sick with it. This is best done by preventing intermittents and their recurrent attacks, and the anæmic state generally.

When this is successfully done, we need have no fear of malarial hæmaturia, for it is only the sequel of some of the various forms of malarial poisoning.

When avoidable, we should never reside near these chill-producing influences, and certainly we should not sleep at night exposed to miasma, as that is the most dangerous exposure possible. We have known laborers to work on fertile farms, surrounded by or near these miasmatic swamps, when this rapid vegetable decomposition was going on during the summer months, and sleep away from these influences at nights, and enjoy complete immunity from malarial fevers; and, again, we have known people to reside all the time under these toxic influences, and keep the doors and windows of the sleeping apartments closed from sunset to sunrise during the sickly season, and also enjoy, at least partial and sometimes perfect, immunity from chills and fever.

If it is a fact, then, that it is the night time when we receive into our systems this

invisible, morbid something called malaria, or miasm or germ, and if we are so situated as to be obliged to submit to exposures of this kind, the poison must be neutralized by the proper remedies as fast as it is taken into the system. We have known this to be effectually done, in many instances, by the taking of five grains of quinine on retiring at night. This to be continued, however, from early spring until frost.

Should a chill occur, never wait for a second one before commencing treatment to anticipate it.

The antidote is quinine or some of the alkaloids of cinchona. Although quinine is the best-known antidote for malaria, it will not always prevent recurrent attacks unless long and judiciously continued. Remedies for the relief of hepatic, renal and splenic complications should be employed, together with good tonic treatment, to insure residents of these fever districts even partial immunity from intermittents, and their recurrent attacks on the seventh, fourteenth and twenty-first days.

A well-marked chill or rigor generally, but not always, develops this disease so plainly, by the sudden change of the normal urine to that of a dark, blood-like fluid, and the whole cutaneous surface to a jaundiced appearance, that the diagnosis is unmistakable, even with the unprofessional who are acquainted with it. This remarkable change so soon after the chill, we suppose to be caused by an occlusion of the ductus communis choledochus, on account of which there is an effusion of bilious matter through the whole system. In violent cases patients frequently lose their mental faculties, remain in a state of partial delirium, with, perhaps, lucid intervals, until the crisis is over, while the minds of others remain clear all the while. These conditions are dependent entirely upon the virulence of the disease. Again, we have seen a few cases so light as not to confine patients to their beds. This is very unusual, however.

Physicians who are not familiar with malarial hæmaturia are sometimes liable to be mistaken, as all cases, even in sections where the disease is common, are by no means malarial hæmaturia. Although the hemorrhage may be present, and those attacked may have been exposed to the very influences that produce it, we have seen cases the cause of which was from overdoses of turpentine and from traumatism, and some from specific causes, etc.

In the treatment of this disease we must endeavor to restore the secretions and relieve the hepatic, splenic and gastric congestion, for these generally are present, and greatly interfere with successful treatment. The symptoms are generally so prominent that a mistake in diagnosis, after once having seen a well-developed case, would be unpardonable, and are generally as follows: The whole cutaneous surface will be found to be of a deep yellow, liver and spleen enlarged and tender upon pressure, from malarial engorgement; tenderness over epigastric region, and also over region of kidneys; conjunctiva almost as yellow as gold; furred tongue, of a dark hue; in violent cases semi-comatose condition, general corporeal coolness, with feeble circulation, vomiting black matter, somewhat like coffee grounds; urine of a dark red color and very copious; temperature from 102° to 105°; circulation when first attacked very rapid, if reaction is established soon after chill or rigor, which generally ushers in the attack. Very frequently reaction cannot be established, and the body remains cool, with firm and cool perspiration, and general congestion and malarial stupor, until death, which very soon occurs in desperate cases.

There seems to be considerable congestion of the duodenum, and, perhaps, duodenitis, and, as before said, supposed occlusion of the ductus communis choledochus, which prevents the free passage of bile, nature's own cathartic, into the duodenum, the result of which is an effusion of bilious matter through the whole system.

The symptoms are always intensified, according to the severity of the attack.

After the secretions have been attended to by the proper remedies (and perhaps mercury is best for this, for it is best tolerated by a nauseated stomach), promote rest and ameliorate nausea by morphine hypodermically, and beware of too much and too frequent drugging, and especially excessive purgation. As the disease is of malarial origin, of course quinine is one of our available therapeutic agents. We give it to neutralize malarial poison and to prevent recurrent chills or rigors. We give it to equalize the circulation and lower the temperature; hence, the necessity for a sufficiency of quinine to accomplish these ends, if possible, and no more.

The idiosyncrasy, temperament, constitution, heart's action, etc., of each individual patient must be carefully considered before the administration of appropriate remedies, and supportive treatment must not be forgotten. We believe quinine to be a heart depressant and a nerve irritant, and will increase gastric irritation, which is generally a very annoying symptom and greatly interferes with treatment, because the stomach fails to retain the remedies.

Dilute nitric acid or sulphuric acid in ice water is pleasant and refreshing, and has a decided tendency to check the much-dreaded hemorrhage.

If uræmic poisoning is suspected, then it is especially indicated.

If quinine has been taken, its solubility will be rendered certain, and its absorption facilitated.

Blistering over epigastrium with cantharidal collodion modifies duodenitis and ameliorates gastric irritation.

Carbolized emulsion (a mixture of bismuth, carbolic acid and glycerine) is a very efficient remedy in gastric troubles, so is oxalate of cerium.

Hyposulphite of sodium, in ten to fifteen-grain doses three times daily, assists nature in restoring the kidneys to normal action. Chloride of iron is often necessary in anæmic patients. Ergot is sometimes indicated.

Hæmostatics must be employed with caution, as suppression may be induced by their too liberal use, which is a very unfavorable symptom in hemorrhagic fever.

To better illustrate, I will give the history and treatment of cases in my practice.

On the night of January 30th, 1886, I was called to see Willie Lawrence, a child but little over two years old, who had, only a few hours previous to my visit, had a chill, and afterward passed (so-called) bloody urine very freely. Upon examination and inquiry, I found that the little fellow had for some time been suffering from recurrent attacks of intermittent fever. He had for a day or two previous to his chill been suffering from diarrhœa; therefore, I deemed it unwise to interfere in any way with his bowels, as the diarrhœa had only been checked a short time. He had all the symptoms of malarial hæmaturia; that is, the vomiting of very dark bilious matter, yellow skin and coolness of body, enlarged liver, very restless, and partial delirium, etc. Stimulants were given and rest promoted by small doses of morphine. Quinine was ordered, to anticipate an expected paroxysm, and aromatic sulphuric acid frequently, in cold water, to dissolve the quinine, which had been given per orem, and to check the excessive hemorrhage.

July 31st. Reaction well established, but symptoms of suppression were present, and I advised squills, digitalis and sweet spirits of nitre, and continued quinine, to equalize circulation and prevent chills. The urine was clearing up and prognosis favorable.

February 1st. Patient doing well, urine normal, and only slight fever. Discontinued digitalis mixture.

February 2d. Patient able to sit up, and much better. A tonic of precipitated carbonate of iron, quinine, whisky and water was ordered. Under this treatment a speedy recovery ensued. The tonic, however, was continued, to prevent recurrent chills and fever, and by doing this I felt sure that a second attack of the hemorrhagic symptoms *would be* averted.

On September 25th, 1886, Mr. Lowe, 26 years of age, a man of feeble constitution and a long sufferer from bronchial and pulmonary troubles, whose residence was a favorable one for malarial diseases, and whose blood had become to some extent defibrinated and impoverished to such an extent as to render him a fit subject for malarial hæmaturia, was the unfortunate victim of this malady. His case was of the most violent form.

He died in less than twenty-four (24) hours after the attack, which was ushered in by a chill, congestion, gastric irritation, vomiting, copious hemorrhages from the kidneys, and the whole cutaneous surface as yellow as gold.

That the skin should undergo such a remarkable change in so short a time may seem to those unacquainted with the disease incredible, but it is, nevertheless, true. Circumstances over which I had no control prevented me from being with the patient until just before he died.

On the 9th of November, 1885, we were called to see Mr. Faulk, in consultation with Dr. Sloppey.

Mr. Faulk was about 26 years old, of vigorous constitution, who had always enjoyed perfect health until a few months before his attack. Although he resided in a healthy locality and one free from miasmatic influences, he contracted chills and fever from frequent exposures in the swamps while hunting during the night time in the autumn months. He was attacked as violently as the case of Lowe, just mentioned. Notwithstanding his former good health and vigorous constitution and almost unprecedented strength, he came very near succumbing, but after a long and tedious combat with disease he recovered under treatment advised before in this paper.

This case plainly illustrates the fact that night exposures are dangerous in malarious districts, even if residence is in healthy localities.

The first case mentioned was the youngest I have ever seen with hemorrhagic fever. So far as my own personal experience goes, the negro is exempt from the hemorrhagic condition caused by malaria. We have known but one mulatto to have the disease.

The PRESIDENT of the Section then read the following paper:—

CLINICAL OBSERVATIONS ON DILATATION AND DEGENERATION OF THE HEART, WITH SPHYGMOGRAPHIC ILLUSTRATIONS.

OBSERVATIONS CLINIQUES SUR DÉLATATION ET DÉGÉNERATION DU CŒUR, AVEC ILLUSTRATIONS SPHYGMOGRAPHIQUES.

KLINISCHE BEOBACHTUNGEN ÜBER ERWEITERUNG UND DEGENERATION DES HERZENS, MIT SPHYGMOGRAPHISCHEN ABBILDUNGEN.

BY A. B. ARNOLD, M.D.,

Professor of Clinical Medicine, College of Physicians and Surgeons, Baltimore.

Among the different forms of organic diseases of the heart there are none which give rise to more constant and distressing symptoms than dilatation of its cavities and nutritional changes of its muscular substance. The disordered mechanism of the centre of circulation due to these anatomical alterations induces a wide range of functional disturbances, and in addition a train of adventitious disorders, which often present considerable difficulties to diagnosis. It is a matter of daily experience that patients suffering from some form of valvular disease do not seek medical advice until the obtrusive symptoms of dilatation or myositis make their appearance. This always happens where the consequences of general venous congestion are rendered palpable.

Many years may elapse before aortic regurgitation or mitral insufficiency causes the patient much discomfort. Things at once change for the worse whenever the effects of a weak or dilated heart make themselves felt.

Dilatation of the heart may occur independent of any pathological change. Probably the faulty condition of the organ in this class of cases is analogous in character to that form of chlorosis which Virchow attributes to defective development of the arterial system.

It is well known that anæmia alone may cause dilatation. A weak, ill-nourished heart, incapable of energetic contractions, possesses little resisting power against the distending force of the blood during the diastole. Attenuation of the ventricular walls and gradual enlargement of the cavities may thus be established.

Dilatation is also the natural result of fatty change affecting the muscular substance of the heart. Such a histological deterioration is not clinically recognizable; but the signs and symptoms which become manifest point to the probable cause.

Obstructive disease of the lung and heart produces by far the greater number of cases of dilated heart. In pulmonary affections, for obvious reasons, the right side of the heart is exclusively involved. An emphysematous lung, or a lung affected with chronic bronchitis, or a large pleuritic effusion, reduces the area of distribution of the pulmonary circulation. The right chamber of the heart is in a constant state of distention, and systemic congestion invariably follows. The same disordered condition of the circulation, but in an aggravated form, ensues in tricuspid insufficiency, and in an indirect manner in valvular disease of the left ventricle. In general, it may be said that the most formidable effects of valvular disease of the heart are observed in cases where either structural changes had destroyed the compensatory hypertrophy of the ventricle or where the dilatation of its cavity ensued from simple attenuation of its walls. The hypertrophy may suffice to reduce the symptoms of incompetent valves or orifices to a minimum; but any excess of the capacity of the cavities, in proportion to the thickness of the walls, elicits more or less the morbid signs of venous stagnation. In well-marked cases of general dilatation the valvular murmurs are usually no longer audible; the impulse of the ventricles is scarcely perceptible; the sounds of the heart are very faint, and the contractions exceedingly irregular. Between these extremes, every conceivable grade of dilatation may be encountered. In regard to the physical signs of dilatation, it may be remarked that the increase of the cardiac dullness in the horizontal direction is only evident in hypertrophous dilatation.

The foregoing brief statement of familiar facts is intended to introduce related observations which this paper proposes to discuss.

Although our therapeutical resources fall short in contending against the deranged mechanism of the heart, yet a judicious treatment frequently succeeds in mitigating its symptoms, especially when directed in warding off the danger arising from dilatation. It appears to me that a more general use of the sphygmograph may be advantageous in determining with some degree of precision not only the existence and progress of the dilatation, but also the indication for the employment of the reputed remedies. Though the sphygmograph conveys no information of the amount of work done by the heart, yet it gives an estimation of arterial tension; hence the state of tonicity of the arterial walls may be taken as an approximative index of the energy of the heart's action. Pond's sphygmograph, which I use, possesses the advantages of portability and easy adjustment, which atone for the want of the more acute tracing of Marey's instrument.

The following graphics, Nos. 1, 2, 3, 4, are taken from healthy persons of different ages :—

These tracings present all the peculiarities of the normal pulse curve. The vertical line, or *upstroke*, registers the arterial diastole which immediately succeeds the contrac-

tion of the ventricle. The elevations of the descending line, that show the undulatory movement of the blood in consequence of the elasticity of the arterial walls, are fairly well brought out. It will be observed that the second, or so-called *dicrotic*, wave, which is due to the rebound of the blood on the semilunar valves, is the only elevation recorded in one of the graphics. But this is not unusual in normal pulse curves, provided that the said wave retains in the tracing its proper distance from the summit and the base of the curve. It will also be noticed in the several figures that the series of curves occur at regular intervals and are equal in height.

FIG. 1.

FIG. 2.

FIG. 3.

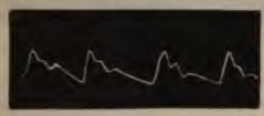
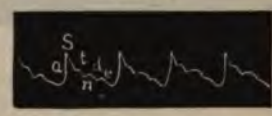


FIG. 4.

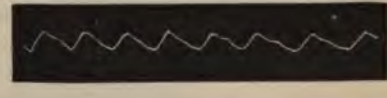


a, ascending line or upstroke; *s*, summit or apex; *t*, tidal or primary wave; *n*, aortic notch; *d*, dicrotic wave or recoil elevation; *e*, secondary wave.

Fig. 5 represents the pulse curve of a very anæmic woman past the menopause. She complained of fluttering in the region of the heart; shortness of breath; frequent gaping; faintness; epigastric uneasiness and anorexia. Her mind was much depressed, and of late she finds herself unfit for any physical or mental exertion. Her pulse was extremely weak, slow, and very compressible, but showed no irregularity. Her lips had a bluish tinge, and the feet were slightly œdematous. The heart's sounds were faint and distant, but there were no murmurs. She had taken large quantities of steel medicine without much benefit. The appearance of the pulse tracing induced me to believe that the patient's symptoms were aggravated and rendered obstinate by the complication of attenuated heart. The figure shows a short *upstroke;* the summit is rounded; there is an entire absence of elevations in the descending line, and the curves occur at unusually wide intervals. In combination with the iron I prescribed large

FIG. 5.

FIG. 6.

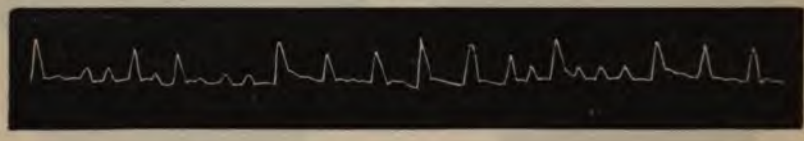


doses of caffeine. At the end of about six weeks from the commencement of this treatment I got the annexed graphic (Fig. 6).

The general improvement of the patient is well depicted in this tracing. I am aware that the diagnosis of dilatation in this case may be disputed; but some of the symptoms and the abnormal pulse curve appeared to me to indicate the existence of dilatation.

Fig. 7 illustrates the pulse curve of a woman, 57 years of age, who had suffered since her early youth from various nervous disorders, which frequently recurred without any known cause. The symptoms chiefly consisted of violent palpitation, constrictive pain of the chest, hurried breathing and nausea. During the last few years the dyspnœa took on the character of orthopnœa. The patient is of short stature and corpulent; she had never suffered from any serious disease; the lungs are sound; there is some enlargement of the area of cardiac dullness; there are no murmurs, and the sounds of the heart are of an unusually high pitch. The attacks of orthopnœa which I witnessed were alarming. The pulse became irregular and the thoracic pain had the anginose character. Near the close of one of these attacks I took the above tracing. All the elements of the normal curve appear to be wanting; many of the curves are

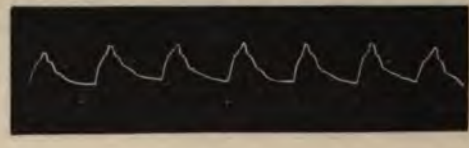
Fig. 7.



exceedingly small. In some of the larger curves the elevations are either absent or indistinct. The descending line forms an acute angle with the *upstroke*, resembling in this respect the steep curve of aortic regurgitation.

Fig. 8 represents the pulse tracing of the same patient during the interval of apparent health. It will be noticed that the deviations from the character of the normal pulse curve merely consist of an arched *upstroke* and plateau summit, peculiar to the senile curve.

Fig. 8.



A heart which is so readily thrown off its balance, as in this case, cannot be a sound one. On the supposition that this organ was originally of weak texture, and disposed to become attenuated and dilated, which the attendant hypertrophy somewhat neutralized, and that some irritation casually evoked syncopic attacks, I concluded to prescribe strychnia, with the intention of improving the muscular contractility of the heart. I am satisfied that this remedy had the effect of prolonging the intervals between the attacks of cardiac asthma and lessened their severity. This improvement could, however, not be sustained, as there is a limit to the safe administration of the alkaloid. The patient, who was under my observation for many years, eventually succumbed to an intercurrent pneumonia, after a three days' sickness. There is reason to believe that death was hastened by the early failure of the heart's action.

Fig. 9 is the graphic of a pulse curve in a case of obstructive disease of the respiratory organs. The patient was an elderly man who had been affected for many years with chronic bronchial catarrh. The symptoms attributed to the existence of heart trouble consisted of lividity and puffiness of the face; a sighing respiration; permanent dyspnœa, irregularity and feebleness of the pulse, inability to sleep in the recumbent position and anasarca of the lower extremities.

The dicrotic wave in this tracing occupies an abnormal position, being too near the base line. This peculiarity indicates diminution of arterial tension. The general venous congestion which attends obstructive diseases of the lungs is not indicated by any other modification of the pulse tracing, unless it be the arhythmical succession and irregularity of the curves, which probably point to an over-distended and dilated right ventricle.

FIG. 9.

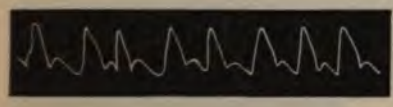
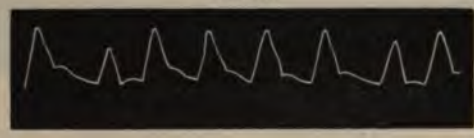


FIG. 10.

Fig. 10 reproduces the familiar pulse curve of aortic regurgitation. The patient was a young man, who experienced no other inconvenience from the valvular disease than occasionally an excited heart, due to hypertrophy of the left ventricle, which sometimes results from aortic insufficiency. A faint murmur at the left apex of the heart indicated the coexistence of slight mitral reflux, which probably mitigated the effects of the aortic trouble.

Although Corrigan and Walshe deprecate the use of digitalis in aortic regurgitation, I was induced to administer this remedy, with the intention of allaying the tumultuous action of the heart, upon an occasion when the patient had imprudently indulged in very active exercise. Fig. 11 shows the effect of the remedy in altering the character of the pulse tracing.

FIG. 11.



This graphic demonstrates the injurious influence of digitalis in cases of this kind. The curves are irregular, and show intermittent heart beats. Experience teaches that sudden death is not uncommon in aortic regurgitation. It is easy to understand why a prolonged distention of the ventricle may bring on a stoppage of the heart's action. Digitalis, which retards the contractions, may thus become dangerous.

It is in mitral insufficiency, the most frequent of all forms of valvular disease, that digitalis proves an invaluable remedy. The more the dilatation is in excess of the hypertrophy, the more decided will be the tonic influence of digitalis.

Fig. 12 represents the pulse curve of a young woman who had two attacks of

FIG. 12.

rheumatic arthritis. Both sounds at the left and right apex are completely replaced by murmurs. A fairly accentuated sound is audible over the right second cartilage.

The impulse, which is diffusive, is felt higher up than normal, and with greater distinctness toward the sternum. There exists, undoubtedly, considerable hypertrophy. The pulse is quick and irregular. The tracing speaks for itself.

Fig. 13 represents the pulse curve of the same patient during a profuse menorrhagia. All the cardiac symptoms were aggravated, and for the first time anasarca appeared in the lower extremities, which rapidly became general.

Fig. 13.

Fig. 14.

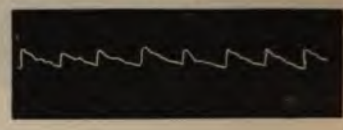
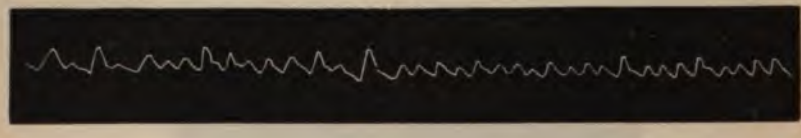


Fig. 14 is a specimen of the tracings, which were nearly of a uniform character when the graphics were taken, under the influence of digitalis, which the patient took in large doses. This remedy continued for a considerable period to perform its full duty, which is graphically indicated by the tracing of Fig. 14.

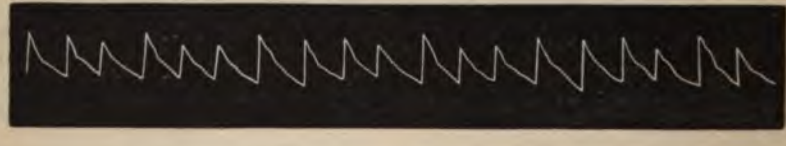
Fig. 15 is an instructive sample of pulse curve, showing the effects of atrophic change of substance of the heart, which had destroyed the compensating hypertrophy. The patient suffers now from all the consequences of such a pathological condition,

Fig. 15.



consisting of dyspnœa, attacks of syncope, irritating cough, profuse expectoration, cyanosis, anasarca and albuminuria. The great relief which the patient derived from the use of digitalis, is depicted in the following tracing (Fig. 16):—

Fig. 16.



The injudicious use of digitalis in a case of valvular disease, which was unattended by any troublesome symptom, but marked by very pronounced murmurs, is exemplified in Fig. 17. It was a case of aortic stenosis. The systolic basic murmur and a murmur synchronous with the contraction most audible at the left apex were both exceedingly loud. The patient, a man past the middle period of life, appeared to be little affected in his general health. His application for a life insurance having been rejected by the medical examiner, who apprised him of the condition of his heart, he was advised by an officious friend to take digitalis. It could not be ascertained how

much of the drug he took, but when I was sent for I found him just recovering from a severe fainting fit. The pulse was down to 52. The tracing, which I took on the same day, shows in a plain manner the retarding influence of digitalis on the action of the heart. It is obvious that the diminished contractions thus artificially pro-

FIG. 17.

FIG. 18.

duced aggravated the difficulty, in the ventricle, of sending a sufficient amount of blood through the constricted aortic orifice.

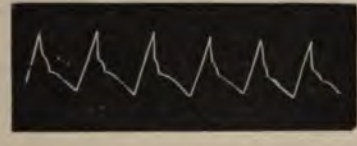
Fig. 18 illustrates the pulse curve of the same patient in his usual state of health.

FIG. 19.

In comparing Fig. 19, which shows the pulse curve of a young negro woman under the full anæsthetic influence of chloroform, with Fig. 17, a striking resemblance will be observed. Both curves indicate a near approach of paralysis of the heart.

Fig. 20 reproduces the graphic of the woman before the administration of the anæsthetic.

FIG. 20.



GEORGE E. FELL, of Buffalo, N.Y., read a report of a case of opium poisoning.

Dr. BRAINARD, of Los Angeles, California, objected to Dr. Fell's method, owing to the danger of the operation. He thought Sylvester's method sufficient, if prolonged and using the precaution to pull forward the tongue. The artificial respiration should be faithfully continued for six or nine hours.

Dr. FELL replied, that in his case artificial respiration was kept up until the patient seemed to be getting worse all the while.

Dr. ENTREKIN, of Ohio, had tried successfully forced respiration in opium poisoning, passing an ordinary piece of tubing through the larynx, and attaching to the external opening a pair of bellows. The connection had, of course, to be broken with each expiration.

The Section then adjourned *sine die.*

The following is the paper in full of Mr. JOSEPH KÖROSI :—

KRITIK DER VACCINATIONS-STATISTIK UND NEUE BEITRÄGE ZUR FRAGE DES IMPFSCHUTZES.

CRITICAL REVIEW OF VACCINATIONAL STATISTICS, WITH NEW CONTRIBUTIONS RELATING TO ITS PROTECTIVE POWER.

CRITIQUE DE LA STATISTIQUE VACCINATOIRE AVEC NOUVELLES CONTRIBUTIONS SUR SON POUVOIR PROTECTIF.

VON JOSEPH KÖRÖSI,
Direktor des communal-statistischen Bureaus in Budapest.

ERSTES KAPITEL.

EINLEITUNG.

ENTWICKLUNGSGANG DES IMPFSTREITES UND ZWECK DER GEGENWÄRTIGEN ARBEIT.

Rasche Verbreitung der Vaccination.

Ursprüngliche Ansicht von der absoluten und lebenslänglichen Schutzkraft, sowie von der Ungefährlichkeit der Vaccination: Jenner (1801), Sacco (1809), Hufeland (1821), Bateman, Sedillot (1840), "Englische Enquête" (1857). Auslese aus den auf die Fragen der englischen Regierung eingegangenen Antworten. Abwehr der unrichtigen Citirungen seitens Prof. Germanus.

Aufsteigende Zweifel. Fälle von Variola nach Blattern und nach Vaccination. Erklärungs- und Vertuschungsversuche. "Falsche" Blattern.

Die Revaccination als Deutero-, später als Polyvaccination. Annahme einer stets kürzeren Schutzfrist. Umsichgreifen der Skepsis. Gregory's Abfall.

Die Theorie des absoluten und des lebenslänglichen Schutzes und der absoluten Ungefährlichkeit des Impfaktes seit ungefähr fünfzig Jahren aufgegeben. Anerkennung der Möglichkeit, dass Krankheiten durch Impfung übertragen werden.

Umsichgreifen der Impfskepsis in den letzten fünfzig Jahren. Hervorragende Namen unter den Impfgegnern. Aussprüche von Impffreunden: Heim, Eitner, Steiner, Auspitz, Fleischmann, Bohn u. A.

Nothwendigkeit einer Revision des Impfstreites.

Unzulänglichkeit des Experimentes und hierauf gegründeter Hypothesen. Die Schutzkraft der Vaccine kann blos durch Erfahrung erhärtet werden. Vorher zumeist Einzelerfahrungen; gegenwärtig statistische Massenerfahrungen. Die Entscheidung also in Händen der Statistik.

Hieraus folgende Nothwendigkeit der Revision der Impf-Statistik, und zwar sowohl nach materieller Richtigkeit der Daten als nach logischer Richtigkeit der Schlüsse. Unzulänglichkeit der bisherigen Schlussweisen. Eintheilung dieser Arbeit. 1) Systematische Darstellung des gegenwärtigen Standes des Impfstreites und Kritik der statistischen Schlüsse. 2) Anwendung einer neuen Methode und Darlegung der Resultate. 3) Anhang: Analyse einiger impfgegnerischer Schriften und des statistischen Materials derselben.

Es ist nicht zu verwundern, wenn eine Entdeckung, welche die Menschheit von der verheerendsten aller Seuchen zu befreien unternahm, sich so rasch über die ganze Welt verbreitete. Am 14. Mai 1796 erfolgte durch Jenner die erste Impfung mit Kuhpocke und schon wenige Jahre nachher war die Impffrage bereits Das, was sie heute noch ist, nämlich die populärste Frage der Hygiene. Im Laufe eines kurzen Jahrzehntes hatte sich die neue Theorie nicht nur in England eingebürgert, sondern wurde auch in Frankreich, in Italien, in Oesterreich (Böhmen 1801), in Ungarn,[1] in

[1] Prof. Bene veröffentlichte im Jahre 1802 eine Unterweisung in der Vaccination ("A mentö himlö eredetéröl, természetéröl és beoltásáról." Pest, 1802), in welcher er bereits 26 Provinzärzte, überdies 17 Aerzte in Pest-Ofen namentlich anzuführen weiss, die sich um die Verbreitung der Vaccination Verdienste erworben.

einigen deutschen Staaten (zuerst in Baden 1803, dann in Bayern 1807, u. z. sogleich zwangsweise), in Schweden (1802 bez. 1816), bald darauf in Württemberg (1818 als Impfzwang) eingeführt. Schon im Jahre 1804 hatte sie Asien auf dem Landwege bis nach Ostindien durchwandert, und gar bald erstreckte sie sich über alle civilisirten Staaten des Erdballes. Geschwinder als jemals eine Epidemie, sagt Sanders, verbreitete sich über die ganze civilisirte Welt die Ausnutzung der Entdeckung Jenner's.

Die rasche Verbreitung der Kuhpocken-Impfung ist begreiflich, wenn man bedenkt, welch' immensen Nutzen dieselbe der Menschheit ohne die geringste Schädigung versprach. Durch die Impfung wurde man, nach der allgemein herrschenden Ansicht, für das ganze Leben gegen die Gefahr der Pockenerkrankung geschützt. Dieser Schutz wurde als ein absoluter betrachtet. Das grosse Heilmittel selbst aber bot weder im Impfakte selbst, noch durch die Folgen desselben die geringste Gefährdung.

Im Laufe ihrer neunzigjährigen Entwickelung hat aber die Theorie der Pockenimpfung, d. h. von der Schutzkraft der Kuhpocke gegen die Gefahr, an Menschenblattern zu erkranken oder zu sterben, sehr namhafte Wandlungen durchgemacht, und es lässt sich nicht läugnen, dass alle diese Wandlungen die Tendenz beweisen, die ursprüngliche Annahme einer absoluten und auf das ganze Leben sich erstreckenden Schutzkraft, sowie der absoluten Ungefährlichkeit des Impfaktes einzuschränken und abzuschwächen.

Wie fest diese ursprüngliche Ueberzeugung in dem wissenschaftlichen Bewusstsein der Zeitgenossen Jenner's gewurzelt, ist aus allen diesbezüglichen, dem Beginne des Jahrhunderts angehörenden Schriften in augenfälligster Weise zu erkennen. Die Fortdauer dieser Ansichten aber lässt sich sogar bis in das sechste Decenium dieses Jahrhunderts verfolgen.

Jenner selbst schrieb im Mai 1801: "Es ist jetzt zu klar, um noch Streit zu erregen, dass die Vernichtung der Menschenpocken, der fürchterlichsten Geissel des Menschengeschlechtes, das Endresultat dieser Entdeckung sein muss." Derselben Ansicht waren jene ersten Impffreunde, die sich gleich Aposteln um die Gestalt Jenner's schaarten. Man lese z. B., wie Sacco über diesen Punkt gedacht. Luigi Sacco war zur Zeit des napoleonischen Königreichs Italien, Direktor der Impf-Anstalt zu Mailand und hat unstreitig die grössten Verdienste um die Einführung der Kuhpocken-Impfung in Italien. Er selbst hat im Verlaufe von nicht ganz einem Decenium mehr als eine halbe Million Menschen geimpft.[1] Sacco erzählt, dass er an vielen Orten die ausgebrochene Blatternepidemie durch energische Impfung zu sofortigem Stocken gebracht, und Thatsache ist, dass zur Erinnerung an sein segensreiches Wirken und in Anerkennung seiner erfolgreichen Bemühungen, sowohl in Bologna, als in Brescia, goldene Denkmünzen auf ihn geschlagen wurden. Sacco liess im Jahre 1809 durch die Regierung ein Rundschreiben an alle Aerzte des Landes ergehen, worin dieselben aufgefordert wurden, anzugeben, ob ihnen im Laufe ihrer Praxis ein einziger Fall vorgekommen wäre, wo geblatterte oder vaccinirte Personen ein zweites Mal von

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[1] Sacco. "Trattato di vaccinatione." Milano, 1809. Kolb ("Zur Impffrage." Leipzig. 1877. S. 72) bestreitet die physische Möglichkeit einer solchen Leistung, und in Folge dessen die Richtigkeit der Angaben Sacco's, und macht Kussmaul, der diese Angaben reproducirt, den Vorwurf, in statistischen Fragen nicht die gleiche Geschicklichkeit wie in medicinischen zu besitzen. Soweit mir bekannt, ist es durchaus nichts Ausserordentliches, bei Massenvorführung von Impflingen im Laufe einer Stunde 50 Impfungen vorzunehmen, was im Tage 300, im Monate 9000 und im Verlaufe eines Jahres mehr als 100,000 durchgeführte Impfungen ergeben würde. Pfeiffer (in Gerhardt's "Kinderkrankheiten") erwähnt, dass Dr. Kranz in Ingolstadt am 10. Januar 1871 an einem Tage 1371 französische Kriegsgefangene impfte. Auch ich kann erwähnen, dass im Jahre 1886 in der Budapester Irrenanstalt 900 Kranke und das gesammte Personal durch Physikus Kresz im Verlaufe einiger Stunden geimpft wurden.

den Blattern befallen wurden. Die hierauf eingelangten zahlreichen Berichte wurden von Sacco nicht veröffentlicht, und begnügt sich derselbe, eine einzige, freilich sehr massgebende Antwort, nämlich die des Ministeriums des Innern, zu reproduciren, in welcher erklärt wird, dass nach Durchsicht sämmtlicher Akten kein einziger ähnlicher Fall zu konstatiren gewesen sei.[1] Sacco selbst aber ist der Ansicht, dass es ein durch die Erfahrung bestätigtes *Gesetz* sei, dass solche Personen für die Dauer ihres ganzen Lebens vor der Gefahr der Blattern-Erkrankung geschützt bleiben.[2] Zwei Decennien später hat kein Geringerer als Hufeland[3] seine Erfahrungen dahin zusammengefasst, dass man gewiss sein könne, dass im Falle der allgemeinen Einführung der Vaccination "gar keine Pocken mehr auf der Erde existiren würden", ferner, "dass die Zeit die schützende Kraft der Vaccinen nicht zu schwächen scheine." Und noch im Jahre 1840 erklärte Sedillot, Präsident der französischen Impf-Commission, dass "die Kuhpocken-Impfung den Menschen für immer vor der Variola schütze, und dass diese Schutzkraft ungeschwächt und absolut wie die der Variola selbst sei"; die französische medicinische Akademie aber sanktionirt diese Thesen, die an Entschiedenheit Nichts zu wünschen übrig lassen, durch ihre Zustimmung.[4] Um dieselbe Zeit erklärte Bateman (in seinem Handbuche der Hautkrankheiten[5]), die Impfung werde die Variola bald ausrotten, und so werde diese kein Gegenstand des Unterrichtes mehr sein können. Ja, selbst im Jahre 1857, als die englische Regierung jene grosse Enquête veranstaltete, deren Resultate die Fachwelt als Pocken-Blaubuch[6] so hoch schätzt, hat eine grosse Anzahl der diesbezüglich befragten 542 Aerzte des In- und Auslandes sich, und zwar mit überraschender Entschiedenheit, dahin erklärt, dass die Möglichkeit einer Erkrankung bei vaccinirten oder geblatterten Personen *absolut* ausgeschlossen sei, und dass sich eine Schwächung der Schutzkraft im Laufe der Zeit nicht behaupten lasse. Aerzte, die auf eine Praxis von dreissig bis vierzig Jahren zurückblicken konnten, in der harten Spital-Praxis ergraute Veteranen der Heilkunst, Medicinalbeamte, die die längste Zeit in Asien, Australien und Amerika fungirt hatten, erklären mit einer imponirenden Sicherheit, dass ihnen solche Fälle *nie* vorgekommen seien.[7]

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[1] "Trattato." Seite 109.

[2] "Trattato", Seite 64, sagt er Folgendes: "Si dovra per necessaria consequenza stabilire comme canone confermato, che quello in cui il vero vaccino siasi compiutamente sviluppato, è garantito dal vajuolo nel decorso di tutta la sua vita."

[3] Hufeland, Novemberheft seines Journals, 1826, Nos. 2 und 6 der daselbst aufgestellten sechs Thesen. (Siehe Stricker, "Studien über Menschenblattern." Frankfurt a. M., 1861. Seite 38.)

[4] "Mémoires de l'académie royale de médecine." VIII, Seite 568. (Siehe Reitz, Seite 9.)

[5] Bateman. "Cutaneous Diseases." Seite 91.

[6] General Board of Health, "Papers relating to the history and practice of vaccination, presented to both Houses of Parliament by command of Her Majesty." London, 1857. Der Bericht ist von John Simon, Officer of the Board, redigirt.

[7] Es dürfte nicht ohne Interesse sein, einige dieser Zeugen selbst sprechen zu hören. Wir lassen nachstehend probeweise die markantesten Aussprüche des ersten Hunderts der Antworten folgen.

Die Frage lautete: Hegen Sie Zweifel darüber, dass erfolgreiche Impfung den Betreffenden einen grossen Schutz vor Blatternerkrankung, und einen fast absoluten vor dem Blatterntode verleiht?

Eine grosse Menge der Antworten beschränkt sich auf ein laconisches: "Nein", "absolut nicht", etc. Es fehlt aber nicht an noch eindringlicheren Bemerkungen. So sagt Prof. Allen, York (No. 2): Auf einem weiten Beobachtungsgebiete ist mir im Laufe von über 30 Jahren nur ein einziger tödtlich verlaufender Fall bei Geimpften aufgestossen. Amphlett vom allge-

In dem Triumphzuge, welchen die Impftheorie durch die ganze Welt feierte, fehlten aber, wie in jenem der römischen Imperatoren, auch die Spötter und die Zweifler nicht. "Gleich dem rothen Faden an dem Tauwerke der englischen Marine, begleitet der Streit die Jenner'sche Entdeckung von ihrer Entstehung an", sagt Flinzer. Bei der Räthselhaftigkeit, mit welcher die Vaccination wirkt, ist dies begreiflich. Die Zweifler

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meinen Hospital in Birmingham (No. 13) ist der Ansicht, dass nach der Impfung Todesfälle an Blattern nicht vorkommen können. Anderson (No. 15) hat gar keine Zweifel; eine elfjährige Praxis in China, wo die Blattern alljährlich epidemisch auftreten, und eine vierzehnjährige Praxis in England haben seine festeste Ueberzeugung gegründet. Bacot, London (No. 25), hat bei Vaccinirten noch nie einen Todesfall beobachtet. Prof. Bamberger, Würzburg, gegenwärtig in Wien (No. 29), kann der Vaccination keinen absoluten oder lebenslänglichen Schutz zuschreiben, hat aber in seinem Spital (Krankenaufnahme jährlich 4000) im Laufe von 2½ Jahren keinen tödtlichen Ausgang bei Vaccinirten zu beobachten gehabt. Barber, Stamford (No 32): "Wenn Impfung schon nicht vor Erkrankung zu schützen vermochte, so mildert sie doch den Verlauf"; hat nur einen einzigen Todesfall bei Geimpften beobachtet. Barnes, Carlisle, Oberarzt der Cumberland Infirmary (No. 37), kann sich nicht erinnern, nach Blattern oder Impfung je einen Todesfall gesehen zu haben. Bateson, Lancaster (No. 41), hat im Laufe einer dreissigjährigen Praxis noch nie einen mit Erfolg Geimpften an Blattern verloren. Bayly, Yarmouth (No. 43): "Ich practicire seit 26 Jahren. Ich war 14 Jahre lang Medicinalbeamter und gehörte 10 Jahre lang dem ärztlichen Personal des Royal Hospitals an; ich war ferner seit Einführung des Zivilstandgesetzes Registrator für Geburten und Todesfälle. Ich habe *nach erfolgreicher* Impfung nie einen Fall von wirklichen Blattern beobachtet und nie einen Todesfall bei Geimpften registrirt." Bealer, Angleton, Impfarzt (No. 44), hat noch nie Blatterntodte bei Vaccinirten gehabt. Bird, Swansea, Hospitalarzt: "Ich habe hierüber nicht den mindesten Zweifel. Es gibt keinen Gegenstand, über welchen ich einen stärkeren Grad von Ueberzeugung besitze. Ich habe in meiner Praxis nie Blatterntod bei Vaccinirten beobachtet." Blacklock, Dumfries, von der Marine (No. 60), kann nach mehr als vierzigjähriger Erfahrung beruhigt versichern, dass er nicht den geringsten Zweifel hierüber hege. Blythman, Swinton (No. 62), hat seit 15 Jahren nicht einen einzigen Fall von Blatternerkrankung nach Impfung gesehen. Brett, Boston (No. 70), hält die Impfung für eine Panacee; "nach erfolgreicher Impfung wird man vergebens nach Blatterntodesfällen fragen." Brown, Preston (No. 77): "Die Impfung bietet ein beinahe vollkommenes Präservativ." Bullar, Southampton (No. 84), hat keinerlei Zweifel; "diese beiden Punkte scheinen mir durch so zahlreiche Thatsachen so vollkommen erwiesen als nur je eine These in der Geschichte der Krankheiten." Burrows, London, Mitglied des Royal Society, Arzt am Bartholomäus-Hospital (No. 90), hat allen Grund zu glauben, dass diese Frage ohne alle Reserve zu bejahen ist. Cammack, Spalding (No. 94), basirt seine Ueberzeugung auf eine Praxis von 50 Jahren, welche nicht den Schatten eines Zweifels aufkommen liess. Cary, Wardford (No. 100): "Die Impfung bietet nahezu absolute Sicherheit"; kann in seiner Praxis von 31 Jahren sich nur auf zwei Fälle (soll wohl heissen, wo der Schutz versagte) erinnern, u. s. w.

Ich kann es nicht unterlassen bei diesem Anlasse meinem Erstaunen darüber Ausdruck zu geben, in welcher unverantwortlichen Weise Prof. Germann in Leipzig ("Historisch-kritische Studien über den jetzigen Stand der Impffrage." Leipzig, 1878. 2. Band, S. 100 ff.) mit den Aussagen des englischen Blaubuches umgeht. Er resumirt die Antworten, welche auf die erste Frage einliefen, folgendermassen:—

"Die Vaccination hat nie von den Anfällen der Blattern befreit und wird nie von denselben befreien, einzig deshalb, weil sie dies nicht kann," und führt als Zeugen hiefür die Antworten Nummer 179, 219, (Hamernick) 478, 508 (Welch), 546, 435, 79, 121 und 192 an. Auch Kolb ("Impffrage", Seite 15) schreibt, in gutem Vertrauen auf Germann und unter Citirung derselben Nummern, eine grössere Anzahl der Aerzte habe sich missgünstig über die Impfung ausgesprochen. Demgegenüber wird es nicht überflüssig sein, Folgendes zu erwähnen:—

Unter *sämmtlichen* befragten Aerzten haben sich nur *zwei* gegen die Impfung ausgesprochen, nämlich Hamernik und Welch; es ist also richtig und auch begreiflich, dass diese zwei Aussagen auch auf diese Frage verneinend lauten. Es ist aber ganz unverzeihlich, beim Leser den

wurden aber durch die übereinstimmenden Berichte über die glänzenden Ergebnisse der Kuhpockenimpfung, durch das Gewicht der grossen Autoritäten, die sich für dieselbe, u. z. wie wir sahen, in so entschiedener Weise einsetzten, übertäubt und unterdrückt. Es war, sagt Bousquet, einfach Blasphemie, Zweifel in die absolute Schutzkraft der Impfung zu setzen.

Zur Erklärung solcher Fälle, wo Geimpfte blatternkrank wurden, liebte man es (und liebt es noch heute), zu der Annahme schlecht durchgeführter Impfung seine Zuflucht zu nehmen. Wo dies nicht half, wo die Wirksamkeit der Impfung ohne Zweifel war, mussten die Blattern es sich gefallen lassen, als "falsche" hingestellt zu werden. Dem absprechenden Urtheile Sacco's, von der Unmöglichkeit einer Blatterung Geimpfter, folgten im Jahre 1823 und 1825 zwei heftige Blatternepidemieen, die in Oberitalien zahlreiche Geimpfte ergriffen. Statt aber das Falsche seiner Behauptungen einzusehen, zog er es vor, die Blattern für falsche, bez. für eine ganz andere Krankheit

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Glauben erwecken zu wollen, als ob auch die übrigen Aussagen ungünstig lauteten. Man vergleiche, wie die von Germann citirten Aussagen — nebenbei bemerkt, nur *neun* unter 542! — dem Wortlaute nach beschaffen sind:—

No. 179: "Ich habe keinen Zweifel, dass die Impfung einen sehr grossen Schutz gegen die Anfälle von Blattern bietet, und ich glaube, dass nach der Impfung vorkommende Blatternerkrankungen im Allgemeinen einen milden Verlauf nehmen."

No. 478: "Ich habe keinen Zweifel, dass erfolgreiche Vaccination in *sehr grossem* Maasse vor den Angriffen der Blattern und vor Tod an denselben beinahe vollkommen schützt. Es ist mir nicht bekannt, von einem tödtlichen Ausgange der Blattern gehört zu haben, wenn über die vorhergegangene erfolgreiche Vaccination kein Zweifel bestand. Da ich seit 30 Jahren eine bedeutende Praxis in einem ausgedehnten Bezirke ausübe, kann ich über den wohlthätigen Einfluss der Impfung nicht den geringsten Zweifel hegen. In allen Fällen, wo ich bei Erwachsenen Blattern nach Vaccination sah, waren diese ungemein mild und haben nachher auch nie eine Entstellung hervorgerufen. Ich habe einige schwere Fälle gesehen, welche zum Glauben führen könnten, dass sich die Schutzkraft der Vaccination nicht bestätigt habe. Aber nach natürlichen Menschenblattern kann dasselbe erfolgen, denn mir selbst kamen einige sehr markante Fälle nach natürlichen Blattern vor."

No. 546: Ist im Register nicht zu finden. Die letzte Aussage trägt die Nummer 532.

No. 435: "Ich glaube, dieselbe (d. i. die Impfung) müsse diese Wirkung haben. Da wir aber dies blos aus der gegenwärtigen geringeren Mortalität folgern können, wie lässt sich die Wirkung des Virus von jener der verbesserten Behandlung solcher Krankheiten, von der geschwächten Kraft des Blatterncontagiums, von dem Einflusse der Zeit und anderer Ursachen absondern? Es ist dies ein Einfluss, der per analogiam zu erklären sein könnte."

No. 79: "Die Vaccination verhindert Blattern nicht, aber bewirkt, dass sie leichter behandelt werden können, und verhindert deren gefährliche Tendenz."

No. 121: "In der *sehr grossen* Mehrzahl der Fälle schützt Vaccination vor der Blattern-Erkrankung; aber es gibt Menschen und selbst Familien, welche trotzdem für die Blattern empfänglich bleiben, obzwar beinahe ausnahmslos in einer sehr milden Form. Ich habe manche solcher Fälle gesehen, jedoch *keinen* mit lethalem Ausgange, und nur zwei bis drei schwere. — Ich habe oft Kinder geimpft, wo, da die Eltern gegen die Impfung eingenommen, einige Familienmitglieder an Blattern gestorben waren. Die Impfung hat die Blattern stets verhindert, mit Ausnahme eines Falles, wo der Vater an confluirenden Blattern gestorben war. Ich impfte sein Kind, und Variola sowie Vacinepusteln entwickelten sich zu gleicher Zeit; erstere in sehr milder Form."

No. 192: "Gewissenhaft und gehörig durchgeführte Vaccination hat der Menschheit den gehofften und absoluten Schutz gegen die Blattern geboten, und zwar für die ganze Lebensdauer der betreffenden, der Operation unterworfenen Individuen."

Die Darstellung Dr. Germann's involvirt also eine gröbliche Entstellung des wirklichen Sachverhaltes. Unter so bewandten Umständen wird man es begreiflich finden, wenn wir uns der Mühe enthoben glauben, sämmtlichen in den drei voluminösen Bänden Prof. Germann's enthaltenen Angaben auf ihre Quelle nachzugehen.

zu erklären.[1] Dieser Begriff der "falschen Blattern" hat den Medicinern viel Kopfzerbrechen verursacht, bis derselbe aus der Pathologie ganz verschwand. Mit berechtigtem Sarcasmus bemerkt Bousquet in seinem preisgekrönten Werke,[2] dass, um die Ehre der Impftheorie zu retten, entweder die Impfung eine falsche sein müsste, oder die Blattern. Ja, man liess sich im Uebereifer für eine für gut gehaltene Sache sogar zu Verheimlichungen und Fälschungen der Sachlage verleiten. So erzählt Eimer,[3] dass man im deutschen Reichsanzeiger schon im Jahre 1805 Fälle meldete, wo bei Vaccinirten Blattern ausbrachen, dass aber diese unliebsamen Erscheinungen, um die Ehre der Vaccination zu retten, für Varicella erklärt wurden.

Der impfeifrige Bousquet selbst erklärt (S. 330), es habe keine Subtilität gegeben, welche die Impfer nicht versucht hätten, um Das, was sie erfuhren, mit Dem in Einklang zu bringen, was sie gelernt hatten. Als sich aber im Laufe der Jahre die Fälle stets mehrten, wo geblatterte oder vaccinirte Personen von den Blattern ergriffen wurden, und es nicht gut anging, diese Blattern zu läugnen oder deren Auftreten auf eine schlecht durchgeführte Vaccination zurückzuführen, mussten die Angriffe der Impfgegner an Gewicht gewinnen.

Die Impftheorie erlitt ferner einen schweren Schlag, als durch Einführung der Revaccination (zum ersten Male in der preussischen Armee, 1831, kurz darauf, 1833, in der württembergischen, um gegen die aus Italien drohende Blatternepidemie zu schützen) es offen eingestanden war, dass die Vaccination nicht für das ganze Leben, sondern nur für mehrere Jahre zu schützen im Stande sei, dass man sich also, um geschützt zu sein, nach längerer Zeit zum zweiten Male vacciniren lassen müsse. Die Impfvertheidiger fühlten mit dieser neuen Theorie den festen Boden unter ihren Füssen schwanken: was Wunder also, wenn man sich von mehreren Seiten gegen die Anerkennung der Revaccination sträubte. Die Pariser medicinische Akademie antwortete im Jahre 1838 auf die Anfrage des Ministers, ob die Schulkinder nicht revaccinirt werden sollten, mit einem unwirschen Nein, ohne sich aber zur geringsten Motivirung herbeizulassen. Gregory, gewiss eine Autorität ersten Ranges, meint,[4] "dass die Doctrin der Proto- und Deuterovaccination alsbald in jene einer Tritovaccination, schliesslich aber in jene Frage übergehen werde, ob Jemand geschützt sei, der sich alljährlich einer Impfung unterzogen?" Gregory hatte ganz richtig vorausgesehen, woher der Vaccinationslehre die grösste Gefahr drohe: die Schutzdauer der Impfung ist seit Einführung der Revaccination wirklich controvers geworden und aus der Deuterovaccination hat sich wirklich die Polyvaccination herausgebildet.

Bei Einführung der Revaccination glaubt man sich durch eine einmalige, etwa im Alter von 10—20 Jahren vorzunehmende Wiederholung der Impfung für zeitlebens geschützt. Heute sind wir schon an dem Punkte angelangt, wo auch eine zweimalige, ja selbst dreimalige Vaccination als nicht genügend betrachtet wird. So wurde z. B. in den Verhandlungen der württembergischen Kammer im Jahre 1858 die Schutzdauer der Vaccination mit 15 Jahren angegeben; die deutsche Impfcommission sprach sich im Jahre 1884 dahin aus, dass die Dauer des Schutzes 10 Jahre betrage;[5] zahlreiche

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[1] Sacco. "De vaccinationis necessitate." Milano, 1832.

[2] Bosquet. "Traité de la vaccine." Paris, 1848. Seite 331.

[3] Eimer. "Die Blatternkrankheit." Leipzig, 1853. Seite 100. Sachse, ein s. Z. renomirter Schriftsteller über die Vaccination, der die Möglichkeit der Pockenerkrankung Geimpfter lange auf das Entschiedenste läugnet hatte, legt in Hufeland's Journal 1833 das Geständniss ab, dass er selbst Fälle von Variola bei Vaccinirten verheimlicht habe.

[4] S. Gregory's Brief in Brown. "Investigations on the present unsatisfactory and defective state of vaccination." Edinburgh, 1842. Seite 105.

[5] S. Schmidt's Jahrbücher. Band 206, S. 193 ff.

Aerzte sind der Ansicht, dass die Impfung nicht länger als für 7 Jahre schütze; es gibt aber Vertheidiger noch kürzerer Schutzfristen.[1]

Unter solchen Umständen ist es begreiflich, wenn die Skepsis stets mehr um sich greift, ja selbst einstige Pfleger der Impfung in das Lager der Impfgegner übertreten. Einen der dramatischesten Fälle bildet wohl die merkwürdige Thatsache, dass der grosse Kliniker Gregory, Director des Londoner Pockenspitals, sich von der durch ihn selbst Jahrzehnte lang gerühmten und gepflegten Impfpraxis abwendete und im Jahre 1852, am Abende seines thatenreichen Lebens, erklärte, "dass die Ausrottung dieser grausamen Krankheit noch in ebenso weiter Ferne steht wie damals, als sie so leichtsinnig und nach meinem bescheidenen Urtheile höchst vermessentlich von Jenner anticipirt wurde. Ich werde zu dem Schlusse getrieben, dass die Empfänglichkeit für das Blattern-Miasma bei Geimpften mit den Jahren wächst, während bei Ungeimpften das Gegentheil stattfindet."[2]

Die Theorie des absoluten, d. i. ausnahmslosen, Schutzes wurde zugleich mit der Einschränkung der lebenslänglichen Schutzfrist aufgegeben. Noch zur Zeit der erwähnten englischen Enquête (1857) hat zwar eine grosse Anzahl der befragten Aerzte sich für einen "fast absoluten" Schutz der Vaccination ausgesprochen. Man kann aber behaupten, dass die Lehre von der lebenslänglichen und absoluten Schutzkraft der Vaccine, das, wie Bousquet sagt, goldene Zeitalter der Vaccinationslehre, seit vier bis fünf Jahrzehnten verlassen ist. Wollte man heute, wo die Statistik uns so zahlreiche Beweise dafür geliefert, dass nicht nur vaccinirte und gepockte, sondern selbst revaccinirte Personen von den Blattern ergriffen wurden und denselben erlegen sind, eine ähnliche Umfrage wie im Jahre 1857 stellen, man würde ohne Zweifel viel reservirtere Antworten erhalten. Kein vernünftiger Arzt würde die Behauptungen Sacco's, Hufeland's oder Sedillot's unterschreiben; keiner würde mehr die Behauptung wagen, dass die Vaccination, oder selbst die Revaccination absolut (d. h. Jedermann unbedingt) und für das ganze Leben gegen die Blattern schütze. Nach den zahlreichen Erfahrungen, dass auch Vaccinirte an Blattern erkranken, ist man mit Recht darüber verwundert, wie Jenner und seine Zeitgenossen, kaum dass sie die Schutzkraft der Vaccine einige Jahre lang beobachtet hatten, bereits die These eines lebenslänglichen Schutzes einsprechen konnten. Erfährt man dann, dass diese Behauptung sich nur auf jene wenigen Erfahrungen stützte, wonach einige Personen, die vor Jahrzehnten sich beim Melken unabsichtlich Vaccine zugezogen, immun blieben, so kann man nicht umhin, diese Generalisation mit Gregory als eine übereilte zu betrachten und jenen Fanatismus, mit dem entgegenstehende Beobachtungen und Beobachter verfolgt wurden, zu bedauern.

Auch in der Streitfrage, ob die Impfung nicht etwa von Schaden sein könne, ist eine sehr bemerkenswerthe Verschiebung der Sachlage eingetreten. Es wird heute zugegeben, dass der Impfakt selbst schliesslich doch, wenn auch nur sehr ausnahmsweise, gefährlich werden könne.

Ernster aber ist die Furcht vor Uebertragung von Krankheiten zu nehmen. Diese wird nicht mehr als Ausgeburt des Wahnwitzes behandelt; die Existenz der durch die Impfung übertragenen Syphilis ist leider kein Ammenmärchen mehr, sondern eine wissenschaftlich verbürgte und anerkannte Thatsache.

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[1] Kolb ("Der heutige Stand der Impffrage." S. 18) erwähnt eines, mir nicht bekannten "Report of the City Hospital of New York", in welchem es heissen soll, dass die Kinderimpfung nutzlos sei, wenn sie nicht alle 3 Jahre wiederholt wird.

[2] Siehe *Medical Times* vom 26. Juli 1852. (Citirt nach Germann's "Historisch-kritische Studie über den jetzigen Stand der Impffrage". Leipzig, 1875. I. Seite 50.)

Die erwähnten Einschränkungen wurden der herrschenden Schule schrittweise und stets erst nach langem Kampfe abgerungen. Wie in allen anderen Dingen, so ereignet es sich aber auch in der Impffrage, dass man der historischen Entwicklung leicht vergisst, dass man den gegenwärtigen Zustand als den allein richtigen, selbstverständlichen betrachtet. Bei beabsichtigter Schädigung des gegenwärtigen Besitzstandes rafft man sich zwar zum Widerstande auf, aber für die Verluste der Vergangenheit hat man die Empfänglichkeit verloren. In der Impffrage tritt noch hinzu, dass man sich auf impffreundlicher Seite über die bisher abgerungenen Concessionen durch das Bewusstsein hinwegsetzt, dieselben seien doch mehr theoretischer Natur: praktisch genommen, repräsentirten alle Einwendungen nur so unbedeutende Einschränkungen, dass die Frage des Impfschutzes hierunter in ihrem Wesen eigentlich nicht gelitten hätte. Von diesem Standpunkte aus halten Viele die Akten der Impffrage für geschlossen und eine weitere Einlassung in den Impfstreit für eine müssige Sache. Man begegnet — freilich nicht bei Fachmännern — auch heute noch häufig genug Aeusserungen, als ob die Frage des Impfschutzes eine über allem Zweifel erhabene, die statistischen Grundlagen derselben die bestgegründeten seien; als ob ein Ausbruch der Blattern stets nur die Strafe für zu laue Behandlung des Impfgeschäftes sei. "Neunzig Jahre nach Jenner dürfte es in Europa keine Blatternepidemie mehr geben", sagte mir ein sonst sehr scharf denkender Medicinalstatistiker. Dabei liebt man es, das Heer der Impfgegner als unklare, unruhige Köpfe, als eine Truppe von Halbwissern hinzustellen, denen es wohl nicht an Ueberzeugung, wohl aber an wissenschaftlichem Ernst und Befähigung mangelt, hinzustellen. Wir wollen es hier dahingestellt sein lassen, ob ein Laie, dessen Kind durch Impfung syphilitisch wurde, nicht ein Recht habe, sein zweites Kind dieser Gefahr zu entziehen; nicht ein Recht habe, sich um Gleichgesinnte umzusehen und ähnliche Thatsachen zu sammeln, um hierdurch die Legislative zu informiren. Wir wollen hier nur einige Anhaltspunkte dafür bieten, dass die Akten der Impffrage durchaus noch nicht geschlossen, dass Untersuchungen über diese, jeden Menschen so nahe interessirende Frage, durchaus nicht überflüssig sind, und dass sowohl die allgemeine, wie die wissenschaftliche öffentliche Meinung noch immer von den quälendsten Zweifeln beherrscht wird.

Wir berufen uns diesbezüglich auf die Thatsache, dass die Partei der Impfgegner und deren Literatur, namentlich seit Einführung des Impfzwanges, in England und Deutschland in starker Zunahme begriffen ist; ferner auf die Thatsache, dass sich den Impfgegnern auch eine Reihe wissenschaftlicher Namen anschloss. So vor allen Gregory, dann Universitäts-Professor Bock in Leipzig, Universitäts-Professor Germann, ebendort, Prof. Hamernick in Prag, Krankenhaus-Direktor Lorinser in Wien, Primarius der Blatternabtheilung Hermann in Wien, Kinderspitals-Direktor Reitz in St. Petersburg, Universitäts-Professor Vogt in Zürich (der nach seinen fulminanten Angriffen gegen den Impfglauben zum Mindesten als Impfskeptiker zu betrachten ist). Auch bei den durch medicinische Hypothesen nicht voreingenommenen Statistikern finden sich Aussprüche, die zu Ungunsten der Impfung lauten, so bei Engel, Kolb, Freiherrn v. Fircks u. A. Es dürfte auch Vielen neu sein, zu erfahren, dass auch Kant ein Impfgegner war, da er die Einführung eines thierischen Saftes in den menschlichen Organismus perhorrescirte.

Für das Umsichgreifen der Impfskepsis spricht auch, und zwar in sehr entschiedener Weise, das Factum, dass in einzelnen Gebieten der bereits eingeführte Impfzwang wieder aufgehoben wurde (so in den Cantonen Zürich und Glarus); desgleichen, wenn hervorragende Fachmänner, die theils enrangirte Impffreunde, theils wenigstens keine Impfgegner sind, einerseits konstatiren, dass die Impfskepsis im Zunehmen begriffen sei, andererseits sogar ihre eigene Unsicherheit in dieser Frage offen eingestehen. Wir lassen hier einige Aeusserungen folgen, die, weil ausschliesslich von Vertheidigern der Impftheorie ausgehend, für die Beurtheilung des Ganges der

öffentlichen Meinung in den letzten 50 Jahren massgebend sind. Für die Mitte der dreissiger Jahre, z. B., findet sich in den Schmidt'schen Jahrbüchern wiederholt die Bemerkung, dass die Zweifel gegen die Impfung zu dieser Zeit allgemein wurden.[1] Im Jahre 1838 berichtete Heim, wie gross das Misstrauen der württembergischen Bevölkerung gegen die Revaccination sei. Es herrschte Abneigung gegen ein Schutzmittel, "von welchem das Volk, da es das erste Mal in seiner Erwartung getäuscht wurde, auch zum zweiten Male nichts Besseres gewärtiget."[2] Ungefähr anderthalb Decennien später markirt die Waffenstreckung Gregory's eine der grössten Niederlagen der Impfvertheidigung. Um dieselbe Zeit (1853) erklärte Eimer in seinem Ansehen geniessenden Werke[3]: "Man hat, gestehen wir es offen, allgemein die Bedeutung der Jenner'schen Erfindung überschätzt. Jetzt, da der Irrthum erkannt wird, *ist man geneigt, den Werth der Schutzpocken-Impfung zu unterschätzen.*" Zwanzig Jahre später (1872) sagt der anerkannte Kinderarzt Prof. Steiner (Prag), trotzdem er den Impfgegnern auf das Gröbste begegnet und denselben (S. 400) gradezu absichtliche Entstellung der Thatsachen vorwirft: "So sehr auch die Ziffern zu Gunsten der Impfung sprechen, so ist die Frage über den wirklichen oder blos scheinbaren Nutzen der Impfung *doch noch nicht unanfechtbar entschieden.* Zahlreiche Fälle, wo geimpfte Kinder an den heftigsten Formen der Variola vera erkrankten, während im Gegentheile nicht geimpfte Kinder blos von leichten Varioloiden befallen wurden, *lassen meiner Ueberzeugung nach manche Bedenken als gerechtfertigt erscheinen.*"[4] Prof. Auspitz hat schon im Jahre 1864[5] erklärt, "der Skepticismus habe die Aerzte erfasst";[6] im Jahre 1883 erklärt derselbe im Referate des n.-ö. Landes-Sanitätsrathes, welches zum Schlusse auf die Einführung des Impfzwanges hinausgeht, also durchaus keiner impffeindlichen Richtung beschuldigt werden kann: "Die Impfsachen stehen nicht mehr so wie vor einigen Decennien. Jedem erfahrenen Beobachter unserer sanitären Zustände gibt sich kund, dass seit längerer Zeit in die diesbezüglichen ärztlichen Anschauungen *einiges Schwanken gekommen ist, welches von Jahr zu Jahr deutlicher hervortritt* . . . Es bildet sich zu Zeiten der Epidemieen eine förmliche Desorganisation der gesammten öffentlichen Meinung in Betreff des früher für unantastbar gehaltenen Werthes der Schutzpocken-Impfung heraus." Aehnlich spricht sich um diese Zeit der langjährige Leiter eines Kinderspitals, Dr. Fleischmann, aus. In einer Arbeit über die zwanzigjährige Erfahrungen des St. Joseph Kinderspitals in Wien[7] erklärt sich derselbe zwar für eine über jeden Zweifel erhabene Schutzkraft der Vaccine, gibt aber dennoch zu, dass gegenwärtig (1870) eine "peinliche Unsicherheit in der Impffrage herrsche." Und noch in allerjüngster Zeit (1875), erklärt einer der anerkanntesten Vertreter der deutschen Wissenschaft, Bohn: "Kaum hat ein anderer

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[1] Vergleiche den ersten Supplementband: Ratter ("Ueber die Blattern im Wiener Krankenhause") spricht "von den ungerechtfertigten Zweifeln, welche man jetzt (1834) ziemlich allgemein über den Schutz der Impfung erhebt". Auch Physikus Ollenrod in Swineburg anerkennt (S. 181), dass sich neuerlich unter den Aerzten "die divergirendsten Stimmen über den Werth der Vaccine vernehmen lassen".

[2] Heim. "Pockenseuchen in Württemberg." Stuttgart, 1836. S. 591.

[3] Eimer. "Die Blatternkrankheit in pathologischer und sanitärer Beziehung." Leipzig, 1853.

[4] Dr. Johann Steiner. "Compendium der Kinderkrankheiten." Leipzig, 1872. S. 398

[5] Siehe Plenarsitzung des Wiener Doctoren-Collegiums in Wittelshöfer's mediz. Wochenschrift, S. 457. Ebendaselbst, S. 623, warnt ein bayerischer Arzt vor Einführung des Impfzwanges in Oesterreich, da dies nur dazu dienen würde, das ohnehin gesunkene Vertrauen in die Impfung ganz zu untergraben.

[6] Siehe Auspitz's Referat in *Wittelshöfer's Wiener mediz. Wochenschrift* v. J. 1873.

[7] Fleischmann, im III. Bande des *Jahrbuches für Kinderheilkunde.*

Gegenstand neben den politischen und religiösen Erscheinungen des XIX. Jahrhunderts alle Schichten der Gesellschaft so tief durchdrungen und die Gemüther immer und immer wieder erhitzt, als die Kuhpocken-Impfung, und der Kampf um dieselbe hat unter unseren Augen fast erbitterter getobt als je zu früherer Zeit."[1]

Die Impfgegner gehen natürlich noch weiter. Die von ihnen behauptete Nutzlosigkeit der Impfung fand in den letzten Jahren um so willigere Ohren, als wir eben in den letzten zwei Jahrzehnten Augenzeugen jener furchtbaren Blatternepidemieen waren, die den ganzen Erdball überzogen; die in ihren Verheerungen sich den ärgsten Seuchenzügen verflossener Jahrhunderte anreihten; die in ihrem Gange aber auch die bestimpfenden Staaten nicht verschonten. Aber nicht zufrieden damit, die Nutzlosigkeit der Impfung zu behaupten, predigen die Impfgegner die Lehre, dass die Impfung geradezu schädlich, dass also deren zwangsweise Durchführung ein frevelhaftes Attentat gegen die Gesundheit und Freiheit der Bürger sei. Da ferner, trotz der sich in der Literatur häufenden Angriffe, eine sachliche Widerlegung derselben noch nicht erfolgt sei,[2] fordern sie eine Revision der ganzen Impffrage, namentlich der Impfstatistik — hoffend, auf solche Weise, dem Impfzwange das Impfverbot folgen zu sehen.

Man kann also durchaus nicht behaupten, als ob es überflüssige Mühe wäre, sich mit der Frage über die Schutzkraft der Vaccination, sowie über deren eventuelle Gefährlichkeit zu beschäftigen, und als ob diese Schutzkraft eine derart anerkannte und über allen Zweifel bewiesene sei, dass eine Widerlegung derselben unmöglich, ein Beweis derselben überflüssig wäre. Diese Ansicht von der Nothwendigkeit einer Revision des Impfstreites gewinnt sogar ganz unabweisbare Kraft, wenn man den Boden und die Waffen untersucht, die zur Austragung dieses Kampfes gewählt sind.

Direkte Experimente über die Schutzkraft der Vaccine sind unmöglich. Man kann mit den schönsten Mikroskopen und mit den glänzendsten Hypothesen doch nie beweisen, dass die Einführung der Kuhpocke vor den Blattern schützen *müsse*. Wenn man auch beweisst, dass bei Vaccinirten die Einimpfung aus *Blatternpustela* keine Pocken mehr erzeugt, so ist hiermit doch nicht bewiesen, dass dies auch dem geheimnissvollen, der Blatternlymphe *vielleicht* ganz fremden Agens der *Epidemieen* unmöglich sein müsse. Die diesbezüglichen Argumentationen, wo es manchmal hiess, da der Erkrankte geimpft gewesen sei, könnten die Blattern unmöglich wirkliche Blattern gewesen sein, erinnern an die ontologischen Spekulationen der Metaphisik, und gleich diesen möchte man auch den Vaccinationsthesen zurufen, dass darüber, ob Etwas bestehe oder nicht, keine Spekulationen, sondern bloss die *Erfahrung* urtheilen könne. In diesem Fundamentalsatze liegt auch die Erklärung, weshalb die Entscheidung dieser Frage zuerst auf dem Gebiete der Einzelnbeobachtung, später auf jenem der Massenbeobachtung, also auf dem Wege der Statistik, erfolgen musste. Sind die physiologischen und chemischen Hypothesen über die Wirkung der Kuhpockenlymphe auf den menschlichen Organismus richtig, so werden dieselben auch durch die Massenbeobachtungen der Statistik bestätigt werden müssen; ergibt aber die Statistik ein conträres Resultat, so wird dasselbe alle noch so schön aufgebauten Hypothesen umstürzen.

Die Entscheidung liegt also in den Händen der Statistik. Dies wird auch theils offen anerkannt, theils durch die Thatsache, dass sowohl die Impfvertheidigung als der Impfangriff sich gleichmässig der Statistik als Waffe bedienen, stillschweigend zugegeben. Wenn vorher die Erfahrung *eines* Arztes gegen jene des anderen, also Mann

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[1] Bohn. Handbuch. Seite 284.

[2] Kolb. "Der heutige Stand der Impffrage." Leipzig, 1879. — Löhnert. "Impfzwang oder Impfverbot." — Seither ist (im Jahre 1881) eine energische Vertheidigungsschrift: "Pocken und Vaccination" von Lotz, Physicus in Basel, erschienen, auf welche sehr verdienstliche Arbeit wir wiederholt zurückzukommen haben werden.

gegen Mann, stand, so führt man jetzt Ziffermassen gegen Ziffermassen in's Feld. Bei dieser Entwickelung der Dinge ist es nöthig, die Waffen, mit denen man hüben und drüben kämpft, und die Stärke derselben zu prüfen, um zu wissen, wo im Gedränge der sich auf der Wahlstatt Tummelden Kernhiebe fallen, wo nur Luftstreiche.

Eine kritische Untersuchung des in dem Processe der Impfgegner gegen die Impffreunde vorgeführten Beweismaterials wird gar bald zu der Ueberzeugung führen, dass, wenn irgendwo, so in dieser Frage, eine Kritik des Beweissverfahrens nicht nur nicht überflüssig, sondern auf das Dringendste nöthig ist. Schon der Umstand, dass von beiden Seiten in leidenschaftlichstem Tone Klagen über tendenziöse Entstellung, ja selbst über Fälschung des statistischen Beweismaterials erhoben werden, lässt eine diesbezügliche Untersuchung als unabweisbar erscheinen. Diese Untersuchung hätte sich also in erster Reihe auf eine Prüfung der Verlässlichkeit, der Wahrheitstreue der angeführten Aussagen zu erstrecken. Noch mehr als der materielle Inhalt des ziffermässigen Beweismaterials fordert aber die formale Seite dieser Beweisführungen, die Methoden, nach welchen beobachtet und geschlossen wird, unsere Kritik heraus. Lassen Sie mich, geehrte Herren, an dieser Stelle gleich vorgreifend bemerken, dass bei Prüfung dieser logischen Unterlagen aller Urtheile über Werth oder Unwerth der Vaccination, wir die überraschendsten Resultate zu gewärtigen haben. Es wird sich herausstellen, dass bei dem gegenwärtigen Stande der Statistik, namentlich aber bei den bisher benützten Methoden, nicht nur jene sehr verwickelte Frage nicht zu beantworten ist, ob eine nach der Impfung auftretende Krankheit, z. B. Syphilis, als eine Folge der Impfung betrachtet werden dürfe, sondern dass nicht einmal auf jene elementarsten Fragen geantwortet werden kann, ob die Blatternepidemieen in Folge Einführung der Impfung abgenommen haben, oder ob die Impfung vor Blatternerkrankung schütze. Das erste Bedenken wird von den Impfgegnern aufgeworfen, die zwei letzten sind aus dem Arsenale der Impfvertheidigung geholt: allen diesbezüglichen Antworten gegenüber hat die Skepsis freies Spiel, denn keine derselben fällt positiv genug aus; ja man kann sagen, dass auf den bisherigen Wegen der Statistik es absolut unmöglich ist, auf die erste und dritte dieser Cardinalfragen überhaupt zu antworten.

Ich hoffe, durch Anwendung einer neuen Methode der statistischen Beobachtungen auf diese und einige andere controverse Fragen der Impfstatistik einiges Licht werfen zu können. Diese neuen Beobachtungen über die Schutzkraft der Vaccination bilden den positiven Theil meines Vortrages. Diesem sende ich einen kritischen voraus, in welchem ich den gegenwärtigen Stand des Impfstreites zu präcisiren, die pro und contra vorgebrachten Argumente in ein *System* zu bringen und so eine leichtere Uebersicht über die Phasen dieses verwickelten und durch seine statistische Durchführung ermüdenden Streites zu ermöglichen versuche.

Als Anhang füge ich ferner die kritische Analyse einiger für den Impfstreit bedeutsamen Streitschriften an. Indem ich mich hierbei, bezüglich der in beiden Lagern verschuldeten logischen Fehler, bloss auf die bereits vorher — innerhalb der systematischen Darstellung durchgeführten — Beweise zu berufen brauche, werde ich in diesem Anhange mehr Gewicht auf die Prüfung des materiellen Inhaltes der angeführten Facta legen können. In soweit ich glaube, in diesem Anhange bewiesen zu haben, welchen Unfug man im Impfstreite mit der Aufstellung statistischer Daten getrieben und wie viel Verwirrung durch eine oberflächliche, ja stellenweise sogar absichtlich entstellte, Behandlung des statistischen Materials hervorgerufen wurde, dürfte man die nicht beabsichtigt gewesene Ausdehnung, welche dieser Theil meiner Arbeit genommen, entschuldigen.

ERSTER THEIL.

ZWEITES KAPITEL.

SYSTEMATISCHE DARSTELLUNG UND KRITIK DER VACCINATIONS-STATISTIK.

Systematische Uebersicht des Impfstreites. Fünf Argumente der Impffreunde: Abnahme der Epidemieen seit dem vorigen Jahrhundert; Seltenheit der Blattern in gut impfenden Staaten; geringere Blattern-Morbidität, -Mortalität und -Lethalität der Geimpften.
Abwehr dieser Argumente Seitens der Impfgegner. Allgemeine logische Einwände derselben gegen die Impfstatistik.
Offensive der Impfgegner: Anklage auf Gefährlichkeit des Impfaktes, auf Deplacirung der Sterblichkeit, Ueberimpfung der Krankheiten und sonstige Schädlichkeiten.

Die ursprünglichen Ansichten über die Bedeutung des Vaccinationsschutzes haben sich, wie wir sahen, im Laufe der Zeit wesentlich geändert. Man schreibt der Kuhpockenimpfung gegenwärtig keinen lebenslänglichen und keinen absoluten Schutz mehr zu; man anerkennt allseitig die Nothwendigkeit der Revaccination; man anerkennt weiter die Möglichkeit von Impf-Schädigungen, und zwar sowohl durch die Impfoperation an und für sich, wie durch die Möglichkeit der Einimpfung von Krankheiten. Man muss also zugeben, dass die Impftheorie wesentliche Veränderungen, und zwar in einschränkendem Sinne, erfahren hat.

Trotz alledem aber, erscheint Das, was von der ursprünglichen Hoffnung auf den Impfschutz als gerettet betrachtet wird, noch immer sehr werthvoll. Die Blattern sind seit Einführung der Impfung denn doch seltener und deren Verwüstungen weniger mörderisch geworden. Man hält sich für überzeugt, dass Geimpfte seltener erkranken, noch mehr aber davon, dass wenn schon von der Krankheit ergriffen, derselben viel seltener unterliegen. Die Impftheorie hat auf ihrer Rückzugslinie an diesen Punkten Halt gemacht und sich hier mit Hilfe eines massenhaften und imponirenden *statistischen* Beweismaterials gegen die Angriffe der Impfgegner verschanzt. Inzwischen ist aber auch das Heer der Zweifler stark angeschwollen, und wird von diesen nicht nur die Richtigkeit der obigen Behauptungen der Impfvertheidigung, und zwar ebenfalls durch statistische Daten, geläugnet, sondern es wird durch die Behauptung, dass die Impfung geradezu schädlich sei, sogar die Offensive ergriffen.

Gehen wir nun daran, die von beiden Seiten angeführten Argumente einer unparteiischen Kritik zu unterziehen, und bringen wir zu diesem Behufe die pro und contra vorgebrachten Beweise und Thatsachen, deren grosse Menge sinnverwirrend wirkt, in ein System.

Die Argumente der Impf-Vertheidigung, die sämmtlich auf statistischen Daten beruhen, bestehen in Nachfolgendem:

1. Seitdem die Impfung in Europa eingeführt wurde, d. i. seit dem Beginne dieses Jahrhunderts, haben die Blattern, welche früher fast unausgesetzt in Europa geherrscht haben und denen fast Niemand entgehen gekonnt haben soll, viel von ihrer Gefährlichkeit eingebüsst, ja dieselben sind in manchen Ländern, so in Deutschland und Schweden, beinahe ganz verschwunden. Nennen wir dieses aus der Geschichte der Impfung geschöpfte Argument der abnehmenden Blattern-Epidemieen das historische.

2. Eine Vergleichung des Zustandes gut und schlecht impfender Staaten ergibt, dass die Blattern in jenen seltener sind. Nennen wir dies den geographischen Beweis.

Beide enthalten eigentlich nur indirekte Beweise über die Wirkung der Impfung. Die nachfolgenden Thesen sind als direkte Beweise zu betrachten:

3. Geimpfte werden seltener von den Blattern ergriffen als Ungeimpfte (Argument der geringeren Morbidität).

4. Es wird behauptet, dass von Geimpften auch weniger an Blattern sterben (Argument der geringeren Mortalität). Dieses Argument bedarf aber, da es eine natürliche Folge der vorhergenannten ist, keiner besonderen Behandlung. Grössere Wichtigkeit gewinnt jedoch dieses Argument in der als These der geringeren Lethalität zu bezeichnenden Form, nämlich

5. als Hinweis auf die Thatsache, dass wenn Geimpfte von Blattern befallen werden, die Pocken sich — Dank der Widerstandskraft des Impfstoffes — modificiren und einen weit ungefährlicheren Verlauf nehmen. Man ist sogar soweit gegangen, jene mildere Form der Blattern, welche als Variola modificata oder Variolois von den eigentlichen Blattern, Variola vera, unterschieden wird, so zu definiren, dass die Variolois die Pockenerkrankung der Geimpften repräsentire.

Die Argumente der Impfgegner hinwieder lassen sich in drei Gruppen bringen. Vor Allem halten sie den drei Thesen der Impfvertheidigung ebensoviele, ebenfalls durch statistische Beobachtungen bewiesene Negationen entgegen. Diese Negationen werden wir im Zusammenhange mit den entsprechenden positiven Thesen in Betracht ziehen. Ausser diesen, gegen den Inhalt der impfvertheidigenden Argumente gerichteten Abwehr, unterziehen die Impfgegner auch die formale, d. i. die logische, Seite der impffreundlichen Argumentation ihrer Kritik. Wir werden in diesem Betrachte zweier Einwände der Impfgegner zu gedenken haben, welche sich gegen die logische Berechtigung der aus der Statistik gezogenen impffreundlichen Folgerungen kehren, u. z. bewegen sich diese Einwände in folgender Richtung:

Soweit auf dem Gebiete der Impfstatistik von einer Mortalitäts- und Lethalitäts-Statistik die Rede sein kann, begegnet man bei den Geimpften fast ausnahmslos günstigeren Resultaten. Es wird nun von den Impfgegnern als ein logischer Fehlschluss hingestellt, wenn man diesen Umstand auf Rechnung der Impfung stellt, da

1. die Gesammtheit der Ungeimpften alle Säuglinge enthält, und die Mortalität in diesem Lebensalter bekanntlich bei allen Krankheiten die grösste ist, mithin auch die Blattern grössere Opfer fordern. Die Impfgegner fordern daher, u. z. mit Recht, dass in den bezüglichen Statistiken die Kinder von den Erwachsenen getrennt werden. Leider wurde in der Blattern-Statistik dieser wichtige Punkt bis in die jüngste Zeit nicht gehörig berücksichtigt.

Aber selbst solchen rectificirten Daten gegenüber, die voraussichtlich noch immer zu Gunsten der Geimpften sprachen, wird noch

2. der Umstand geltend gemacht, dass man stets nur gesunde und widerstandsfähige Kinder impfe, kranke und schwächliche aber zurückstelle; dass demnach die Gesammtheit der Ungeimpften stets eine Gesammtheit der Schwächeren repräsentire; es also diesem Umstande, nicht aber dem Nichtgeimpftsein zuzuschreiben sei, wenn die Lethalitätsverhältnisse bei diesen im Allgemeinen, mithin auch bei Blattern, ungünstigere sind.

Von diesen beiden Einwürfen hat der erste zur Verbesserung der statistischen Beobachtung geführt, während der zweite eine principielle Verneinung sämmtlicher zu Gunsten der Impfung zeugenden Mortalitäts- und Lethalitäts-Statistiken involvirt und eigentlich aller Vaccinationsstatistik den Boden entzieht.

Bis hierher halten die Impfgegner sich in der Defensive.

Es erübrigen aber noch die nachfolgenden drei Thesen der Impffeinde, welche — indem dieselben die Impfung geradezu unter die Anklage der Schadenstiftung stellen — die offensive Taktik des Impfangriffes repräsentiren. Diese unter der Bezeichnung "*Impfschäden*" zusammenzufassenden Thesen sind die folgenden:

1. Die Behauptung von der Gefährlichkeit des Impfaktes an und für sich, sowie dass durch die Impfung die Sterblichkeit blos deplacirt worden sei. Und zwar einerseits derart, dass die Blattern, welche früher blos als Kinderkrankheit grassirten, mit dem Fortschritte der Impfung stets höhere Altersklassen ergreifen; andererseits soll auch in

den Todesursachen eine blosse Deplacirung Platz gegriffen haben, indem im gleichen Maasse, als die Blattern abnehmen, exanthematische sowie constitutionelle Krankheiten häufiger geworden wären ;

2. wird behauptet, dass durch die Vaccination direkt Krankheiten eingeimpft werden, wobei namentlich auf die Ueberimpfung der Syphilis grosses Gewicht gelegt wird ;

3. soll die Impfung noch eine Reihe anderer Uebel verschuldet haben : nicht nur, dass die Impfung die Ursache sein soll, dass die Blattern nicht erlöschen, sondern soll diese die Ursache einer Reihe von Krankheiten, sowie mit eine Ursache der physischen, ja sogar der hiemit zusammenhängenden moralischen Depravation unserer Generation sein.

Einer so gemeinschädlichen Institution gegenüber ist es nur begreiflich, wenn die Forderung der Impfgegner in dem Verlangen nach einem Verbote der Impfung gipfelt, während die Argumente der Impfvertheidigung zur Forderung der zwangsweisen Vaccination geführt haben.

Die Gesammtentwickelung des um die Vaccinationstheorie entbrannten Kampfes, gleichsam aus der Vogelperspektive betrachtet, bietet demnach das folgende Bild :

a) IMPFVERTHEIDIGUNG.

INDIREKTE BEWEISE.

1. Abnahme der Blattern im XIX. Jahrhundert (historischer Beweis).
2. Seltenheit der Blatternepidemieen in besser impfenden Staaten (geographischer Beweis).

DIREKTE BEWEISE.

3. Geringere Morbidität der Geimpften.
4. Geringere Mortalität der Geimpften.
5. Geringere Lethalität der Geimpften.

b) IMPFANGRIFF.

KRITIK UND NEGATION.

1—5. Negation der Punkte 1—5.

6. Allgemeine logische Einwände (*a*. ungünstigere Altersbesetzung; *b*. schwächere Widerstandskraft der Ungeimpften).

POSITIVE ANGRIFFE (IMPFSCHÄDEN).

7. Schädlichkeit des Impfaktes an sich.
8. Eventuelle Ueberimpfbarkeit fremder Krankheiten, namentlich der Syphilis.
9. Sonstige physische und moralische Schädigungen.

Begeben wir uns nun auf das Terrain des Kampfes und sehen wir vorerst, wie sich derselbe in beiden Lagern um die Bollwerke der Impfvertheidigung gestaltet.

DRITTES KAPITEL.

ABNAHME DER BLATTERN-EPIDEMIE IM XIX. JAHRHUNDERT.

(ERSTER INDIREKTER, HISTORISCHER, BEWEIS.)

Dieses Argument das populärste. Ob auch das stärkste? Abwartende Haltung der deutschen Impfcommission zu dieser Frage.

Schwierigkeiten der Frage: 1) Mangel an statistischen Daten; 2) Läugnung des Thatbestandes; 3) Schwierigkeit, den Causelnexus herzustellen.

1) Die prävaccinatorische Epoche ist zugleich die prästatistische. Nur *ein* Land bietet statistische Beobachtungen: Schweden.

Die schwedische Blatternstatistik: Grosse Abnahme der Blattern in Schweden seit Einführung der Impfung. Einwände der Impfgegner: Die Epidemie nahm schon vor Einführung der Impfung ab; die geringe Anzahl der geimpften Kinder konnte das Auftreten der Epidemie kaum gehindert haben. Antwort hierauf.

2) Die Thatsachen sollen die ganze Impftheorie eigentlich Lügen gestraft haben: *a*) weil die versprochene Ausrottung der Blattern noch nicht erfolgte; *b*) weil selbst in Staaten mit strengstem Impfzwange Epidemieen vorkommen und fortwährend Geimpfte erkranken und sterben; Hinweis auf die grossen Epidemieen in Grossstädten, wo die Todesrate der Pocken oft noch grösser als vor Entdeckung der Vaccination.

3) Viele Epidemieen haben abgenommen, ja sind auch ganz verschwunden, ohne dass man geimpft hätte; warum sollte dies bei Blattern nicht möglich sein? Die Blattern, die schon zu Beginn des Jahrhunderts von selbst abnahmen, hätten auch ohne Vaccination weiter abnehmen können: Schwierigkeit, zwischen Einführung der Vaccination und der Abnahme der Blattern einen Causelnexus herzustellen; Möglichkeit anderer Ursachen. Dennoch plausibel, die Impfung als Ursache anzuerkennen. Die Impfung war ein grosses Experiment, das den Voraussetzungen entsprach; die Last des Beweises fällt also den Läugnern zu.

Alle Nachrichten, welche über die Blatternepidemieen der prävaccinatorischen Zeit auf uns gekommen, lassen annehmen, dass zu dieser Zeit die Blattern viel furchtbarer auftraten als gegenwärtig. Noch zu Zeiten Süssmilch's[1] (im letzten Drittel des XVIII. Jahrhunderts) und Hufeland's[2] (im ersten Viertel des XIX. Jahrhunderts) galt es in Europa als ausgemacht, dass jeder Mensch einmal in seinem Leben die Blattern bekommen müsse. Sehr bezeichnend ist in dieser Beziehung, wenn man z. B. bei Hildebrandt anlässlich der Besprechung der Braunschweiger Pockenepidemie v. J. 1787 die Bemerkung findet, dass wenn irgend Jemand stirbt ohne in seinem Leben die Pocken gehabt zu haben, anzunehmen sei, "dass er dieselbe im Mutterleibe müsse überstanden haben." Wer in unserer Zeit würde es mehr wagen, in einem Staate, wo geimpft wird, solche Ansichten über die Unausweichlichkeit der Pocken zu hegen? Ueberall, wo in Europa geimpft wird, betrachtet man eine Blatternerkrankung heute nur mehr als einen ausserordentlich unglücklichen Zufall. Wenn auch die Blattern noch immer, und zwar selbst epidemisch, auftreten, kennt man doch keine Fälle mehr,

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[1] Süssmilch ("Göttliche Ordnung", 4. Ausgabe, Berlin, 1775, S. 528): "Da ferner alle Menschen, bis auf sehr wenige die Pocken ausstehen müssen." Süssmilch räth entschieden zur Einpfropfung der natürlichen Blattern. "Ein Gegner derselben könnte sagen 'es wäre doch möglich, dass mein Kind unter den wenigen Auserwählten sein könnte, welche die Pocken nicht bekommen.' Aber das gilt nicht... ebensowenig, als dass man in einer Lotterie unter hundert Loosen das betreffende bekommen wird."

[2] Hufeland ("Bemerkungen über die natürlichen Blattern", Wien, 1799, 3. Auflage) bemerkt für die Stadt Halle, dass daselbst die Blattern alle fünf, sechs Jahre einkehren. "Die Mortalität war sehr leidlich, denn *nur* (!) ein Drittel starb. Wie glücklich gegen die Epidemie v. J. 1777, wo 136 starben, oder selbst gegen die weit geringer verbreitete des Jahres 1782, wo 86 starben."

dass durch dieselben, wie durch Pest und schwarzen Tod, ganze Ortschaften aufgerieben wurden.[1]

Auf die Abnahme der Pocken seit der Entdeckung Jenner's hinzuweisen, ist in Folge dessen auch das beliebteste und, in Folge seiner Unmittelbarkeit, populärste Argument der Impfvertheidigung. Ich habe schon Hufeland's und Anderer gedacht, die durch die Impfung die Pocken vom Erdballe auszurotten hofften, und noch heute führt manches der angesehensten Lehrbücher (so z. B. auch Niemeyer) dieses Factum, als zur Begründung der Impftheorie vollkommen genügend, an.[2] Es möge aber schon vorgreifend bemerkt werden, dass es um die Beweiskraft dieses Argumentes wohl doch nicht so überaus glänzend bestellt sein mag, wenn man erfährt, dass die Impf-Commission des deutschen Reiches, zehn Jahre nachdem im ganzen deutschen Reiche der Impfzwang eingeführt wurde, die Frage noch einmal aufwirft, ob die Impfung im Stande sei, ein wiederholtes Befallen von der Krankheit zu verhindern, und *ob die Abnahme der Pocken seit Anfang dieses Jahrhunderts dem Einflusse der Impfung oder anderen Einflüssen zuzuschreiben sei?* und wenn man dann sieht, dass in den Beschlüssen dieser Commission der erste Theil dieser Frage zwar als überwiegend allgemeine Regel bejaht wird, bezüglich des zweiten Theils derselben aber die Commission es nicht über ihr Gewissen bringt, auf dieselbe in bejahendem Sinne zu antworten, sondern es vorzieht, diese heikle Frage *offen zu lassen!* Es ist aber auch durchaus nicht leicht, auf diese Frage gewissenhaft zu antworten, und zwar aus nachfolgenden Gründen:

SCHWIERIGKEIT DER FRAGE.

I. MANGELS STATISTISCHER ANGABEN.

Die prävaccinatorische Periode ist zugleich die prästatistische. Weiss man ja selbst noch heute, bei dem gegenwärtig so hoch entwickelten Zustande der Statistik, noch für eine ganze Reihe europäischer Staaten die Todesarten, also auch die Anzahl der Blatterntodesfälle, nicht anzugeben. Wie wollte man dann solche Daten für das XVIII. Jahrhundert beschaffen? Unsere Kenntniss von den durch die Pocken vordem verursachten Verheerungen ist eine mehr traditionelle als statistische. Selbst Süssmilch, der grosse Statistiker des Zeitalters Friedrichs des Grossen, weiss nur Bruchstücke anzuführen. Was auf uns gekommen, lässt freilich Schauer erregende Zustände ahnen. So ist z. B. Süssmilch der Ansicht, dass kaum Jemand der Pocken-Krankheit entgehen könne und dass ein Zwölftel der Menschheit an dieser Krankheit zu Grunde gehe. Was für Zustände mögen ferner geherrscht haben, wenn man auf die wahrhaft erschütternde Notiz stösst, dass z. B. im Jahre 1756 in Gera 317 Kinder geboren wurden, von denen in diesem einen Jahre nicht weniger als 208 der schrecklichen Krankheit erlagen.[3]

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[1] Man lese z. B. in Krantz' "Geschichte von Grönland", London, 1767, (Blaubuch, S. 3), wie daselbst ganze Häuser ausstarben und die Leichen unbegraben umherlagen. Auf einer Insel fand man nur mehr drei kleine Kinder: der Vater hatte zuerst das ganze Dorf begraben und erlag schliesslich selbst den Blattern.

[2] Selbst Niemeyer, "Lehrbuch der speciellen Pathologie und Therapie", Berlin, 1871, S. 617, findet sich mit den gegen die Wirksamkeit der Impfung möglichen Bedenken mit Folgendem ab: "Gegen die auf statistischem Wege festgesetzte Thatsache, dass im vorigen Jahrhundert ein Zehntel der Menschheit an Pocken starb (in Europa etwa jährlich 400,000 Menschen), ein anderes Zehntel durch die Pocken entstellt wurde, und dass seit der Einführung der Kuhpocken-Impfung die Morbilität im Ganzen geringer geworden und die Mortalität an den Pocken auf ein Minimum reducirt worden ist, fallen alle gegen die Kuhpocken-Impfung erhobenen Bedenken, selbst wenn sie begründet wären, nicht in die Wagschale."

[3] Stricker. "Studien über Menschenblattern, Vaccination und Revaccination". Frankfurt a. M., 1861. S. 3 (nach Dr. Jani in Gera).

Derartige ziffernmässige Nachweise sind aber im vorigen Jahrhundert selten; für ganze Länder und längere Zeitstrecken existiren sie — mit Ausnahme Schweden's — gar nicht.

Die Impfgegner verfehlen auch nicht, auf die Unverlässlichkeit der auf das vorige Jahrhundert bezughabenden statistischen Daten hinzuweisen.[1] Und selbst Vertheidiger der Impfung, wie Bohn, erklären, dass bei der Frage, ob die Blattern seit Einführung der Vaccination seltener geworden seien, "auf die einfache Antwort mit einer Mortalitäts-Statistik der früheren Jahrhunderte und des jetzigen verzichtet werden müsse".[2]

Aber *eine* Hauptwaffe war doch in den Händen der Impf-Vertheidigung geblieben: es gab *ein* Land, in welchem man seit der Mitte des vorigen Jahrhunderts nicht nur Volkszählungen, und zwar jährliche, veranstaltete, sondern sogar die Anzahl der an Blattern Verstorbenen, selbst in den verlassensten Weilern, gewissenhaft registrirte. Dieses klassische Land der ältesten demographisch-statistischen Beobachtungen ist Schweden, das Reich, dem die Aufklärung und Bildung Europa's auch in sonstiger Beziehung so viel Dank schuldet.

SCHWEDISCHE BLATTERNSTATISTIK.

Die Anzahl der an Blattern Verstorbenen wird in Schweden schon seit dem Jahre 1749 registrirt, aber bis zum Jahre 1773 beziehen sich die Angaben zugleich auch auf die an Masern Verstorbenen; vom Jahre 1774 aber bis auf den heutigen Tag ist die Anzahl der an den Blattern Verstorbenen unvermengt zu entnehmen. Ich beschränke mich also in meiner hier beigefügten Tabelle[3] und der auf Grund derselben entworfenen graphischen Darstellung nur auf diese Periode.

Die Impfung beginnt in Schweden mit dem Anfange unseres Jahrhunderts und ist dieser Zeitpunkt in der Zeichnung durch eine rothe Linie kenntlich gemacht. Im Jahre 1816 erfolgte die Einführung des Impfzwanges: von da ab sind im Diagramme die Colonnen, welche die Menge der Blattern-Todesfälle (auf je eine Million Einwohner berechnet) repräsentiren, roth gefärbt. Ein Blick auf diese Zeichnung beweist nun, dass mit dem Beginne dieses Jahrhunderts in Schweden eine ganz neue Aera der Blattern-Sterblichkeit angebrochen ist. Während die graphische Darstellung der Todesfälle in den Jahren vor Einführung der Impfung eine, steilen Bergspitzen vergleichbare Zeichnung aufweist, verlaufen die Sterblichkeits-Curven der späteren Jahre wie welliges Hügelland. Der Hinweis der Impfgegner, dass übrigens auch in Schweden im Laufe der letzten Zeit die Blattern wieder in beträchtlicher Weise zunehmen, muss als unberechtigte Auxese bezeichnet werden: während in der prävaccinatorischen Periode, die wir bis zum Jahre 1816 auszudehnen zugeben, nahezu jedes zweite Jahr ein Epidemiejahr war, d. h. genauer gesprochen, unter 42 Jahren 18 waren, in denen mehr als *ein* Permille der Bevölkerung an Blattern starb, ist in den von 1817 bis 1885 reichenden 69 Jahren *kein einziges* zu verzeichnen, in welchem die Pockensterblichkeit auf *ein* Permille gestiegen wäre; während früher die Zeichnung Leichenberge von 5000, 6000,

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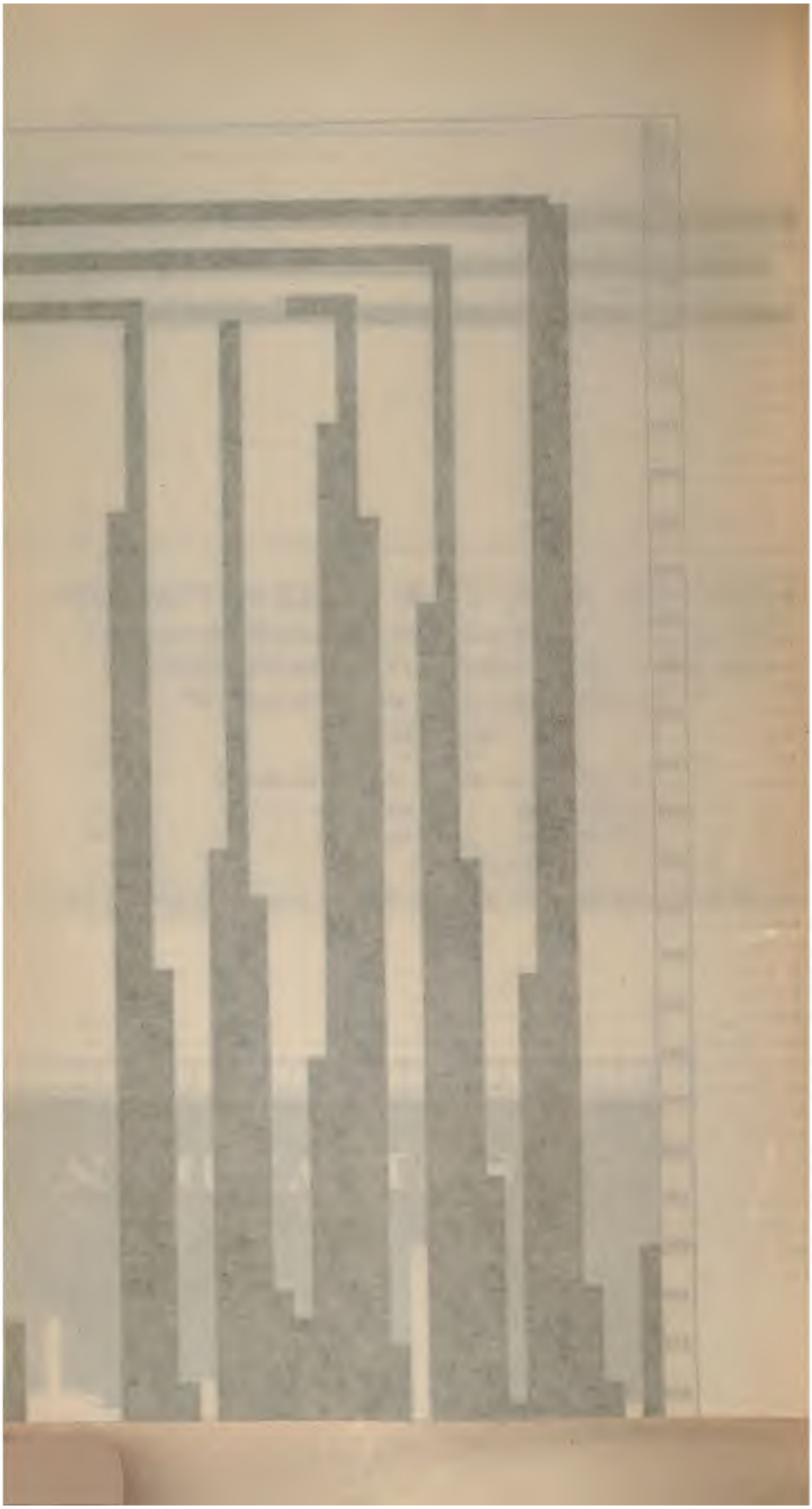
[1] So z. B. Kolb, "Zur Impffrage", Leipzig, 1877, S. 24: "Ist von Durchschnittszahlen aus ganzen Ländern, während des vorigen Jahrhunderts die Rede, so steht es zum Voraus ausser Zweifel, dass man entweder geradezu aus der Luft gegriffene Schätzungen vor sich hat, oder dass die (jedenfalls höchst ungenauen) Notirungen aus einzelnen kürzeren Perioden, vielleicht blos einer Anzahl Monate! als Proportionalzahlen für das ganze Jahrhundert gelten mussten. *Eine auch nur in rohester Weise organisirte Statistik gab es noch zu Ende des XVIII. Jahrhunderts in den meisten Ländern überhaupt nicht*".

[2] Bohn, l. c. S. 287.

[3] Siehe im Anhange dieses Kapitels. Im Hinblick auf die späteren Darlegungen ist daselbst auch die Anzahl der Typhusfälle nachgewiesen.

VON 1.000.000 EINWOHNERN
3200
3100
3000
2900
2800
2700
2600
2500
2400
2300
2200
2100
2000
1900
1800
3137



ja selbst 7000 Blatterntodten auf je eine Million Lebender repräsentirt (wobei nicht zu vergessen ist, dass die Gesammtzahl aller Verstorbenen zusammen gegenwärtig nur 20,000 beträgt), steigt die Mortalität seit Einführung des Impfzwanges nur dreimal über 500, erreicht aber nie — selbst in dem grossen Epidemiejahr 1874 nicht — die Höhe von 1000 Fällen.

Das einzige Land also, wo uns ein continirlicher Ueberblick der Blatternmortalität in der prävaccinatorischen Epoche ermöglicht ist, zeigt uns, dass die Gefährlichkeit dieser Krankheit seit Einführung der Impfung in höchst bemerkenswerther Weise nachgelassen hat. Wo immer die Vaccinationsfrage in Verhandlung kommt, wird den in Schweden gemachten, sich nunmehr schon über ein Jahrhundert erstreckenden Beobachtungen ein Ehrenplatz eingeräumt und haben dieselben keinen geringen Einfluss auf die Einführung der Impfung, ja selbst des Impfzwanges genommen.

Eben in Folge dessen widmen aber derselben auch die Impfgegner besondere Aufmerksamkeit und es wäre ungerecht, den von dieser Seite gemachten Ausstellungen eine gewisse Berechtigung abzusprechen.

Sie verweisen vor Allem darauf, dass in Schweden die Blattern eigentlich schon ein Jahr *vor* Einführung der Impfung abgenommen haben, dass also diese Abnahme keine *Folge* der Impfung sein konnte! Die graphische Darstellung bestätigt die Richtigkeit dieses Einwandes. Nicht minder richtig ist die Bemerkung, dass selbst in den ersten drei Jahren nach Einführung der Impfung in Schweden, wo nur die Nachgeborenen, und auch diese erst im späteren Alter vaccinirt wurden, höchstens 5 Procent der Bevölkerung geimpft sein konnten, die übrigen 95 Procent aber nach wie vor ungeimpft dastanden. Wie wäre es nun möglich, der Impfung eines zwanzigsten Theils der Bevölkerung so grosse Folgen zuzuschreiben, wenn man andererseits sieht, dass in Wien oder Berlin, wo doch seit einem halben Jahrhundert geimpft wird, und wo zu unserer Zeit mindestens drei Viertel der Bevölkerung geimpft ist, die Blattern in den Jahren 1882 und 1873 dennoch ebenso mörderisch auftreten konnten, wie in den ärgsten Perioden des vorigen Jahrhunderts?

Die Richtigkeit dieser Einwände muss zugegeben werden; dieselben erleiden aber durch folgende Erwägungen wieder einige Abschwächung. In Schweden waren nämlich, wie Lotz (Seite 64 und 67) treffend bemerkt, zu Beginn des Jahrhunderts nicht nur die Geimpften, sondern auch jene grosse Menge geschützt, die bereits geblattert hatte, während in Berlin und Wien seit Beginn dieses Jahrhunderts keine Epidemie mehr ausgebrochen war, so dass diese bei ihrem 1871 erfolgten Auftreten, trotz der verbreiteteren Impfung, doch eine grössere Anzahl ungeschützter Individuen vorfinden konnte. Es könnte ferner hinzugefügt werden, dass die Blatternfälle fast ausschliesslich das Kinderalter betrafen: wenn also die Erwachsenen in Folge der bereits überstandenen Blattern, die Kinder aber durch die Impfung geschützt wurden, kann der Umstand, dass die geimpften Kinder nur ein Zwanzigstel der Gesammtbevölkerung betrugen, nicht besonders ins Gewicht fallen. Was ferner den Umstand anbelangt, dass in Schweden die Blattern bei Einführung der Impfung bereits im Abnehmen begriffen waren, so kann dies, braucht aber auch gar nicht geläugnet zu werden: das Verdienst der Impfung liegt eben, wie auch Lotz hervorhebt, nicht darin, dass die Epidemie vom Jahre 1797, nachdem dieselbe 1798 ihren Höhepunkt erreicht hatte, im Jahre 1799, wie alle Epidemieen, wieder zurückging, sondern darin, dass, während sie vorher in Intervallen von circa fünf Jahren immer zurückkehrte, sie nach Einführung der Impfung weder bei ihrem letzten Aufflackern im Jahre 1805, noch im Allgemeinen zu irgend einer Zeit ihrer späteren Wiederkehr, grössere Dimensionen annehmen konnte (siehe Lotz, "Pocken und Vaccination," Seite 62).

II. LÄUGNUNG DES THATBESTANDES.

Mit Ausnahme von Schweden mangelt es uns also an systematischen statistischen Beobachtungen über die prävaccinatorische Zeit. Es behaupten aber die Impfgegner, dass soweit statistische Daten vorlägen, aus denselben die Abnahme der Blattern gar nicht zu erweisen sei, ja, dieselben führen sogar solche Mortalitätsdaten der jüngsten Zeit an, aus denen hervorgehen soll, dass trotz hundertjähriger Vaccinirung der europäischen Bevölkerung, die Blattern stellenweise noch viel verheerender auftreten als im vorigen Jahrhunderte. Die Impfgegner wenden also in dieser Phase des Impfstreites das Vertheidigungsmittel der Läugnung des Thatbestandes an. Nach ihrer Ueberzeugung haben die Thatsachen eigentlich die ganze Impftheorie Lügen gestraft. Sie verweisen hierbei darauf:

a) dass man seinerzeit der festen Ueberzeugung gewesen sei, dass die Vaccination die Blattern ausrotten werde. Dies ist nun nicht erfolgt. Selbst in Ländern, wo die Vaccination am frühesten, am allgemeinsten und am energischsten durchgeführt wurde, wie in Bayern und Württemberg, sterben noch immer Geimpfte und selbst Geblatterte an Pocken;

b) dass trotz der unglaublich raschen Einbürgerung der Impfung in allen Culturstaaten Europas, und trotzdem, dass man zur Verbreitung derselben alle möglichen Pressionsmittel anwendete (so den sogenannten "indirekten Zwang", d. i. Anempfehlung durch die Behörden, Strafe im Falle der Erkrankung, Ausschliessung Ungeimpfter von Schulen, wie dies bis 1875 in Preussen der Fall war und es noch heute in Oesterreich und Frankreich ist), trotzdem man selbst vor direkten, mit Strafen sanctionirten Zwangsmassregeln nicht zurückschreckte (so in Bayern und Württemberg schon seit Beginn dieses Jahrhunderts), diese Länder noch immer nicht nur sporadischen, sondern selbst epidemischen Ausbrüchen der Blattern ausgesetzt sind. So sind z. B. in Bayern, diesem Musterlande der Impfung, in den Jahren 1872–'73 8000 Menschen den Pocken erlegen, in dem einen Jahre 1872 allein aber 30,742 an denselben erkrankt.

Was speciell die *Grossstädte* betrifft, führt Vogt, auf Grund der in meiner "Statistique Internationale des grandes villes" gesammelten Daten, den Beweis,[1] dass während im vorigen Jahrhundert diese Seuche $\frac{1}{10}$ bis $\frac{1}{12}$ der Todesfälle verursacht haben soll, im Laufe der letzten Pandemie in Prag $\frac{1}{12}$, in Köln $\frac{1}{12}$, in Breslau $\frac{1}{11}$, in London $\frac{1}{10}$, in Lüttich $\frac{1}{10}$, in Budapest $\frac{1}{10}$, in Wien $\frac{1}{7}$, in Paris $\frac{1}{7}$, in Triest $\frac{1}{8}$, in Berlin $\frac{1}{6}$, in Rotterdam $\frac{1}{5}$, in Hamburg $\frac{1}{5}$, in Haag die Hälfte aller Todesfälle auf Rechnung der Blattern kam.[2] Solche Thatsachen veranlassen den Krankenhausdirektor Lorinser[3] zu folgender Bemerkung: "Wenn die gegenwärtig (1873) herrschende Epidemie, die an Intensität den vorherrschendsten Epidemieen des vorigen Jahrhunderts gleichkommt, alle jene Länder schonungslos durchzieht, in denen die Impfung in vollem Schwunge

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[1] Vogt. "Für und wider die Kuhpockenimpfung." Bern, 1879. Seite 183.

[2] Bloss der Richtigkeit halber und nicht um solche, bei Ziffernarbeit fast unausweichliche, Irrthümer dem Autor anrechnen zu wollen, sei hier erwähnt, dass in Haag nicht die Hälfte, sondern $\frac{1}{5}$, in Budapest hingegen nicht $\frac{1}{10}$, sondern $\frac{1}{6}$, der Todesfälle auf Blattern entfielen. Vogt citirt meine internationale Statistik der grossen Städte auch in anderen Fällen und möge deshalb noch auf folgende irrige Citate aufmerksam gemacht werden: Hamburg's Todesfälle beziehen sich auf den ganzen Staat, also auf 334,810 Einwohner (nicht auf die städtische Bevölkerung von 236,279 Seelen); es entfallen daher auf 100,000 Einwohner nicht 1544, sondern nur 1029 Todesfälle (Seite 44; man corrigire auch Seite 113). In Venedig beträgt die Anzahl der Todesfälle 493 (nicht 1084, was eine auf Seite 128 der "Statistique Internationale" angegebene Percentual-Ziffer ist); es entfallen demnach auf 100,000 Einwohner nicht 851, sondern nur 379 Fälle (siehe Seite 44).

[3] Lorinser. "Bedenken gegen die Impfung." *Wiener Medicinische Wochenschrift*, 1873. Seite 303.

ist, so muss auch dieser angebliche Beweis der Schutzkraft der Vaccine als hinfällig betrachtet werden."

In wie weit solche Schlussfolgerungen berechtigt seien, darauf soll später im Abschnitte über den "geographischen Beweis" eingegangen werden. Es genüge hier zu erwähnen, dass die von impfgegnerischer Seite angeführten Thatsachen richtig sind:[1] auch in den bestimpfenden Ländern, in Bayern, Schweden, England etc., waren Pockenepidemieen möglich, und wir finden es nicht loyal, wenn die Impfvertheidigung zur Rettung der Impftheorie mit der unbewiesenen Hypothese dazwischen tritt, dass in diesen Fällen viele Impfungen erfolglos gewesen wären, oder dass der Impfeifer abgenommen haben müsse. So lange solche Voraussetzungen nicht bewiesen werden, fordern sie mit Recht die Ironie der Impfgegner heraus. "So oft die Blattern schwächer wurden," sagt Lorinser, "trug die Impfung den Lohn davon, während wenn sie stärker auftraten, die Vernachlässigung der Impfung alle Schuld auf sich nehmen musste."[2]

III. MÖGLICHKEIT ANDERER URSACHEN.

Die Impfgegner läugnen also vor Allem die *Thatfrage:* wie man sieht, lässt es sich auch nicht behaupten, dass die Blattern im Laufe dieses Jahrhunderts verschwunden seien. Die Impfgegner glauben aber, diese These von der Abnahme der Epimedieen sogar concediren zu können, ohne hiedurch zur Impftheorie bekehrt zu sein, indem sie behaupten, dass die Natur der Blattern-Epidemieen im Allgemeinen eine mildere geworden sei, wie wir Aehnliches auch bei Scorbut und bei Cholera wahrnehmen könnten. Einige Volkskrankheiten seien ja ohne alle Impfung sogar ganz verschwunden; so kommt z. B. die Beulenpest in civilisirten Ländern nicht mehr vor. Der fortschreitenden Cultur, den Fortschritten der Heilkunde müsse doch auch ein gewisser Einfluss zugestanden werden. "Alle Seuchen," sagt Vogt (Seite 79), "weichen von selbst, ohne direktes menschliches Hinzuthun, nur die Pocken sollen die Eigenschaft besitzen, dies nicht zu thun, sondern allein der Entdeckung Jenner's gewichen sein."

Selbst Impffreunde wie Bohn geben zu, dass die glückliche Blatternzeit, die zu Beginn dieses Jahrhunderts anzubrechen schien, einem *Zusammentreffen* der Schutzpockenentdeckung mit dem Nachlassen der Krankheit überhaupt zuzuschreiben sei.

Wenn man bedenkt, wie schwer, ja strenge genommen, wie unmöglich es ist, auch bei den einfachsten Vorgängen zu beweisen, was die Ursache einer Wirkung gewesen; wenn man ferner in Betracht zieht, wie viele andere Ursachen noch auf das Zurückgehen von Epidemieen, deren Kommen und Gehen ja so räthselhaft ist, einwirken: wird man die Schwierigkeit einsehen, mit denen es verbunden ist, die Behauptung, dass die Blattern nur in Folge der Vaccination zurückgegangen seien, zu beweisen.

Andererseits lässt sich aber die gegentheilige Behauptung, wonach die Blattern im Laufe dieses Jahrhunderts von selbst schwächer geworden seien, ebenfalls nicht beweisen. Immerhin wird der unbefangene Beobachter sich in dieser Frage doch mehr auf die Seite der Impfvertheidiger neigen. Recapituliren wir nämlich den Gang

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[1] So sagt z. B. Bohn (Handbuch, S. 27) über die jüngste mit dem Jahre 1830 beginnende Periode der Blatternepidemieen: "Diese Periode gleicht einem Rückfall in vorige Jahrhunderte. In den volkreichen Städten nicht mehr ausgehend, haben die Blattern in immer grösserer und öfterer Wiederkehr fast auf allen Punkten der Erde zahllose, meist weit um sich greifende Epidemieen gesetzt, um zuletzt in einer Pandemie auszuarten, wie sie unser Jahrhundert nach Umfang und Intensität nicht erlebt hatte."

[2] Lorinser. "Aberglaube in der Medicin." Wittelshöfer's *Wiener Medicinische Wochenschrift,* 1872. No. 44.

dieser Beweisführung, so findet sich Folgendes: Auf Grund von an einzelnen Personen angestellten Beobachtungen über die Schutzkraft der Kuhpocke, gelangt man durch Raisonnement zur Annahme, dass es zweckmässig sein könnte, ganze Bevölkerungen systematisch zu impfen; man unternimmt nun dieses grosse Experiment und die Folge entspricht den Erwartungen: die Blattern werden seltener und schwächer. Wenn man nun behauptet, dieselben wären auch ohne die Impfung schwächer und seltener geworden, so muss das *onus probandi*, die Last des Beweises, billiger Weise doch Jenen zugeschoben werden, die eine so unwahrscheinliche Behauptung aufstellen; dies schon deshalb, weil die Impfgegner eine positive Behauptung zu erhärten hätten, die Impffreunde aber eine negative, was stets sehr misslich, wenn nicht unmöglich ist. Wie sollte man beweisen, was sich ereignet hätte, wenn die Vaccination *nicht* eingeführt wird? Die von den Impfgegnern angeführten Thatsachen beweisen, positiv genommen, gar nichts, höchstens nur so viel, dass trotz Einführung der Impfung noch immer Blatternepidemieen vorkommen, und dass dieselben in volkreichen Orten, namentlich in Grossstädten, leicht ausbrechen. Diese Thatsachen können aber höchstens als Beweis dafür gelten, dass zur Entwicklung der Blatternepidemieen nicht eine einzige Ursache, sondern eine ganze Reihe, und zwar gegenwärtig noch unbekannter Ursachen mitwirken mag, und dass unter diesen Ursachen einige sein mögen, gegen welche die Impfung nicht schützt. In diesem Sinne muss auch jenes bemerkenswerthe Factum gedeutet werden, dass die Grossstädte dem Blattern-Contagium einen so besonders dankbaren Boden bieten. Es möge aber hier gleich auch darauf aufmerksam gemacht werden, dass unter den Grossstädten, auf deren Epidemieen man sich zu berufen liebt, sehr wenige zu sein pflegen, in welchen gehörig geimpft wird, geschweige dass der Impfzwang eingeführt wäre. So führte z. B. Vogt fünfzehn Grossstädte an, in denen die Blatternepidemie "im Zeitalter des Impfsegens" noch immer so enorme Verwüstungen angerichtet haben. Von diesen fünfzehn Grossstädten erfreut sich aber auch nicht eine einzige des "Impfsegens", d. h. in keiner einzigen derselben ist der Impfzwang wirklich eingeführt. Ja in einigen derselben ist es um die Impfung sogar herzlich schlecht bestellt, so meines Wissens in Budapest, Wien, Paris, Prag, welch' letztere Stadt von ihrem eigenen Physikus als geradezu impffeindlich gesinnter Ort bezeichnet wird.[1]

Die Argumentation dieses ganzen historischen Beweises findet übrigens seinen eigentlichen Abschluss in dem nachfolgenden geographischen Beweise.

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[1] Pelc. "Bericht über die Sanitätsverhältnisse von Prag i. J. 1883", sagt: "Im Ganzen ist Prag impffeindlich. In den Prager Schulen sind 10—30 Procent geimpft. Revaccinationen kommen so gut wie gar nicht vor."

Sterblichkeit in Schweden, in den 111 Jahren von 1774-1884, an Blattern und Typhus.

Jahr.	Bevölkerung.	Blattern-Sterblichkeit.	Typhus-Sterblichkeit.	Jahr.	Bevölkerung.	Blattern-Sterblichkeit.	Typhus-Sterblichkeit.	Meningitis.
			(Febris continua, febris septica et petechialis.)	1827	2,827,719	600	7,871	
				1828	2,846,788	257	9,847	
				1829	2,863,132	53	9,264	
				1830	2,888,082	104	7,353	
				1831	2,901,039	612	?	
1774	1,997,809	2,065	4,947	1832	2,922,801	622	?	
1775	2,020,847	1,275	4,920	1833	2,959,141	1,145	?	
1776	2,041,289	1,503	5,358	1834	2,983,055	1,049	?	
1777	2,057,147	1,943	4,439	1835	3,025,439	445	?	
1778	2,073,296	6,607	4,337	1836	3,059,356	138	?	
1779	2,089,624	15,102	3,959	1837	3,076,184	361	?	
1780	2,118,281	3,374	3,394	1838	3,090,262	1,805	?	
1781	2,132,912	1,485	4,137	1839	3,106,459	1,934	?	
1782	2,140,986	2,482	5,046	1840	3,138,887	650	?	
1783	2,143,570	3,915	5,464	1841	3,173,160	237	?	
1784	2,145,213	12,453	6,494	1842	3,206,776	58	?	
1785	2,149,773	5,077	6,785	1843	3,236,632	9	?	
1786	2,156,109	671	6,989	1844	3,275,133	6	?	
1787	2,163,812	1,771	6,500	1845	3,316,536	6	?	
1788	2,171,866	5,462	5,858	1846	3,342,927	2	?	
1789	2,163,765	6,764	14,226	1847	3,362,072	13	?	
1790	2,158,732	5,893	11,408	1848	3,397,454	71	?	
1791	2,178,719	3,101	3,259	1849	3,441,286	341	?	
1792	2,211,643	1,939	4,226	1850	3,482,541	1,376	?	
1793	2,239,119	2,103	4,533	1851	3,516,647	2,488	?	
1794	2,266,990	3,964	4,476	1852	3,540,409	1,534	?	
1795	2,281,137	6,740	5,010	1853	3,563,316	279	?	
1796	2,300,793	4,503	3,835	1854	3,608,124	204	?	
1797	2,322,814	1,733	4,141	1855	3,641,011	41	?	
1798	2,344,228	1,357	4,737	1856	3,642,988	52	?	
1799	2,356,993	3,756	4,928	1857	3,687,601	560	?	
1800	2,347,303	12,032	5,872	1858	3,734,240	1,289	?	
1801	2,356,027	6,057	5,594	1859	3,787,735	1,470	?	
				1860	3,859,728	708	?	
			(Febris continua et septica, scarlatina miliaria.)				(Febris nerv.)	(Meningitis.)
				1861	3,917,339	193	1,110	321
				1862	3,965,899	148	1,069	277
1802	2,374,358	1,533	5,634	1863	4,022,564	307	957	286
1803	2,391,837	1,464	6,265	1864	4,070,061	741	1,139	243
1804	2,408,108	1,460	6,860	1865	4,114,141	1,332	1,590	250
1805	2,427,408	1,090	6,023	1866	4,160,677	1,217	2,187	430
1806	2,428,429	1,482	7,179	1867	4,195,681	1,061	1,862	436
1807	2,434,721	2,129	8,065	1868	4,173,080	1,429	4,240	356
1808	2,418,840	1,814	12,527	1869	4,158,657	1,474	4,069	261
1809	2,382,075	2,404	21,171	1870	4,168,525	764	2,986	215
1810	2,377,851	824	9,193	1871	4,204,177	329	1,515	222
1811	2,396,581	698	7,430	1872	4,250,412	346	1,723	211
1812	2,407,679	404	8,058	1873	4,297,972	1,122	1,926	211
1813	2,416,548	547	6,261	1874	4,341,579	4,063	1,881	268
1814	2,534,541	308	5,565	1875	4,383,291	2,119	1,942	256
1815	2,465,066	472	5,325	1876	4,429,713	604	1,407	231
1816	2,497,484	690	4,590	1877	4,484,542	357	1,137	223
1817	2,521,442	242	5,789					
1818	2,546,411	305	6,359					
1819	2,561,480	161	7,210	1878	4,531,863	202	1,550	255
1820	2,584,690	143	5,877	1879	4,578,901	144	1,158	234
				1880	4,565,668	175	1,238	245
			(Febris nervosæ, septicæ, remittentes et intermittentes.)				(Typhus, febr. nerv., meningitis.)	
1821	2,610,870	37	5,853					
1822	2,646,314	11	5,141	1881	4,572,245	299	1,143	
1823	2,689,031	39	4,166	1882	4,579,115	159	1,325	
1824	2,726,877	618	3,903	1883	4,603,595	125	1,320	
1825	2,771,552	1,248	3,962	1884	4,644,448	58	1,398	
1826	2,804,926	625	5,294	1885	4,682,769	4	1,078	

VIERTES KAPITEL.

STAND DER BLATTERNEPIDEMIEEN IN BESSER UND SCHLECHTER IMPFENDEN STAATEN.

(ZWEITER INDIREKTER, GEOGRAPHISCHER, BEWEIS.)

Vorzüge des geographischen Beweises über den historischen. Classificirung der Staaten (bez. Städte) nach gut- und schlechtimpfenden, u. z.: *a*) je nach Einführung oder Fehlen des Impfzwanges; *b*) je nach der Zu- oder Abnahme der Impfungen.

Unterscheidung zwischen innerer Disposition und äusseren Blatterkeimen. Die Impfung schwächt bloss die ersteren. Also trotz Impfung Epidemieen möglich. Richtigstellung der Frage.

Gut und schlecht impfende Staaten. Vergleichung von Schweden, Schottland, England, und Preussen mit Oesterreich und den Niederlanden. Ergebniss zu Gunsten der Impfung.

Gut und schlecht impfende Städte. Vergleichung von 21 deutschen mit 11 fremden Städten. Impfgünstige Ergebnisse. Richtigstellung der Angaben Germann's über Berlin.

Impfcuriosa. Die Beispiele der Impfgegner beschränken sich auf Ausnahmen. Curiosa gegen Curiosa.

Falsche Parallelismen zwischen Epidemieen und Verlauf des Impfgeschäftes kurz vor Ausbruch der Epidemie. Solche falsche Schlüsse sowohl bei Impffreunden, als bei -Gegnern.

Gleich der These, dass in Folge der Einführung der Schutzpockenimpfung die Blattern überall abgenommen hätten, ermangelt eigentlich auch die folgende, wonach in gutimpfenden Staaten die Blattern seltener wären als in schlecht impfenden, eines *direkten* Beweises. Wir sehen, dass sogar die Thatfrage, ob nämlich die Blattern im Laufe dieses Jahrhunderts wirklich abgenommen hätten, in Zweifel gezogen wird. Aber selbst zugegeben, dass der Thatbestand unzweifelhaft feststünde, bewiese derselbe noch nicht, dass die Epidemieen nur deshalb nachgelassen hätten, weil die Impfung eingeführt wurde. Man fände bloss, dass mit der Einführung der Impfung auch die Abnahme der Epidemieen zusammenfalle, wobei es jedoch offen bleibt, ob diese Annahme nicht einer anderen Ursache zuzuschreiben wäre. Vom streng metaphysischen Standpunkte aus könnte ein derartiger Causalismus freilich selbst für die einfachste Erscheinung nicht hergestellt werden, worauf übrigens nur deshalb hingewiesen sein soll, um uns in Erinnerung zu rufen, dass bei einem so vielfachen Ursachengewebe, welches das Kommen und Gehen der Epidemieen bedingt, es durchaus nicht so leicht hält, eine ursächliche Erklärung zu bieten. Für unser praktisches Erkennen und unser Thun können solche metaphysische Bedenken freilich ohne Einfluss bleiben. Der Einwand, dass es eine blosse Hypothese sei, wenn man die Abnahme der Blattern auf die Einführung der Impfung zurückführen wollte, braucht uns nicht weiter zu beunruhigen: alle Versuche zur Erklärung des Causalzusammenhanges äusserer Erscheinungen beruhen eigentlich nur auf Hypothesen. Die Frage ist nur, ob die Hypothesen plausibel und stark genug sind.

Alle Hypothesen über irgend eine Causalverbindung gewinnen nun an Stärke, wenn sich die Aufeinanderfolge der als Ursache und Wirkung angenommenen Erscheinungen unter verschiedenen Bedingungen wiederholt. Wenn bei einem Experimente die begleitenden Umstände (d. i. also die mit möglichen Ursachen) sich wiederholt ändern, *eine* der angenommenen Ursachen aber ständig bleibt, und wir dann sehen, dass auch eine der Wirkungen eine gewisse Stetigkeit zeigt, so wird die Wahrscheinlichkeit dafür, dass hier bloss ein zufälliges Zusammentreffen zweier Erscheinungen stattgefunden habe, stets geringer, hingegen die Wahrscheinlichkeit eines nothwendigen, also causalen Nexus stets grösser. Bei einem gewissen Punkte wird die Wahrscheinlichkeit eines nothwendigen Zusammenhanges so stark, dass dieselbe für das

praktische Leben als Gewissheit dienen kann. In der uns beschäftigenden Frage tritt nun eine solche Steigerung der Wahrscheinlichkeit ein, wenn wir verschiedene Länder oder verschiedene Städte mit einander daraufhin vergleichen, ob, je nach der grösseren oder geringeren Verbreitung der Impfung, die Blattern daselbst häufiger oder seltener sind. Finden wir, dass unsere Voraussetzungen durch eine solche Untersuchung bestätigt werden, so wird es hierdurch stets plausibeler, dass wirklich die Impfung die Ursache dessen war, dass der Ausbruch der Blattern verhindert wurde. Diese Untersuchung wollen wir nun in den nachfolgenden internationalen Vergleichungen unternehmen.

Man könnte den ersten, von dem im Laufe dieses Jahrhunderts eingetretenen Zurückweichen der Epidemie hergenommenen, indirekten Beweis den historischen, den jetzt erwähnten zweiten den geographischen nennen. Bei dem gegenwärtigen Stande der Vaccinationsstatistik müssen diese geographischen Beweise als zu den fruchtbarsten gehörig betrachtet und geschätzt werden. Dieselben umfassen zwar nur eine bescheidene Reihe von Fällen, immerhin aber doch mehr als der historische Beweis, in welchem eigentlich nur eine einzige Veränderung der begleitenden Umstände — nämlich der Uebergang vom XVIII. auf das XIX. Jahrhundert — zur Beobachtung kommt.

Will man nun in eine Vergleichung des verschiedenen Standes der Blatternepidemien in verschiedenen Staaten eingehen, so setzt ein in dieser Richtung zu versuchender Beweis eine Classifikation der zu vergleichenden Staaten (bez. Städte) nach ihrem Impfzustande voraus. Den characteristischen Eintheilungsgrund bildet hierbei das Vorhandensein oder das Ermangeln des *Impfzwanges*. Da aber der Impfzwang, namentlich zur Zeit der vor einem Decennium ausgebrochenen jüngsten Pandemie, nur in wenigen Staaten eingeführt war, sich also bei einer solchen Eintheilung bloss zwei grosse Gruppen (nämlich Staaten mit und Staaten ohne Impfzwang) ergeben würden, erscheint es berechtigt, auch die Anzahl, beziehentlich die Ab- oder Zunahme der vorgenommenen Jahresimpfungen als ein Princip der Eintheilung zu acceptiren.

Die grosse Pocken-Pandemie, welche zu Beginn der 70er Jahre dieses Jahrhunderts Europa überzog, wird von den Impfgegnern als glänzender Beweis für die Richtigkeit ihrer Behauptungen über die Nutzlosigkeit der Impfung in's Treffen geführt. Sie verweilen hierbei namentlich bei jenen Beispielen mit Vorliebe, wo in gutimpfenden Staaten die Blattern zu starken Epidemieen ausarteten, wie z. B. in Bayern und in England. Hierbei übersehen sie aber einen Cardinalpunkt.

UNTERSCHEIDUNG ZWISCHEN INNERER DISPOSITION UND ÄUSSEREN KEIMEN.

Wie für jede epidemische Krankheit, so sind auch für Blattern die Ursachen in zwei Gruppen zu bringen. Das Kommen und Gehen der Epidemieen hängt nothwendigerweise mit äusseren, uns zumeist unbekannten, Ursachen zusammen. Neben diesen äusseren, objectiven Ursachen müssen aber auch individuelle Ursachen angenommen werden, da wir sehen, dass die Epidemieen stets nur einen Theil der Menschen ergreifen, den anderen Theil aber, trotzdem derselbe denselben äusseren Ursachen ausgesetzt war, unangefochten lassen.

Nun kann im äussersten Falle auch nur so viel angenommen werden, dass durch die Impfung alle *individuellen* Factoren, d. i. die Disposition der Personen, geändert werden können. Selbst der allerstrengste Impfzwang wird aber nicht die äusseren, uns umgebenden Keime der Epidemie zerstören können; es werden also selbst bei strengstem Impfzwange Blatternfälle möglich sein. Der Werth des Impfschutzes ist folglich nicht durch die höchst oberflächliche Auffassung zu begründen, ob durch dieselbe allen Epidemieen ein Ende bereitet worden sei oder nicht. Die richtige Würdigung des durch Impfung gebotenen Nutzens ergiebt sich vielmehr aus der Frage, ob für den Fall, dass die

objektiven Krankheitsursachen, die äusseren Keime, zu wirken beginnen, die durch die Impfung hervorgerufene Aenderung der inneren Disposition die Ausbreitung dieser Keime hemmt oder nicht?

Von diesem Standpunkte aus sind also die Vergleichungen zwischen gut- und schlechtimpfenden Staaten zu beurtheilen. Wir werden demnach nicht darauf unser Augenmerk richten, ob in Staaten mit Impfzwang Blattern gar nicht mehr vorkommen, sondern darauf:

1. ob dieselben daselbst seltener sind?

2. ob deren Seltenheit mit der Einführung bez. allgemeinen Anwendung der Impfung zusammenfällt?

3. ob in Fällen, wo eine namhafte Steigerung der äusseren Factoren eintrat (Epidemieen), die schwächere Disposition einen Druck auf die Verbreitung der Blattern ausübte, also ob in Epidemiezeiten gutimpfende Länder nicht besser gestellt sind als schlechtimpfende?

VERGLEICHUNG GUT- UND SCHLECHTIMPFENDER STAATEN.

Wir vergleichen in Folge dessen:

Schweden, Schottland,
England, Preussen,

als Länder mit Impfzwang, mit:

Oesterreich und den Niederlanden,

als Länder ohne Impfzwang.

Indem wir jedes dieser Länder für eine längere Periode in Betracht ziehen, kommen wir in die Lage, den Einfluss der Einführung oder der Verallgemeinerung der Vaccination — also den historischen Beweis — mitverfolgen zu können.

Die Ziffernwerthe und Quellen sind am Schlusse dieses Kapitels angegeben. Die Resultate sind, der unmittelbar anschaulichen Erkenntniss zuliebe, auch graphisch dargestellt, und zwar in dem im vorigen Kapitel erwähnten Diagramme der schwedischen Pockensterblichkeit. Die *vor* Einführung des Impfzwanges bestandene Sterblichkeit ist für jeden Staat durch schwarzes, die *nach* Einführung des Impfzwanges beobachtete Sterblichkeit durch rothes Colorit dargestellt.

Ein Blick auf diese graphische Zeichnung lehrt nun, dass die Staaten ohne Impfzwang eine viel höhere Pockensterblichkeit aufweisen, als Staaten mit Impfzwang, und dass in jedem Staate mit Einführung strengerer Impfmassregeln die Blattern an Gefährlichkeit verlieren. In schlechtimpfenden Staaten sterben die Pocken fast nie aus, in gutimpfenden kommen sie fast gar nicht vor. In ganz Schottland kommen in ruhigen Zeiten nur ein Dutzend, in vielen Jahren nur drei bis vier Pockentodesfälle vor! Auch in England finden sich Jahre, wo fast gar keine Pockentodesfälle vorfielen, so starben in ganz England:

1873 nur 3 Personen.	1879 nur 12 Personen.
1874 " 2 "	1882 " 11 "
1875 " 1 "	1883 " 2 "
1878 " 12 "	

Desgleichen kam in Preussen seit Einführung des Impfzwanges nur folgende Anzahl von Pockentodesfällen vor:

1875: 926	1880: 710
1876: 810	1881: 990
1877: 88	1882: 1007
1878: 188	1883: 547
1879: 339	

Demgegenüber finden wir in Oesterreich (wohlgemerkt ohne Ungarn!) selbst in den günstigsten Jahren nicht weniger als ungefähr 5000 Todesfälle (z. B. 1865: 4767,

1870: 6187, 1862: 6751 u. s. w.). In Wien oder Budapest allein sterben mehr Menschen an Blattern, als in ganz Schottland oder in ganzen Königreichen Deutschlands, seit daselbst der Impfzwang eingeführt ist.[1]

Um speziell den wohlthätigen Einfluss der Impfung auf den Ausbruch der *Epidemieen* nachzuweisen, mögen hier die nachfolgenden statistischen Daten Raum finden, welche die Verheerungen der jüngsten Pandemie in schlechtimpfenden Staaten nachweisen. Es erlagen bei dieser Gelegenheit den Blattern von je einer Million Einwohner in gutimpfenden Ländern, wie Schweden, England, Schottland, Bayern, 1400—1800 Menchen, hingegen in schlechtimpfenden, wie Preussen, Oesterreich (nur Cisleithanien) Niederlande, 5000—6600! Es betrugen nämlich die Pockentodesfälle

a) IN GUTIMPFENDEN LÄNDERN:

Schottland	1871–72	6,260=1,470	auf eine Million Einwohner.
Schweden	1873–75	8,072=1,660	" " " "
Bayern	1873–75	6,260=1,660	" " " "
England	1871–72	42,048=1,830	" " " "

b) IN SCHLECHTIMPFENDEN LÄNDERN:

Preussen	1871–72[2]	145,048=5,060	auf eine Million Einwohner.
Niederlande	1870–72	20,231=5,490	" " " "
Oesterreich (Cisleithanien)	1872–74	141,084=6,180	" " " "

Hätte also z. B. in Preussen die jüngste Pandemie ebenso wirksame Schutzeinrichtungen vorgefunden wie in Grossbritanien, Schweden oder in Bayern, so wären hierdurch in diesem Lande etwa hunderttausend Menschen vor dem Blatterntode gerettet worden, in Oesterreich aber 120,000!

Freilich lässt sich nicht in Abrede stellen, dass auch in schlechtimpfenden Staaten die Blatternepidemieen mehrjährige Pausen aufweisen, dass also der in Deutschland beobachtete Niedergang der Epidemie möglicherweise doch nur das dem Wellenberge folgende und der nächsten Hebung vorausgehende Wellenthal sein könne. So sehen wir z. B., dass gegenwärtig die Niederlande, trotzdem daselbst der Impfzwang nicht eingeführt ist, günstiger stehen als das Zwangsimpfung übende England. Andererseits findet sich, dass hie und da trotz der verschärften Impfmassregeln, die Blattern zugenommen haben. Man vergleiche z. B. in der graphischen Darstellung die Mortalitätscurve Englands, wo trotz der stets strengeren Gesetzgebung die Pockensterblichkeit sich zu Beginn der sechziger Jahre auf gleicher Höhe mit Oesterreich hält, und wo gerade knapp nach Einführung des strengen Impfgesetzes in 1867, freilich beim Einbruch der Pandemie, die Curve wieder stark aufsteigt. In solchen Fällen hat eben die Kraft der äusseren, durch die Impfung nicht berührten Blatternkeime zugenommen. Freilich hat ein derartiges Herüber- und Hinübergreifen in den Erklärungen manches Missliche an sich. Wir dürfen aber eben nie vergessen, dass wir es hier nicht mit jenem direkten Beweise zu thun haben, ob von einer abgesonderten Menge von Ungeimpften mehr erkrankten als von Geimpften, sondern bloss mit dem indirekten Beweise, ob dort, wo mehr Geimpfte vorhanden sind, mehr oder weniger den Blattern verfallen? Auf jenem direkten Beweise ruht ohne Zweifel das Hauptgewicht der Beweisführung; wir werden uns aber mit diesem erst in den nächsten zwei Abschnitten, sowie im zweiten Theile zu beschäftigen haben.

Uebergehen wir nun zur Vergleichung der gefährlichsten Heerde der Epidemieen, nämlich zu den *Städten*.

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[1] Als im Laufe des heurigen Jahres die Durchführung des Impfzwanges im ungarischen Abgeordnetenhause verhandelt wurde, erregte Ministerpräsident Tisza gerechte Verwunderung durch die Bemerkung, dass man an deutschen Universitäten, da den Studenten daselbst mehr gar keine Blatternfälle vorgeführt werden können, die Aerzte nach Ungarn sende, damit sich ihnen Gelegenheit biete, Blatternkranke zu sehen.

[2] Also vor Einführung des neuen Impfgesetzes.

VERGLEICHUNG GUT- UND SCHLECHTIMPFENDER STÄDTE.

Diesbezüglich enthalten die Protocolle der deutschen Impfcommission sehr dankenswerthe Zusammenstellungen, die wir hier folgen lassen. Es starben nämlich von je 100,000 Einwohnern an Pocken in den Jahren :

1875—1883 (9 Jahre).	Zusammen.	Jährlich.	1875—1883 (9 Jahre).	Zusammen.	Jährlich.
Berlin,	14.9	1.6	Crefeld,	4.5	0.4
Hamburg,	5.7	0.6	Dortmund,	4.4	0.4
Breslau,	15.9	1.7	Heilbronn,	176.7	14.7
München,	13.0	1.4	Beuthen,	357.3	29.8
Dresden,	14.3	1.6	Liegnitz,	Niemand (!)	0.0
Hannover,	2.4	0.2	Königshütte,	108.5	9.0
Elberfeld,	4.3	0.4	Bochum,	5.3	0.4[1]
Magdeburg,	2.3	0.2	Regensburg,	8.9	0.7
Altona,	2.3	0.2	Trier,	4.0	0.3
Stettin,	4.9	0.4	M. Gladbach,	Niemand (!)	0.0
Aachen,	98.2	8.2			

Man vergleiche nun hiermit die Zustände in benachbarten, also ähnlichen Einflüssen ausgesetzten, aber weniger gutimpfenden Städten, wie diese aus der am Schlusse des Capitels beigefügten, den genannten Impfprotokollen entnommenen Tabelle von Jahr auf Jahr ersichtlich gemacht sind und aus welcher wir die nachfolgenden Durchschnittswerthe berechnen :

Paris	1875–83,	jährlich 33.4	Lemberg	1873–83,	jährlich 139.6
Wien	1875–83,	" 89.2	Krakau	1874–83,	" 119.3
Warschau	1878–86,	" 118.2	Troppau	1873–83,	" 83.9
St. Petersburg	1878–83,	" 117.0	Brünn	1873–83,	" 66.4
Budapest	1874–86,	" 113.2	Brüssel	1870–83,	" 61.4
Triest	1873–83,	" 64.9			

Von den angeführten deutschen Städten weisen also das Maximum Beuthen mit 30, Heilbronn mit 17, Königshütte mit 9 Fällen auf, während in den übrigen Städten von 100,000 Menschen *kaum Einer* jährlich den Pocken erliegt. In den benachbarten, schlechter impfenden, Städten hingegen beträgt schon das Minimum 33 Fälle, das Maximum aber 117, 118, 119 (St. Petersburg, Warschau, Krakau), ja in Lemberg sogar 140; selbst in Wien starben noch 89 auf 100,000 Einwohner.[2]

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[1] Im Jahre 1871, vor Einführung des Impfzwanges, 3553!

[2] Germann, "Historisch-statistische Daten über den jetzigen Stand der Impffrage," gibt (I. Band, Seite 15) eine Dr. Toni (Löhnert in Chemnitz) entnommene graphische Darstellung, welche beweisen soll, dass die Blatternsterblichkeit im vorigen Jahrhundert, also vor Einführung der Impfung, geringer als in der Pockenepidemie des Jahres 1871 war. Diese Parallele ist jedoch nicht berechtigt. Vor Allem hat es keinen Sinn die Sterblichkeit Berlins aus sechs auseinanderliegenden Jahren mit der Sterblichkeit 17 anderer Städte in dem einen Jahre 1871 zu vergleichen. Ferner ist zu bemerken, dass in Deutschland das Jahr 1871, höchst wahrscheinlich in Folge des deutsch-französischen Krieges, ein so arges Pockenjahr war, wie es im Laufe des ganzen Jahrhunderts nicht vorgekommen. Um ein richtiges Urtheil zu gewinnen, hätte Germann die einzelnen Städte unter sich, oder gutimpfende Städte mit schlechtimpfenden vergleichen müssen. Wir wollen diese Parallele an Germann's Statt consequent durchführen und zugleich bis auf den heutigen Tag verlängern. Es starben in Berlin von je 100,000 Einwohnern:

a) *vor Einführung der Impfung:* In der Epidemie von 1759, 526; 1766, 360; 1770, 383; 1786, 345; 1789, **500**; 1801, **434**;

b) *nach Einführung der Impfung ist durch sechzig Jahre* **keine** *Epidemie zu verzeichnen.*

Dann folgt die Epidemie von 1864 mit 281, 1871 mit 392 und 1872 mit 348 Verstorbenen. (Bis hieher sind die Angaben Guttstadt's Artikel in der Zeitschrift des preussischen statistischen Bureaus 1873, entnommen.) In den zwei weiteren Jahren bis zur Einführung des Impfzwanges starben: 1873, 1.0; 1874, 0.02;

c) *nach Einführung des Impfzwanges kommen in Berlin* **fast keine** *Pocken-Todesfälle vor.* In neun Jahren entfielen im Ganzen auf 100,000 Einwohner 14½, also per Jahr 1½!

Die Vergleichung gut und schlecht impfender Staaten und Städte führt also gleichmässig zu der Annahme, dass die Impfung einen grossen Schutz vor den Blattern biete. Dieser Beweis ist zwar, wie bereits erwähnt, nicht direkt erbracht, da wir nämlich nicht die Morbidität oder Mortalität einerseits der Geimpften, andererseits der Ungeimpften vergleichen konnten; da aber die angeführten zahlreichen Fälle sich alle darin gleichen, dass betreffenden Ortes gut oder schlecht geimpft wird, und da das Resultat fast ohne Ausnahme stets dieselbe impfgünstige Tendenz zeigt, so wird die Wahrscheinlichkeit dafür, dass ein Causalnexus zwischen Impfung und Blatternschutz vorliege, eine so grosse, dass man denselben, praktisch genommen, als hergestellt betrachten kann. Man ersieht übrigens hieraus wieder, von welch grossem Interesse es sei, diesen Beweis auch auf direktem Wege zu erbringen. Dies soll eben im zweiten Theile dieses Referates versucht werden.

Wir sehen nun auch, welche Bedeutung jenen Nachweisen der Impfgegner beizulegen sei, wonach trotz Vaccination, Revaccination und Impfung noch immer Blattern-Epidemieen vorkämen, und dass dieselben in gewissen Orten, namentlich in grossen Städten, noch sehr stark aufträten. Die Frage ist aber doch die: "Was ist die allgemeine Regel? Haben seit Einführung der Impfung — und wohlgemerkt *dort, wo* die Impfung eingeführt wurde! — die Blattern in der Mehrzahl der Fälle zugenommen, abgenommen, oder ist der Zustand ein unveränderter geblieben?"

Auf eine derart formulirte Frage wird jede unbefangene statistische Beobachtung zu Gunsten der Impfung aussagen müssen. Was die Impfgegner hiergegen vorbringen, ist nichts Anderes, als eine Sammlung von Ausnahmen, ein Kuriositäten-Kabinet der Impf-Statistik, das nur Denjenigen zu verwirren vermag, der die daselbst nicht vorgezeigten, normalen Folgen der Schutz-Impfung nicht kennt.[1] So wird man Uneingeweihten

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[1] Curiosa verfehlen zwar selten den beabsichtigten Eindruck, sollten aber bei grossen Fragen nie als beweisende Argumente angeführt werden. Dieselben leisten der wissenschaftlichen Forschung genügenden Dienst, wenn sie Staunen erregen und so zu Ausgangspunkten systematischer Beobachtungen werden. Die ganze Jenner'sche Entdeckung von dem Schutze der Kuhpockenimpfung nimmt ja in jenem Curiosum ihren Anfang, dass Jenner als Landarzt bemerkte, wie Kuhmägde, die von den Euterpocken inficirt wurden, sich gegen die Blatternkrankheit immun erwiesen. Käme es statt auf unbefangene Sammlung statistischer Massenbeobachtungen bloss auf eine Sammlung erstaunlicher Curiosa an, so würde die Impfvertheidigung wahrscheinlich nicht den Kürzeren ziehen. So erwähnte z. B. Eulenburg in den Verhandlungen der deutschen Impfkommission v. J. 1885 eines Gutsbesitzers in Posen, der alle seine Arbeiter, 150 an der Zahl, impfen liess; ein einziger weigerte sich und dieser bekam die Pocken und erlag denselben auch. Als einen weiteren interessanten Fall will ich den nachfolgenden anführen, welchen ich, seines drastischen Inhaltes willen, mir angelegen sein liess, mit aktenmässiger Genauigkeit zu verfolgen.

Die Revaccination des österreichisch-ungarischen Heeres war bis zum Jahre 1886 eine ziemlich mangelhafte und die Blatterntodesfälle in dieser Armee auch ziemlich zahlreich, wie sich dies später aus einer Vergleichung mit der Blatternsterblichkeit des preussischen Heeres noch ergeben wird. Im Jahre 1886 erfolgte — und zwar, wie es heisst, auf die direkte Veranlassung des Kronprinzen *Rudolf* — die Einführung der allgemeinen Revaccinationspflicht für die Truppe. Hierbei ereignete sich in Budapest der nachfolgende merkwürdige Fall, für den ich dessen Gewährsmann selbst sprechen lasse. Dr. Michael Vollerii, Regiments- und Chefarzt in der Garnison Budapest, schreibt mir unter dem 9. Dezember 1886 wie folgt: "Die gesammte Mannschaft der zwei in Budapest garnisonirenden Bataillone des Infanterie-Regimentes No. 23 wurde in den Monaten Dezember und Januar 1885 mit sehr gutem Erfolge wiedergeimpft. Die, bei welchen kein Erfolg sichtbar, wurden zum zweiten Male der Impfung unterzogen. Am 3. Oktober 1886 sind 250 Rekruten eingerückt; 249 wurden am 8. Oktober wiedergeimpft und zwar 208 mit Erfolg, 41 ohne Erfolg. Rekrut Josef Teleki der 8. Kompagnie ist der Einzige, welcher nicht geimpft wurde, da er sich am Impftage mit Abscess im Marodenzimmer befand und so entging. Dieser erkrankte Ende Oktober an Blattern und kam am

wohl mit dem Factum imponiren können, dass, trotzdem in Bayern seit Beginn dieses Jahrhunderts strenger Impfzwang praktizirt werde, in den Jahren 1873–'75 daselbst dennoch viele Tausende an Blattern gestorben seien; man dürfte aber dann nicht verschweigen, dass zu gleicher Zeit im weniger streng impfenden nachbarlichen Oesterreich gerade viermal so viel an Blattern starben! Man könnte auf das Factum hinweisen, dass in Deutschland trotz Einführung des Impfzwanges, in Aachen, in Heilbronn, in Beuthen mehrere Todesfälle vorkommen konnten. Man setzt aber nicht hinzu, dass dies nur Ausnahmen sind, dass in fast allen übrigen Städten Deutchlands die Blattern beinahe ausgerottet erscheinen, und zwar derart, dass in einzelnen Städten ein ganzes Jahrzehnt hindurch nicht ein einziger Todesfall vorgekommen!

Die von Impfgegnern beliebte Sammlung der Curiosa beweist im besten Falle nur so viel, dass zur Entwickelung von Blattern-Epidemieen nicht nur *eine* einzige und bekannte, sondern eine ganze Reihe von unbekannten Ursachen mitwirken, und dass unter diesen Ursachen einige sein mögen, gegen welche die Impfung vielleicht nicht schützt.

FALSCHE PARALLELISMEN ZWISCHEN VERBREITUNG DER IMPFUNG UND EPIDEMIE-AUSBRUCH.

Seitens der Impfgegner wird schliesslich wiederholt der Nachweis geliefert, dass, trotzdem in einem gewissen Jahre die Anzahl der vorgenommenen Impfungen gestiegen war, dennoch kurz darauf eine Blattern-Epidemie ausbrechen konnte. Solche Beweise sind aber schon a priori als unbrauchbar zu beseitigen. Man kann zwei, drei und mehrere Jahre hindurch fleissig geimpft haben und der Ausbruch einer verheerenden Epidemie ist trotzdem möglich, einfach schon deshalb, weil trotz fleissiger Impfung in den letzten Jahren, noch immer sehr viele Ungeimpfte von den Vorjahren zurückbleiben konnten. Und selbst wenn dies nicht der Fall wäre, wurde ja schon bewiesen, dass zwischen den subjektiven Ursachen (Disposition) und zwischen den äusseren, objektiven Ursachen (Epidemiekeim) unterschieden werden müsse. Selbst wenn alle Menschen geimpft wären, werden Epidemieen noch immer möglich sein. Es involvirt eine tendentiöse Entstellung, wenn die Impfgegner der Impfvertheidigung die Argumentation unterschieben, als ob die Nichtgeimpften, beziehungsweise die Nichtimpfung, die Ursache der Blattern-Epidemieen wären.[1] Es ist meines Wissens noch keinem Impfvertheidiger eingefallen, die Nichtgeimpften als die wirkende Ursache, als jenes unbekannte X hinzustellen, welches die Epidemie hervorruft; sondern es wird eine grosse Anzahl Ungeimpfter stets nur als eine günstige Vorbedingung, als eine Präparation des Bodens betrachtet, auf welchem der aus unbekannten Ursachen entstehende Blatternkeim aufgehen kann.[2]

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1. November sub Kranken-Aufnahmszahl 2394 in's Garnisonsspital No. 17 in Behandlung. Es ist zwar nur ein Fall noch nichts beweisend, doch auffallend. Ich bin aber fest überzeugt, dass nur in Folge der Wiederimpfung keine Blatternerkrankungen bei der Mannschaft vorgekommen sind, da diese zu allen Infektionskrankheiten ungemein disponirt und auch im Vergleiche zu den anderen in Budapest garnisonirenden Truppenkörpern stets mit der grössten Ziffer daran partizipirt. So war es im Winter 1884–'85 mit Lungenentzündung, 1885–'86 mit Masern der Fall. In meiner Ueberzeugung werde ich noch dadurch bestärkt, dass von den 35 Offizieren, welche dem Impfzwange nicht unterliegen, und von welchen sich nur zwölf wiederimpfen liessen, zwei, und zwar beide Nichtrevaccinirte, an Blattern erkrankt sind."

1 So sagt z. B. Vogt Seite 80: "dass wir keine Pockenschrift aus der medicinischen Schule besitzen, die nicht das epidemische Aufflackern der Krankheit allein (?) einer unzulänglichen Impfung und deren Verschwinden dem eifrigen Gebrauche der Lanzette zuschriebe."

2 Man vergleiche Pettenkofer's Theorie über die Entstehung der Cholera-Epidemieen. Der geniale Autor hat hier schon vor Jahren auf die Nothwendigkeit hingewiesen, zwischen zwei entschiedenen Reihen von Ursachen zu unterscheiden.

Es kann die Statistik der Impfungen also bloss als ein *annäherndes* Symptom zur Beurtheilung des allgemeinen Impfzustandes eines Landes betrachtet werden. Eine auf dieses Symptom aufgebaute Beweisführung hat aber mit der Schwierigkeit zu kämpfen, dass der Beweisgang zum Theile auf ein Negativum gerichtet ist. Denn es soll bewiesen werden, dass dort, wo viel geimpft wird, Epidemieen *nicht* so leicht ausbrechen: die zu untersuchende Wirkung ist also eine negative. Es findet diese Schwierigkeit auch nicht immer Ausgleichung durch den umgekehrten Gedankengang, dass dort, wo wenig geimpft wird, Epidemieen leichter zum Ausbruche gelangen und auch stärker auftreten müssen; denn es ist ja gar nicht nothwendig, dass in solchen Orten in Folge der solcherart gesteigerten subjektiven Disposition auch die objektive Ursache ins Leben gerufen wurde. Man wird demnach diese Frage am richtigsten nur in folgender Weise formuliren können: Zeigt sich, bei Ausbruch einer mehrere Territorien überziehenden Epidemie, dort, wo im Laufe der vorangegangenen Jahre besser geimpft wurde, eine geringere Ausbreitung der Seuche, oder nicht? Der bessere Fortgang des Impfgeschäftes mag hierbei nach zwei Richtungen hin untersucht werden: 1) ob im Verhältnisse zur Bevölkerung[1] in dem einen Territorium mehr geimpft wurde, als in dem anderen? 2) ob in ein und demselben Territorium im Laufe der jüngsten Zeit die Anzahl der Impfungen eine verhältnissmässig zunehmende oder abnehmende sei? Hier in eine specielle Untersuchung dieses Zusammenhanges einzugehen, ist aber nicht unbedingt geboten. Das gesuchte Resultat wird nämlich davon abhängig sein, ob der *direkte* Impfschutzbeweis herzustellen sei oder nicht. Da die Kritik des direkten Beweises uns in den nächsten zwei Abschnitten beschäftigen wird, überdies aber es die Hauptaufgabe dieser ganzen Arbeit ist, auf diese gewichtigste Frage des ganzen Impfstreites eine Antwort zu bieten, können wir uns an dieser Stelle des weiteren Eingehens in die diesbezüglich vorgebrachten statistischen Facta entheben. Wir begnügen uns deshalb, hier nur im Allgemeinen darauf hingewiesen zu haben, dass die Parallelismen zwischen Impfungsdichtigkeit und Epidemieenausbruch keine genügende Beweiskraft besitzen.

Derartige falsche Parallelismen zwischen der Anzahl der Impfungen einerseits und Erkrankungs- und Todesfällen an Blattern andererseits sind übrigens nicht blos von Impfgegnern, sondern auch von Impffreunden aufgestellt worden. So ist ja jene von der Impfvertheidigung so nachdrücklich betonte Behauptung, dass i. J. 1802 in Schweden die Pocken in Folge der Einführung der Vaccination erloschen, eigentlich auch nichts Anderes, als ein derartiger falscher Parallelismus. Die ersten wenigen Vaccinationen geschahen nämlich in Schweden erst im Jahre vorher.

Desgleichen verfällt der Impffreund Brunner[2] in denselben Fehler. Wiederholt

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[1] Vogt mag im Rechte sein, wenn er die Anzahl der Impfungen lieber zur Anzahl der Geburten als zu jener der Lebenden in Verhältniss setzen will. Bei dem Umstande, dass sich das Geburtenverhältniss im Laufe einer Generation nicht zu stark zu ändern pflegt, namentlich aber mit Rücksicht darauf, dass es sich hier nicht um die Messung einer Thatsache, sondern blos um Gewinnung einer allgemeinen Orientirung, eines blos annähernden Symptomes handelt, dürften beide Arten der Berechnung je nach Bequemlichkeit anwendbar sein. Vogt hat z. B. die diesbezüglich von Guttstadt für Berlin nach der Einwohnerzahl berechnete Verhältnissziffer auf Geburten umgerechnet. (Siehe "Für und wider die Kuhpockenimpfung." S. 106). Eine Vergleichung der beiderseitigen Zahlen ergibt, dass die einzelnen Perioden auf beiden Wegen fast gleichmässig characterisirt erscheinen, trotzdem sich diese Berechnung auf mehr als drei Decennien erstreckt, welch langer Zeitraum hinlängliche Gelegenheit für Schwankungen des Geburts-Coëfficienten bot.

[2] Aus Vogt, "Für und wider die Kuhpockenimpfung," S. 7, citirte ich folgende Stelle:

"Brunner beginnt den Abschnitt über die Pocken (im Züricher Canton von 1801—1820) mit den Worten: Im gleichen Maasse wie die Impfung Fortschritte machte, trat die Pocken-

macht derartige unrichtige Schlüsse Vogt ("Für und wider die Kuhpockenimpfung"), so bei Besprechung der in Frankreich, in Berlin und Oesterreich bestehenden Verhältnisse. Bezüglich des letzten Staates heisst es z. B. Seite 130: "Die pockenreichsten Bezirke zeigen hier im Durchschnitte für das Jahr 1874 eine 173 Mal grössere Pockensterblichkeit als die gleich grosse Bevölkerung der pockenärmsten Bezirke, und dennoch war im unmittelbar vorhergehenden (!) Jahre in jenen die Impfung eifriger betrieben worden als in diesem".[1]

Die auf solche Prämissen begründeten Beweise sind also — gleichviel, ob sie zu Gunsten oder Ungunsten der Impfung sprechen — im Vorhinein als unrichtig bei Seite zu lassen.

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seuche zurück, — und stützt sich daselbst die Annahme eines solchen ursachlichen Zusammenhanges zwischen Impfung und Pocken darauf, dass im Canton Zürich in den Triennium 1804—1806, in welchem 4018 Kinder geimpft worden waren, 2171 an Pocken erkrankt seien, während im folgenden Triennium, in welchem 8018 Kinder geimpft waren, nur noch 219 Personen von Blattern ergriffen wurden." — Wie man sieht, fehlt hier die wichtigste Voraussetzung, ob nämlich im Laufe des zweiten Trienniums, falls man weniger fleissig geimpft hätte, eine Pockenepidemie zum Ausbruche gekommen wäre. Da die Impfung den Ausbruch der Epidemie verhindern soll, so gelangt man bei dieser Erwägung zu dem scholastischen Begriffe einer potentiellen, aber nicht erkennbaren Epidemie. Diesem logischen Undinge lässt sich nur beikommen, wenn man nachweisen kann, dass ringsumher, in schlechter impfenden Cantonen, zu dieser Zeit wirklich Blatternepidemien grassirten, während der besser impfende von denselben weniger zu leiden hatte.

[1] Dieses "eifriger" reducirt sich auf den Unterschied von 90.3 Procent Geimpfter gegen 89.9 Procent.

POCKENSTERBLICHKEIT IN LONDON, 1838-1885.*

Jahr.	Volkszahl.	Pocken-todes-fälle.	Auf je 100,000 Lebende.	Jahr.	Volkszahl.	Pocken-todes-fälle.	Auf je 100,000 Lebende.
1838	1,766,169	3,817	216	1862	2,860,117	366	13
1839	1,802,751	634	35	1863	2,905,210	1,996	69
1840	1,840,091	1,235	67	1864	2,950,361	547	19
1841	1,878,205	1,053	56	1865	2,995,551	640	21
	Gesetzliche Einführung der Impfung.			1866	3,040,761	1,391	46
				1867	3,085,971	1,345	44
1842	1,917,108	360	19		Neues Impfgesetz.		
1843	1,954,041	438	22				
1844	2,033,816	1,804	87	1868	3,131,160	597	19
1845	2,073,298	909	44	1869	3,176,308	275	9
1846	2,113,535	257	12	1870	3,221,394	973	30
1847	2,202,673	955	43	1871	3,267,251	7,912	242
1848	2,244,837	1,620	72		Neueste Verschärfung des Impfzwanges.		
1849	2,287,302	521	23				
1850	2,330,054	499	21	1872	3,319,736	1,786	54
1851	2,378,081	1,062	45	1873	3,373,065	113	3
1852	2,416,367	1,159	48	1874	3,427,250	57	2
1853	2,459,899	211	9	1875	3,482,306	46	1
	Impfzwang.			1876	3,538,246	736	21
				1877	3,595,085	2,551	71
1854	2,503,662	694	28	1878	3,652,837	1,417	39
1855	2,547,639	1,039	41	1879	3,711,517	450	12
1856	2,591,815	531	20	1880	3,771,139	471	12
1857	2,636,174	156	6	1881	3,831,719	2,367	62
1858	2,680,700	242	9	1882	3,893,272	430	11
1859	2,725,374	1,158	42	1883	3,955,814	136	3
1860	2,770,181	898	32	1884	4,019,361	898	22
1861	2,815,101	217	8	1885	4,083,928	899	22

* Annual Summary of births, deaths and marriages in London and other great towns, 1885. S. XXII.

POCKENSTERBLICHKEIT IN ENGLAND, 1838-1885.*

Jahr.	Volkszahl.	Pocken-todes-fälle.	Auf je 100,000 Lebende.	Jahr.	Volkszahl.	Pocken-todes-fälle.	Auf je 100,000 Lebende.
1838	15,287,699	16,268	106	1862	20,371,013	1,579	8
1839	15,514,255	9,131	59	1863	20,625,855	5,891	29
1840	15,730,813	10,434	66	1864	20,883,887	7,624	37
1841	15,929,492	6,368	40	1865	21,145,151	6,361	30
	Gesetzliche Einführung der Impfung.			1866	21,409,684	2,977	14
1842	16,130,326	2,715	17	1867	21,677,525	2,467	11
1843	16,332,228	?	?		Neues Impfgesetz.		
1844	16,535,174	?	?	1868	21,948,713	1,994	9
1845	16,739,136	?	?	1869	22,223,299	1,482	7
1846	16,944,092	?	?	1870	22,501,316	2,547	12
1847	17,150,018	4,227	25	1871	22,788,594	23,062	101
1848	17,356,882	6,903	40		Neueste Verschärfung des Impfzwanges.		
1849	17,564,656	4,644	26				
1850	17,773,324	4,665	26	1872	23,096,495	19,022	82
1851	17,982,849	6,997	39	1873	23,408,556	2,303	10
1852	18,193,206	7,320	40	1874	23,724,834	2,084	9
1853	18,404,368	3,151	17	1875	24,045,385	849	4
	Impfzwang.			1876	24,370,267	2,408	10
				1877	24,699,539	4,278	17
1854	18,616,310	2,808	15	1878	25,033,259	1,856	7
1855	18,829,000	2,525	13	1879	25,371,489	536	2
1856	19,042,412	2,277	12	1880	25,714,288	648	3
1857	19,256,516	3,936	20	1881	26,061,736	3,098	12
1858	19,471,291	6,460	33	1882	26,413,861	1,317	5
1859	19,686,701	3,798	19	1883	26,770,744	957	4
1860	19,902,713	2,713	14	1884	27,132,449	2,234	8
1861	20,119,314	1,290	6	1885	27,499,041	2,827	10

* Vgl. XLVIII Annual Report of the Registrar General of births, deaths and marriages in England. (1885.) SS. XXX, LX.

POCKENSTERBLICHKEIT IN OESTERREICH (CISLEITHANIEN), 1862-1884.

Jahr.	Mittlere Volkszahl.	Pocken-todes-fälle.	Auf je 1,000,000 Lebende.	Jahr.	Mittlere Volkszahl.	Pocken-todes-fälle.	Auf je 1,000,000 Lebende.
1862	20,326,993	6,751	332.1	1874	21,099,676	36,442	1,727.1
1863		7,209	354.6	1875		12,151	575.9
1864		7,545	371.2	1876		8,564	405.9
1865		4,767	234.6	1877		11,706	554.8
1866		7,484	368.2	1878		13,313	631.0
1867		9,840	484.1	1879		11,273	534.3
1868		7,519	369.9	1880		14,232	674.5
1869		7,612	374.5				
				1881	22,341,752	18,019	806.5
1870	21,099,676	6,177	292.7	1882		21,154	946.8
1871		8,074	382.7	1883		13,310	595.8
1872		39,368	1.865.8	1884		11,521	515.7
1873		65,274	3,093.6				

Nach den betreffenden Jahrgängen des "Jahrbuches", bez. der "Statistik des Oesterreichischen Sanitätswesens".

POCKENSTERBLICHKEIT IN SCHOTTLAND, 1855-1885.*

Jahr.	Bevölkerung.	Pockentodesfälle.		Jahr.	Bevölkerung.	Pockentodesfälle.	
		Absolute Zahlen.	Von je 100,000 Lebenden			Absolute Zahlen.	Von je 100,000 Lebenden.
1855	2,978,065	1,309	43.95	1869	3,305,885	64	1.93
1856	2,995,771	1,306	43.59	1870	3,336,707	114	3.42
1857	3,012,310	845	28.05	1871	3,368,921	1,442	42.80
1858	3,027,665	332	10.97	1872	3,404,798	2,446	71.84
1859	3,041,812	682	22.42	1873	3,441,056	1,126	32.72
1860	3,054,738	1,495	48.94	1874	3,477,704	1,246	35.83
1861	3,069,404	766	24.96	1875	3,514,744	76	2.16
1862	3,097,009	426	13.75	1876	3,552,183	39	1.10
1863	3,126,879	1,646	52.64	1877	3,590,022	38	1.06
1864	3,156,021	1,741	55.16	1878	3,628,268	4	0.11
	Einführung des Impfzwanges.			1879	3,665,443	8	0.22
				1880	3,705,995	10	0.27
1865	3,185,437	383	12.02	1881	3,745,485	19	0.51
1866	3,215,129	200	6.22	1882	3,735,400	3	0.08
1867	3,245,098	100	3.08	1883	3,825,744	11	0.29
1868	3,275,350	15	0.46	1884	3,866,521	14	0.36

* XXXI Annual Report of the Registrar General on the births, deaths and marriages registered in Scotland during the year 1885; und XXI Annual Report on Vaccination, SS. 7 und 26.

POCKENSTERBLICHKEIT IN DEN NIEDERLANDEN, 1866-82.*

Jahr	Bevölkerung.	Pockentodesfälle.		Jahr.	Bevölkerung.	Pockentodesfälle.	
		Absolute Zahlen.	Von je 100,000 Lebenden.			Absolute Zahlen.	Von je 100,000 Lebenden.
1866	3,498,408	1,413	40.39	1874	3,796,109	130	3.42
1867	3,525,448	542	15.37	1875	3,839,428	195	5.08
1868	3,552,488	143	4.03	1876	3,882,741	113	2.91
1869	3,579,529	50	1.40	1877	3,926,061	26	0.66
1870	3,622,845	707	19.52	1878	3,969,377	11	0.28
1871	3,666,162	15,711	428.54	1879	4,012,693	8	0.20
1872	3,709,477	3,731	100.58	1880	4,056,009	79	1.95
				1881	4,099,326	76	1.85
	Einführung des indirecten Impfzwanges.			1882	4,142,642	153	3.69
1873	3,752,795	351	9.35				

* Die Mortalitätsziffern sind folgenden Quellen entnommen: 1866–69, Algemeine Statistiek von Nederland, II Leiden, 1873, S. 156. 1870, Statistische Bescheiden, Zesde Deel, tweede Stuck, s'Gravenhage, 1873, S. 307. 1871, Stat. Besch. Zewende Deel, tweede Stuck, s'Graven., 1874, S. 307. 1872, St. Besch. Achtste Deel, tweede Stuck, s'Gravenh., 1876, S. 307. Für die späteren Jahre die betreffenden Jahrgänge des "Staatkundigen Staathuishoudkundig Jaarbockje". Die Volkszahl betrug am 31. December 1859 3,309,128; am 31. December 1869 3,579,529; am 31. December 1879 4,012,693 Seelen; für die dazwischen liegenden Jahre haben wir den jährlichen Zuwachs in gleichmässiger Weise für jedes Jahr zugerechnet.

ÜBERSICHT DER POCKENTODESFÄLLE IN EINIGEN AUSSER-DEUTSCHEN STÄDTEN IN DEN JAHREN 1870-1886.

Jahr.	Triest.			Budapest.			Lemberg.			Krakau.		
	Einwohner.	Pockentodesfälle.	Auf 100,000 Lebende berechnet.	Einwohner.	Pockentodesfälle.	Auf 100,000 Lebende berechnet.	Einwohner.	Pockentodesfälle.	Auf 100,000 Lebende berechnet.	Einwohner.	Pockentodesfälle.	Auf 100,000 Lebende berechnet.
1873	97,262	53	54.5	...	...	...	95,525	534	559.0	55,747	34	61.0
1874	104,009	77	74.0	296,272	945	319.0	97,629	118	120.9	57,225	55	96.1
1875	110,756	36	32.5	299,811	426	142.1	99,733	82	82.2	58,703	56	95.4
1876	117,503	11	9.4	303,352	103	34.0	101,837	224	220.0	60,181	76	126.3
1877	124,250	77	62.0	317,370	90	28.0	103,941	101	97.2	61,659	158	256.2
1878	130,997	151	115.3	333,694	242	72.5	106,045	5	4.7	63,137	26	41.2
1879	130,997	27	20.6	350,018	416	118.9	108,149	...	...	64,615	99	153.2
1880	137,744	130	94.4	366,342	327	89.3	110,250	1	0.9	66,095	142	214.8
1881	144,491	24	16.6	375,437	442	117.7	112,354	75	66.8	67,573	92	136.1
1882	144,491	1	0.7	384,776	393	102.1	112,354	325	289.3	68,000	67	98.5
1883	146,357	4	2.7	394,115	77	19.5	112,354	106	94.3	69,000	43	62.3
1884[2]	148 245[1]	393[1]	265.1	403,454	67	16.6	...	...	...	70,000[1]	216[1]	308.6
1885[2]	150,157[1]	241[1]	160.5	412,893	179	43.4	...	...	...	70,000[1]	7[1]	10.0
1886[2]	152,093[1]	1[1]	0.7	422,557	1,558	368.7	...	...	...	71,850[1]	8[1]	11.1
Durchschnitt	...	...	64.9	...	...	113.2	...	...	139.6	...	...	119.3

Jahr.	Troppau.			Brünn.			Warschau.			Brüssel.		
	Einwohner.	Pockentodesfälle.	Auf 100,000 Lebende berechnet.	Einwohner.	Pockentodesfälle.	Auf 100,000 Lebende berechnet.	Einwohner.	Pockentodesfälle.	Auf 100,000 Lebende berechnet.	Einwohner.	Pockentodesfälle.	Auf 100,000 Lebende berechnet.
1870	...	...	...	...	...	...	...	...	...	176,706	224	126.8
1871	...	...	...	...	...	...	...	...	...	185,000	958	517.8
1872	...	...	...	...	...	...	...	...	...	183,000	17	9.2
1873	18,046	16	88.7	77,002	286	371.4	...	...	...	185,000	4	2.2
1874	18,406	7	38.0	77,810	65	83.5	...	...	...	185,000	6	3.2
1875	18,765	...	..	78,608	21	26.7	...	...	...	188,264	28	14.9
1876	19,124	...	...	79,416	5	6.3	...	...	...	188,264	162	86.0
1877	19,483	15	77.0	80,224	...	...	...	...	...	173,000	122	70.5
1878	19,843	59	297.3	81,032	16	19.7	308,548	1,051	340.6	173,670	1	0.6
1879	20,202	...	...	81,840	...	...	315,199	104	33.0	175,188	9	5.1
1880	20,562	...	...	82,655	99	119.8	336,703	38	11.3	177 086	5	2.8
1881	20,882	9	43.1	83,463	44	52.7	379,763	663	174.6	165,366	9	5.4
1882	20,882	55[2]	263.4	83,463	42[2]	50.3	379,763	1,374	361.8	165,350	82	49.6
1883	20,882	24[2]	114.9	83,463	...[2]	...	383,973	147	38.3	166,351	136	81.8
1884	...	...	...	...	...	...	384,732[1]	104[1]	27.0	171,293[1]	93[1]	54.3
1885	...	...	...	...	...	...	404,889[1]	189[1]	46.7	175,811[1]	4[1]	2.3
1886	...	...	...	...	...	...	431,572[1]	131[1]	30.4	174,751[1]	20[1]	11.4
Durchschnitt	...	...	83.9	...	...	66.4	...	...	118.2	...	...	61.4

Die Daten sind den Protokollen der deutschen Impfcommission von 1885 entnommen, mit Ausnahme der hier folgenden Zusätze:

1) Aus dem *Bulletin Hebdomadaire de Statistique Démographique et Médicale* (Jannsens). 2) Oesterr. Statistik des Sanitätswesens.

Ferner sind die Budapester Daten meinen eigenen Arbeiten über die Mortalität dieser Stadt entnommen.

FÜNFTES KAPITEL.

GERINGERE MORBIDITÄT DER GEIMPFTEN.

(ERSTER DIREKTER BEWEIS.)

Die Statistik verweigert auf diese Cardinalfrage jeden Aufschluss, weil sie die Impfverhältnisse der Lebenden nicht kennt. Problem der lebenden Gesammtheiten. Berechtigte Forderung der Impfgegner nach Beschaffung richtiger Verhältnisszahlen; auch durch Impffreunde anerkannt. Schätzungsweise Feststellung der Impfverhältnisse. Zweischneidigkeit solcher Schätzungen.
Flinzer's Zählung des Impfstandes. Wichtigkeit ihrer Ergebnisse.

Nach den bisherigen, bloss indirekten, Beweisen kommen wir nun zu den eigentlich ausschlaggebenden, direkten.

Wir begegnen hier zuerst jener These der Impfvertheidigung, dass die Geimpften seltener von den Blattern befallen werden als die Ungeimpften. Statistische Daten, welche die Richtigkeit dieses Satzes bewiesen, wären gewiss von einschneidenster Wichtigkeit für die Frage des Impfschutzes. *Leider verweigert aber die Statistik über diesen wichtigen Punkt jede Auskunft.* Gleich Anderen ist auch mir nichts darüber bekannt, dass es irgendwo Aufzeichnungen darüber gäbe, in welchem Maasse, bei grossen Bevölkerungen, Geimpfte und Nichtgeimpfte von den Blattern ergriffen wurden, und zwar aus der einfachen Ursache, weil die Anzahl der Geimpften oder Ungeimpften noch in keinem Lande erhoben werden konnte, man also auch nicht wissen kann, ob hier oder dort verhältnissmässig mehr erkranken. Wir stossen hier auf eine Schwierigkeit der statistischen Beobachtung, der wir im Laufe dieser Untersuchung noch wiederholt begegnen werden, die auch vielen anderen statistischen Untersuchungen, namentlich auf dem Gebiete der Mortalitäts Statistik, einen unübersteiglich scheinenden Damm entgegensetzt. Wir wollen diese Schwierigkeit kurz als das Problem der lebenden Gesammtheiten bezeichnen, und besteht dasselbe in Folgendem:

PROBLEM DER LEBENDEN GESAMMTHEITEN.

Wenn es sich darum handelt, die Veränderungen zu erkennen, die irgend eine Ursache in irgend einer Gesammtheit hervorbringt, so ist es nothwendig, den ursprünglichen Zustand der Gesammtheit, sowie die Art der im Laufe der Zeit eingetretenen Veränderungen beobachten zu können. Kennt man bloss die letzteren, so lässt sich kein Schluss auf die Kraft, ja nicht einmal auf die Existenz, der wirkenden Ursache ziehen. Dennoch wird dieser Schluss sehr oft versucht und sind selbst Autoritäten von der Begehung dieses Fehlers nicht frei geblieben. Es handle sich z. B. um die Sterblichkeit der einzelnen Altersklassen, das ist um die Frage: wie viel sterben im Laufe eines Jahres unter je hundert 0—1-, 1—2-, 2—3-Jährigen etc. Hier ist die Stammgesammtheit, die Anzahl der in einer Altersklasse *Lebenden* und wollen wir dadurch, dass wir die Anzahl der aus derselben Verstorbenen zu der Stammgesammtheit in Verhältniss setzen, die Grösse der Mortalität dieser Altersklasse erkennen. Nun hat man aber Mortalitäts-Tabellen construirt, als deren Grundlage bloss die Anzahl der aus einer Altersklasse *Verstorbenen* diente, ohne dass man die Anzahl der in derselben Lebenden gekannt hätte. Derart ist z. B. die erste der versuchten Mortalitäts-Tabellen, jene des berühmten Halley, beschaffen und ist dies auch die Ursache, weshalb man die Halley'sche Methode als unrichtig erkennen und verlassen musste.

Gleicherweise steht es, oder scheint es zum Mindesten, auch um die Impfstatistik zu stehen: wir kennen auch hier nur die Anzahl der erkrankten Geimpften, wissen

aber nicht, wie gross die Gesammtheit jener gesunden Geimpften sei, aus denen diese Anzahl hervorging.

Das Problem der lebenden Gesammtheiten wurde zwar zuerst von den Impfgegnern in den Streit eingeführt, aber auch Freunde der Impfung müssen zugeben, dass sie, bei Aufwerfen jener Cardinalfrage: Erkranken von je hundert Geimpften weniger an Blattern als von hundert Ungeimpften? den Boden unter den Füssen schwanken fühlen. — Um zuerst mit den Impfgegnern zu beginnen, hören wir z. B., was Dr. Kraus, Redacteur der *Wiener medicinischen allgemeinen Zeitung*, über die Behandlung der Impffrage auf dem dritten internationalen medicinischen Congresse (Wien, 1873) bemerkt: "Wenn es möglich wäre," sagt Dr. Kraus, "20, 30 Jahre hindurch einige Tausend Geimpfte und Ungeimpfte nicht aus den Augen zu lassen und zu sehen, wie viel von den Geimpften an den Blattern erkrankten, wie viel von den Ungeimpften, dann könnte die Entscheidung in dieser Frage getroffen werden. Das ist aber bis jetzt nicht geschehen. Die Statistik *aller Länder ist in dieser Frage dubios. In allen Ländern des Erdballes stellen sich einer genauen Statistik dieselben Schwierigkeiten entgegen, und darum muss auch die Frage insolange eine offene bleiben, bis nicht exactere Statistiken vorliegen werden.*" Desgleichen sagt Lorinser[1]: "Wollte man einen genauen Nachweis liefern, ob verhältnissmässig mehr Ungeimpfte oder Geimpfte an Blattern erkranken, so müsste man nicht nur die Zahl der erkrankten Geimpften und Ungeimpften genau kennen, sondern man müsste auch wissen, wieviel geimpfte und ungeimpfte Individuen überhaupt innerhalb der Grenzen eines gewissen Territorums leben. Nun beruhen aber die Angaben über die Zahl der lebenden geimpften und ungeimpften Individuen immer nur auf Schätzungen; die Zahl der an Blattern Erkrankten kennt man in der Regel sehr ungenau oder gar nicht und bei sehr vielen Erkrankten lässt es sich nicht mit Sicherheit nachweisen, ob sie geimpft oder nicht geimpft worden seien." Auch in den Debatten der deutschen Impf-Commission vom Jahre 1884 wurde Seitens der Impfgegner auf dieses Problem der lebenden Gesammtheiten hingewiesen. Desgleichen betont auch Vogt die Nothwendigkeit, neben der Anzahl der erkrankten Geimpften oder Ungeimpften, auch die Gesammtzahl aller Geimpften und Ungeimpften (und zwar nach Altersklassen) kennen zu müssen, um rechnen zu können. "Ein solches Document existirt aber," fügt Vogt hinzu, "in unserer ganzen Literatur noch nicht."[2]

Die Berechtigung jener Forderung, dass man vor Allem den Impfzustand der Lebenden kennen müsse, wird übrigens auch von unbefangen denkenden Impffreunden anerkannt, wenn man auch die Schwierigkeit nicht verkennt, welche die Beschaffung dieser Daten bietet.[3] Man höre z. B., wie sich Bohn hierüber ausspricht: "Die Impf-

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[1] "Bedenken gegen die Impfung." *Wiener medicinische Wochenschrift.* 1873. S. 303.

[2] Vogt. "Für und wider die Kuhpockenimpfung." S. 55. Vogt verlangt hier überdies noch, dass die Statistik auch die seit Vornahme der Vaccination verstrichene Zeit in Betracht ziehen sollte. Dieses Argument wäre eigentlich im Interesse der Impfvertheidigung, genauer gesagt in jenem der Revaccinations-Vertheidigung, zu stellen. In der That wird von dieser Seite auch die Forderung aufgestellt, dass unter den Erwachsenen nur die Revaccinirten als Geschützte zu betrachten wären. Die Einführung solcher Unterscheidungen — wie diese von Lorinser formulirt u. z. B. von Keller auch durchgeführt wurden — würde gewiss zu präciseren Resultaten führen, als jene, die in der Vaccinationstatistik gang und gäbe sind. Sollte es aber möglich sein, eine geringere Morbidität der Geimpften insgesammt nachzuweisen, *trotzdem* unter diesen angeblich Geschützten sich zahlreiche Nichtvaccinirte befinden — worüber im zweiten Theile mehr — so wäre hierdurch die Frage des Impfschutzes schon genügend erwiesen und die Impfvertheidigung könnte sich dieses, nur zu *ihren* Gunsten dienenden, die Technik der statistischen Beobachtung aber sehr erschwerenden Verbesserungsvorschlages eventuell ganz entschlagen.

[3] Man vergleiche z. B. den Bericht des nieder-österreichischen Sanitätsrathes, bez. des Referenten, Prof. Auspitz. (Siehe Wittelshöfer's Wochenschrift. 1873. S. 32.) Wie schwierig das

gegner fordern den tadellosen Zahlenbeweis für die bevorzugte Lage, in welcher sich die Vaccinirten befinden sollen, wohlwissend, wie misslich ein solcher gerade hier durchzuführen sei. Denn für eine derartige Statistik muss die Kenntniss aller Geimpften und Ungeimpften in einer gegebenen Bevölkerung verlangt werden, und nicht nur die Kenntniss aller Pocken-Erkrankungen und Todesfälle — *zwei für grössere Menschencomplexe kaum erschwingbare Forderungen.*"[1]

Wie wir sehen werden, scheitert nicht nur diese, sondern noch eine Reihe der Fundamentalfragen der Impfstatistik an diesem Problem, das eine geradezu unübersteigbare Schwierigkeit zu enthalten scheint. Ich werde zum Schlusse meines Referates nachweisen, ob es nicht dennoch möglich sei, dieses Problem zu lösen, d. h. auf die Frage, ob zwischen Pocken-Impfung und Pocken-Erkrankung ein Causalnexus bestehe, sowie auf einige weitere Fragen der Vaccinationsstatistik durch die von mir bereits seit mehreren Jahren in Anwendung gebrachte Methode der "Intensitätsmessung" auch ohne Kenntniss der lebenden Zahl ganz correcte Antwort geben zu können. So aber, wie die Dinge in der Statistik bis jetzt stehen, muss man zugeben, dass das Argument der geringeren Morbidität der Geimpften ein für grössere Bevölkerungsmassen statistisch unbeweisbares, die Klage also, dass uns die Statistik über diesen Cardinal-Punkt keine Unterlagen biete, als eine berechtigte zu betrachten ist.[2]

Material für diese Frage zu beschaffen sei, mag schon daraus hervorgehen, dass sich einem so anerkannten Fachmanne und einem so rührigen Vertheidiger der Impfung wie Prof. Auspitz, im Laufe eines Jahrhunderts nur zwei statistische Belege geboten, nämlich die Württemberger Epidemie von 1833 und die Marseiller von 1838. Mit beiden Beweismitteln ist es aber nicht am Besten bestellt. Was nämlich die aus Württemberg citirten Angaben betrifft, ist aus denselben nicht die eigentliche Intensität, das ist die Frequenz der Erkrankungen auf je 100 Einwohner (und zwar nicht einmal im Allgemeinen, geschweige denn nach Altersklassen), sondern bloss so viel zu entnehmen, wie gross die Anzahl der erkrankten Geimpften sei. Dass die Anzahl derselben im Laufe von fünf Jahren nur 945 beträgt, ist gewiss sehr bemerkenswerth; vom Standpunkte der methodischen Beweisführung in der Morbiditätsfrage ist aber mit dieser Angabe gar nichts geleistet, wenn man nicht auch die Anzahl der lebenden Geimpften kennt. Die Angaben über die Impfverhältnisse der Marseiller Bevölkerung aber beruhen geradezu auf unverlässlichen Schätzungen.

Das lebhafte Interesse, das die Vaccinationsstatistik für die Beschaffung solcher Daten hegt, verleitete schon manchen Fachmann, den Weg der Schätzungen zu betreten; so hat selbst Hebra im englischen Blaubuche sich hierauf eingelassen. Wie leicht man auf diesem schlüpfrigen Wege ausgleiten könne, zeigt eben das obenerwähnte Beispiel der Marseiller Epidemie. Wie anderwärts, so ist natürlich auch in Marseille die Anzahl der geimpften und ungeimpften Lebenden nicht bekannt. Die Angaben von Sue und Savart, wonach unter den unterdreissigjährigen Einwohnern Marseille's 2000 Geblatterte, 30,000 Geimpfte und 8000 Ungeimpfte gewesen wären, sind rein willkürliche Annahmen und tragen den Stempel dieser Willkürlichkeit schon in der Rundheit der Ziffern auffällig zur Schau. Wohin man aber mit willkürlichen Schätzungen gelangt, zeigt Vogt, der ("Kuhpockenimpfung", S. 85) für diese selbige Marseiller Epidemie auf Grundlagen von Schätzungen zu ganz entgegengesetzten, das ist impffeindlichen Resultaten gelangt.

[1] Bohn. Handbuch. S. 296.

[2] Böckh, Direktor des communal-statistischen Bureaus in Berlin, hat den Versuch gemacht, den Impfzustand der Berliner Bevölkerung gelegentlich der Volkszählung conscribiren zu lassen, doch gelangte sein Vorhaben nicht zur Ausführung.

Einen besseren Erfolg kann *Flinzer*, allerdings auf einem unvergleichlich kleinerem Gebiete, nämlich jenem der Stadt Chemnitz (mit im Ganzen 64,255 Einwohnern), aufweisen. Wenn man von einer ähnlichen Aufnahme in dem kleinen Fleckchen Waldheim absieht, bilden Flinzer's Daten über die Blattern-Epidemieen von Chemnitz in den Jahren 1870 und 1871 (erstes Heft der Mittheilungen des statistischen Bureaus in Chemnitz, 1873) jenen einzigen Beitrag der statisti-

schen Litératur, wo sich ein Morbiditäts-Coëfficient auf Grundlage der Seelenzahlen von Geimpften und Ungeimpften berechnen lässt. Deshalb, sowie in Berücksichtigung der grossen Umsicht, mit welcher die Flinzer'sche Arbeit im Allgemeinen durchgeführt ist, wird diese, trotz des bescheidenen Umfanges, auf welchen sich dieselbe erstreckt, in der Vaccinations-Literatur mit Recht hochgestellt. Die Resultate Flinzer's ergeben nun, dass an Blattern erkrankten von 54,891 Geimpften und Revaccinirten 802, von 5,712 Ungeimpften 2,546, von 4,652 Geblatterten 2, während von den unter den Geimpften schon mitgezählten 1,928 Revaccinirten 16 erkrankten. Es erkrankten mithin von 1000 Geimpften 15.13, von 1000 Revaccinirten 8.3, von 1000 Geblatterten 0.43, *hingegen von Ungeimpften* **546**!

Diese Ergebnisse bieten gewiss eine höchst interessante Zeugenschaft zu Gunsten des Impfschutzes; aber selbst diesen gegenüber gelten die bereits erwähnten und im Nachfolgenden noch näher zu behandelnden logischen Einwendungen der Impfgegner. Es ist nämlich möglich, dass die grössere Sterblichkeit der Ungeimpften nur eine natürliche Folge jenes Umstandes sei, dass sich unter diesen alle Kinder, ferner aber auch alle schwächlichen und kranken Personen befinden. Auf den letzten Einwand kann im Rahmen der bisher angewendeten statistischen Methoden nicht geantwortet werden. Leider bietet die sonst so vortreffliche Statistik Flinzer's auch keinen Anhaltspunkt dafür, dem auf die verschiedenartige Besetzung der Altersklassen basirten Einwand näher zu treten. Flinzer's Arbeit schreitet nämlich nicht nach Altersklassen fort, sondern unterscheidet bloss zwei grosse Altersgruppen: unter und über 14 Jahre, macht es also unmöglich, den Einfluss des Alters von jenem der Impfung zu trennen. Die diesbezügliche Bemängelung Vogt's Flinzer gegenüber muss daher als berechtigt angesehen werden. Hingegen bieten uns die Angaben Flinzer's über einen anderen wichtigen Punkt Aufschluss, der unseres Wissens bisher nicht genügend berücksichtigt wurde. Wir sehen nämlich, dass trotzdem die Anzahl der Ungeimpften in der Gesammtbevölkerung eine verschwindend geringe ist, dennoch dieser Bruchtheil es ist, welcher die überaus grosse Mehrzahl der Blatternfälle verursacht!

SECHSTES KAPITEL.

GERINGERE MORTALITÄT UND LETHALITÄT DER GEIMPFTEN.

(ZWEITER, BEZIEHUNGSWEISE DRITTER DIREKTER BEWEIS.)

Unterschied zwischen Mortalitäts- und Lethalitäts-Coëfficienten. Mortalitäts-Coëfficient lässt sich gleich dem Morbiditäts-Coëfficienten nicht beschaffen; desgleichen Lethalitäts-Coëfficient für ganze Bevölkerungen nicht; Unzulänglichkeit der auf Grund der polizeilichen Krankenmeldungen berechneten Lethalitäts-Coëfficienten. Beispiele aus Berlin und Budapest. Bemerkungen über die individuale Beobachtungsmethode.

Lethalitäts-Coëfficienten aus Spitälern. Dieselben lauten zumeist zu Gunsten der Impfung. Sporadische Angaben, wonach die Lethalität Geimpfter und Ungeimpfter keine Verschiedenheit aufwiese. Angaben, wonach die Lethalität Geimpfter grösser wäre (Reitz, Hermann). Empfehlenswerthe Vorsicht solchen Behauptungen gegenüber. Widerlegung der Angaben von Reitz und Hermann. Allgemeine Bedenken gegen die Brauchbarkeit von Hospitalsbeobachtungen; es begegnen sich hierin sowohl Impfvertheidiger (Oesterlen, Marson, Farr, Bohn) wie Impfgegner (Lorinser): Hospitals-Coëfficienten also allseitig abgelehnt.

Mortalitäts- und Lethalitäts-Statistik beim Heere. Grosse Abnahme der Sterblichkeit in der preussischen, beziehungsweise deutschen Armee, seit Einführung der Revaccination. Grössere Pockensterblichkeit im Kreise der schlechter geimpften Civilbevölkerung. Einwände der Impfgegner gegen die Vergleichung von Militär und Civil. Richtigere Vergleichung von Heer zu Heer. Günstigere Pocken-Morbidität, -Mortalität und -Lethalität bei besser geimpften Armeen. Vergleichung der Armeen von Deutschland, Oesterreich-Ungarn, Frankreich.

Sterben von Geimpften wirklich weniger an Blattern, als von Nichtgeimpften? Das ist die letzte Hauptfrage, und die bejahende Antwort hierauf würde den letzten direkten Beweis der Impfvertheidigung bilden. Die Antwort auf diese Frage wäre nach zwei Richtungen zu formuliren:

a) Wie viel von hundert Lebenden an Blattern sterben (Mortalitäts-Coëfficient);

b) Wie viel von hundert Blattern-Erkrankten (Lethalitäts-Coëfficient).

Beide Antworten würden (unter Berücksichtigung anderer mitwirkenden Faktoren, worüber später,) verlässlichen Aufschluss über die Richtigkeit oder Unrichtigkeit der Theorie vom Impfschutze bieten.

Bei der ersten Frage begegnen wir nun wieder dem Gespenste der Impfstatistik, nämlich dem Problem der lebenden Gesammtheiten: man weiss eben nicht, wie viele Geimpfte und Ungeimpfte unter den Lebenden existiren! Die Impfgegner beharren aber auf ihrem Schein, und die Impffreunde müssen stets resignirt die Unmöglichkeit einbekennen, diese logisch vollkommen berechtigte Forderung zu erfüllen.[1]

Hinsichtlich des Lethalitäts-Coëfficienten ganzer Bevölkerungen begegnen wir derselben Schwierigkeit: auch zur Berechnung dieses Coëfficienten bedürfte man der Anzahl sämmtlicher erkrankten Vaccinirten und Unvaccinirten, und zwar nach Altersklassen. Wo die Anzeigepflicht für epidemische Krankheiten eingeführt ist, erhält man die Anzahl der Erkrankten aus den polizeilichen Anzeigen. Ist nun hier bei Blatternerkrankungen auch angegeben, ob der Betreffende geimpft war oder nicht,

[1] So sagt Bohn (Handbuch der Vaccination, Seite 297): "Es müssen jene rigorosen Anforderungen der Impfgegner geachtet werden, und bleibt so lange eine Lücke in unseren Beweisen, bis ihnen eine zwingende Statistik entgegentritt." Dieser Beweis ist aber für grössere Bevölkerungsmassen begreiflicherweise noch nirgends geführt worden. Auch Bohn kann nur die bereits genannten zwei, hinsichtlich ihrer Volksmenge denn doch nicht genug grossen Städte, nämlich Chemnitz und Waldheim, anführen, wo die Impfverhältnisse der Lebenden bekannt waren.

(was übrigens meines Wissens nur an den allerwenigsten Orten der Fall sein dürfte), so sollte man glauben, in solchen Fällen die Gesammtheit, nämlich jene der Erkrankten, gewonnen zu haben, welcher man dann (auf Grund der Todtenscheine) jene der Gestorbenen entgegensetzen könnte. Auf diese Weise schiene die Lethalitätsfrage für ganze Bevölkerungen beantwortet.

Leider enthalten aber die Angaben über die erstere Gesammtheit eine unvermeidliche, in der Natur der Sache gelegene Fehlerquelle, und zwar in Folge der *Unvollständigkeit* der Anzeigen. Man hat es nämlich noch nirgends durchgesetzt, dass sämmtliche Erkrankungsfälle zur Anzeige gelangten, und wird sich diese, für eine genaue Statistik unerlässliche, Vollständigkeit auch nie erreichen lassen.[1] Selbst bei Fällen, wo der Arzt bei Zeiten gerufen wurde, können, wenn die Krankheit sich noch im Prodromal-Stadium befindet, oft nur Fiebererscheinungen constatirt werden; bis aber der Arzt den Kranken ein zweites Mal aufsucht, findet er oft nur eine Leiche, hat also mehr keinen Erkrankungsfall zu melden. Dies gilt selbst für städtische Verhältnisse, wo der Arzt sofort zur Hand ist; wie erst auf dem flachen Lande, oder in Gebirgsdörfern, wo Verkehrshindernisse den zweiten Besuch oft ganz unmöglich machen? Wie erst bei den niederen Klassen, die gar keinen Arzt rufen? Die Gesammtheit der Verstorbenen wird — weil leicht und richtig controlirbar — unter solchen Umständen stets vollständiger, der Lethalitäts-Coëfficient also stets ein zu grosser sein. Man könnte sich hierbei noch damit beruhigen, dass der Coëfficient bei Geimpften um ebensoviel anwüchse, wie bei Ungeimpften; da aber die Ungeimpften zumeist den unteren Ständen angehören, ist eine gleichmässige Auslassung von Erkrankungsfällen kaum anzunehmen. Man hat deshalb mit der auf polizeiliche Erkrankungsmeldungen basirten Lethalitäts-Statistik, so z. B. selbst in Berlin mit der Statistik des geh. Medicinalrathes Müller, sehr üble Erfahrungen gemacht, und ist es daher begründet, wenn man diese polizeiliche Beobachtungsquelle eine trübe nennt (Bohn).

Ich will folgenden ziemlich drastischen Beweis aus der jüngsten Cholera-Epidemie in Budapest anführen, um zu erhärten, wie irreführend solche Lethalitäts-Berechnungen sein können. Der erste Cholera-Todesfall erfolgte daselbst am 7. September 1886; am 11. starben vier weitere Personen. Es mussten also bereits zahlreiche Erkrankungsfälle vorgefallen sein: die officielle Liste der Erkrankungen weist aber noch immer *keinen einzigen* Fall auf. Am 12. starben abermals vier Personen und erst an diesem Tage langen die ersten Erkrankungs-Anzeigen, aber im Ganzen nur zwei Fälle, ein. Es folgen darauf am 13. zehn Erkrankungsfälle und ein Todesfall. Insgesammt waren also in den ersten acht Tagen 12 Erkrankungs- und 10 Todes-Fälle angemeldet. Die Statistik würde demnach eine Lethalität von mehr als 80% aufweisen, während bei vollkommener Anmeldung der Erkrankungsfälle sich kaum ein halb so grosses Verhältniss ergeben dürfte.

Ich will noch auf einen weiteren, meines Wissens bisher noch nicht berücksichtigten Mangel solcher Berechnungen aufmerksam machen. Selbst für den unwahrscheinlichen Fall, dass *alle* Erkrankungsfälle vollständig angemeldet würden, wäre die Relationirung beider Gesammtheiten noch immer anfechtbar; es fehlt eben alle Gewähr dafür, ob die beiden Theile dieser Relation, ob also Zähler und Nenner des Bruches homogen sind. Es seien z. B. sechs Erkrankungsfälle angemeldet worden. Von diesen seien A. B. im Orte selbst, C. D. im Nachbarorte gestorben, während E. F. geheilt wurden. Die Behörde des ersten Ortes wird daher in diesem Falle nur eine Lethalität von $\frac{2}{6}$ konstatiren können, während dieselbe faktisch $\frac{4}{6}$ betrug.[2] Wären dann noch

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[1] Selbst in Berlin sind die polizeilichen Krankenmeldungen nach Guttstadt unvollständig. Siehe Zeitschrift des preussischen statistischen Bureau. 1873. Seite 129.

[2] Diese Schwierigkeit ist eine allgemeine, auf alle Systeme von Mortalitätstabellen sich erstreckende, und kann bloss dadurch behoben werden, wenn man statt mit blossen Zahlen mit den Trägern derselben,

drei zugewanderte Kranke gestorben, würde — und zwar wieder ganz unrichtig — eine Sterblichkeit von $\frac{3}{2}$ berechnet werden; wären fünf zugewanderte Kranke gestorben, so würde eine sich bloss auf die Registrirung aller vorgefallenen Fälle beschränkende Statistik behaupten wollen, dass die Sterblickkeit $\frac{7}{6}$ betrug, d. h. von sechs Erkrankten *sieben* starben! Man sieht, wie die Individuen des Zählers gar nicht aus der im Nenner enthaltenen Gesammtheit hervorgegangen sind, dass also Zähler und Nenner gar nicht homogen sind, der ganze Sterblichkeitsbruch also ein logisches Unding ist. (Es mag hierbei nicht überflüssig sein, zu bemerken, dass die Beschlüsse des statistischen Congresses ausdrücklich bestimmen, dass der Sterblichkeits-Coëfficient auf Grund der faktischen Mortalität zu berechnen sei, demnach die Ausschliessung der Ortsfremden, beziehungsweise die Zuzählung der Weggezogenen nicht gebilligt wird.) Die geringe Ortsbewegung von Blatternkranken mag in der Mehrzahl der Fälle das praktische Gewicht dieses Bedenkens stark reduciren, obwohl die Grossstädte, welche grosse Spitäler und hervorragende Aerzte besitzen, stets darauf rechnen können, unter ihren Todten eine grosse Anzahl zugewanderter Kranken zu finden. Es schien mir aber, dass bei einer systematischen Darstellung der Schwierigkeiten, mit denen die Impfstatistik zu kämpfen hat, auch dieser Umstand nicht unerwähnt bleiben sollte.

LETHALITÄTS-COËFFICIENTEN AUS SPITÄLERN.

Wenn sich solcher Art Lethalitätscoëfficienten für ganze Bevölkerungen nicht gewinnen lassen, so bieten doch die Aufzeichnungen der Spitäler sehr werthvolle Aufschlüsse über das Sterblichkeitsverhältniss jener Klasse geimpfter und ungeimpfter Erkrankten, welche die öffentlichen Krankenhäuser aufsuchen.

An Stelle der Relationirung von blossen Ziffern kann man hier zu Verhältnisszahlen gelangen, die aus der direkten Beobachtung der *Individuen* hervorgehen, in dieser Beziehung also den Ansprüchen "individualer" Mortalitäts-Beobachtung genügen oder doch genügen könnten[1] und derart absolut richtige Auskunft darüber zu bieten vermöchten, wie viel Procent von geimpften Erkrankten genesen oder sterben, und wie viel von ungeimpften? Die Impfvertheidigung findet nun in dem Factum, dass solche Spitalscoëfficienten fast ausnahmslos zu Gunsten der Impfung sprechen, eine nicht geringe Stütze, und sehen wir auch, dass ein grosser Theil der zu Gunsten der Impfung aufgeführten statistischen Daten eben aus solchen Spitalscoëfficienten besteht.

Unter solchen Umständen verliert jene Behauptung der Impfgegner, dass die Blattern-Lethalität in diesem Jahrhundert trotz der Impfung nicht geringer geworden sei als sie im vorigen gewesen, alle Beweiskraft. Selbst wenn dem so wäre, wie Nittinger behauptet, dass nämlich im vorigen Jahrhundert nur 7—8% der an Blattern Erkrankten gestorben sein soll, während gegenwärtig die allgemeine Lethalität (d. i. bei Geimpften und Ungeimpften zusammen) über 10% betrüge, ist hiermit gar nichts

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also mit den Individuen selbst, operirt, d. h. jene Methode befolgt; die ich als individuale in Vorschlag gebracht. (Siehe Körösi, "Welche Unterlagen hat die Statistik zu beschaffen, um richtige Mortalitätstabellen zu gewinnen." Berlin, 1875. Ferner Prof. Dienger, "Berechnung von Sterbetafeln," "Rundschau der Versicherungen," 1874, und "Benützung des Materials der Versicherungsgesellschaften zur Herstellung von individualen Sterbetafeln." Ebendaselbst, 1873. Desgleichen Prof. Favero's Bemerkungen zu dieser Methode, in den Annali della Statistica, Serie III, Vol. 5, wo auch die Uebersetzung der Abhandlungen Dienger's anzutreffen ist.) Die Forderung individualer Beobachtung ist, wie erwähnt, auch auf andere Gebiete der statistischen Beobachtung anwendbar, so z. B. bei Berechnung der Heilungsprocente in Spitälern, und i. A. überall dort, wo die Möglichkeit einer Vertauschung der Elemente in der Gesammtheit des Standes und der Gesammtheit der Bewegung angenommen werden kann. So hat z. B. Sperk in St. Petersburg auch zur Gewinnung seiner sehr lehrreichen Beobachtungen über Luetik inskribirter Mädchen die Individualmethode angewendet. (Siehe Archiv für Dermatologie und Syphilis, 1886.)

[1] Dass dies auch hier nicht der Fall ist und wie besserungsbedürftig die Methoden der Hospitalsberechnungen seien, hierüber vergleiche meine Bemerkungen über die Lethalitätsberechnungen des Wiedener Krankenhauses in Wien (enthalten in meinem Vortrage in dem ungarischen hygienischen Verein, mitgetheilt in der *Deutschen Vierteljahresschrift für öffentliche Gesundheitspflege*; auch als Separatabdruck bei Vieweg erschienen.

gegen die Impfung bewiesen, wenn man Hand in Hand hiermit nachweisen kann, dass jetzt von ungeimpften Kranken doch drei, vier, fünf Mal so viel und noch mehr sterben als von Geimpften. Wie schwach es übrigens um die Mortalitätsstatistik der verflossenen Jahrhunderte bestellt ist, braucht hier nicht von Neuem bewiesen zu werden.

Wir dürfen aber jene Thatsachen und Argumente, durch welche die Beweiskraft solcher Spitalscoëfficienten geschwächt werden könnte, nicht mit Stillschweigen übergehen. Wir müssen also erwähnen, dass man gegen die erwähnten Thatsachen andere ähnliche anführt, die beweisen sollen, dass die Lethalität der Geimpften geradezu grösser als jene der Ungeimpften sei. Ueberdies aber werden auch noch Argumente ins Treffen geführt, wonach Spitalsbeobachtungen in Bausch und Bogen als zur Gewinnung richtiger Aufschlüsse unbrauchbar zu verwerfen seien.[1]

SPITALS-BEOBACHTUNGEN ÜBER DIE NUTZLOSIGKEIT DER IMPFUNG.

Bei Berufungen auf solche Spitals-Erfahrungen, die von einer grösseren Lethalität Geimpfter sprechen, wolle man ganz besonders vorsichtig sein. Man wende sein Augenmerk auch der Anzahl der beobachteten Fälle zu. Man wird finden, dass jene Gewährsmänner, welche sich auf antivaccinatorische Resultate der Spitalsbeobachtungen berufen, zumeist nur über sehr wenige Fälle, — oft kaum über ein Dutzend! verfügen. Es ist leider ein auch in der Impfstatistik sehr verbreitetes Uebel, aus einer zu geringen Anzahl von Fällen allgemeingiltige Folgerungen ziehen zu wollen. In der Mehrzahl der Fälle wird man überdies auch Irrthümer, sei es der Aufzeichnung, sei es des Schliessens, ja sogar des einfachen Calculs antreffen. Ich selbst habe in dieser Beziehung einige ebenso lehrreiche als überraschende Erfahrungen gemacht, die ich hiermit zum allgemeinen Besten mittheile. So citirt z. B. *Reitz*, Direktor eines Kinderspitals in St. Petersburg, die angeblich impffeindlichen Erfahrungen im Wiener allgemeinen Krankenhause, wodurch also kein Geringerer als Hebra selbst, der entschiedene Vertheidiger der Schutzpockenimpfung, als Zeuge gegen dieselbe angeführt wurde. Der Primarius der Abtheilung für Hautkrankheiten am Wiedener

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1 Im Vorübergehen wollen wir auch jener vereinzelten Beobachtungen gedenken, welche dahingehen, als ob speciell den wirklichen Blattern Geimpfte ebenso häufig unterlägen, wie Ungeimpfte. Schon im Jahre 1835 findet sich der Ausspruch eines Physikus in Mähren, Fischer in Fulnek (siehe Schmidt's Jahrbücher, I. Supplement-Band, 1836), wonach bei 187 beobachteten Fällen die Vaccination zwar vor der Erkrankung geschützt habe, dass aber hinsichtlich des Verlaufs und der Dauer der Krankheit sich überall dasselbe Bild geboten habe. Im englischen Blaubuch vom Jahre 1857 findet sich ferner die nachfolgende bemerkenswerthe Behauptung Varrentrapps, als Präsidenten des Frankfurter hygienischen Vereins: "Nach den Erfahrungen des Rochus-Spitals zeigt sich kein wesentlicher Unterschied im Verlaufe vollkommen ausgebildeter Blattern bei Geimpften und Ungeimpften; ja selbst für bereits Geblatterte ergibt sich kein günstigerer Ausschlag." Der Werksarzt der grossen Bergwerkscolonie zu Anina in Ungarn, Dr. Kieska, veröffentlicht im Jahrgang 1874 des ungarischen *Orvosi Hetilap* eine Zusammenstellung über die dortige Blatternepidemie, bei welcher er zum selben Ergebnisse gelangt. Kieska schloss sich zu dieser Zeit eben deshalb den Impfgegnern und speciell seinem Chefarzt Keller an. Vierzehn Jahre nachher habe ich an den Verfasser, der inzwischen Kreis-Arzt im Neutraer Comitate geworden, die Anfrage gerichtet, ob ihn seine weiteren Erfahrungen in dieser Hinsicht, sowie in seiner impfgegnerischen Ueberzeugung bestärkt hätten? Seine Antwort lautete im oben angegebenen Sinne, doch muss ich bemerken, dass seine mir zur Verfügung gestellten neueren statistischen Aufzeichnungen (von mir im *Orvosi Hetilap* 1887 mitgetheilt) entschieden zu Gunsten der Impfung sprechen, also mit der von ihm verfochtenen These in Widerspruch stehen. Derartige Bemerkungen sind aber doch zu sporadische und erstrecken sich auf zu wenig Fälle, als dass man denselben ein entscheidendes Votum beimessen könnte (man vergleiche auch den Schluss dieses Capitels, sowie den zweiten Theil dieses Referates).

Krankenhause, *Hermann*, beruft sich wieder auf die impffeindlichen Erfahrungen seiner eigenen Klinik. Indem ich aber beiden, in Folge der erwähnten Umstände höchst gewichtigen Berufungen bis auf die Quelle nachging, gelangte ich zu, ich darf sagen, sehr überraschenden Resultaten. Da beide Untersuchungen an einer anderen Stelle[1] ausführlicher wiedergegeben sind, begnüge ich mich hier mit der kurzen Wiedergabe derselben.

Reitz kann zwar nicht läugnen, dass auch im Wiener allgemeinen Krankenhause von Ungeimpften viel mehr an Blattern starben als von Geimpften; indem er aber die Fälle von Variola vera von jenen an Variola modificata scheidet, findet er, dass an Variola vera von Geimpften sogar mehr starben.[2] Bei Benützung der aus dem Wiener allgemeinen Krankenhause geschöpften Daten darf aber nicht vergessen werden, dass Hebra hinsichtlich der Blatternfrage Identist war, d. h. verschiedene Formen der Blattern-Erkrankungen (Schafblattern, Variolois und echte Blattern) für ein und dieselbe Krankheit hält, infolge dessen die diagnostische Unterscheidung derselben für unmöglich erklärt und insofern er dieselbe dennoch in seine Berichte aufnimmt, dies ein gestandenermaassen nur zu dem Zwecke thut, um den verschiedenartigen Verlauf, nicht aber die verschiedenartige Natur dieser Krankheit in Rubriken zu bringen. Nach dem Vorgehen der Wiener Schule wird also eine Diagnose auf Variola vera oder modificata im Vorhinein nicht gestellt, sondern diese Unterscheidung erst *nach Ablauf* der Krankheit gemacht. *Und zwar werden die schwersten Fälle, nämlich jene, die länger als vier Wochen währten*, ferner *alle lethalen Fälle*, gleichviel wie lange sie währten, als Variola vera, die leichteren als V. modificata oder Varicella bezeichnet. Was Wunder also, wenn V. vera fast lauter Todesfälle aufweist? Beachtet man aber, dass die Geimpften fast alle nur an Variolois und Schafblattern erkrankten, die Ungeimpften aber zu 60% an den wirklichen Blattern, so wird man zugeben müssen, dass auch die Ergebnisse des allgemeinen Krankenhauses zu Gunsten des Impfschutzes sprechen. Was die daselbst befolgte Weise der Classificirung betrifft, so kann es nicht unsere Aufgabe sein, diese Auffassung zu kritisiren; es ist aber klar, dass unter den obschwebenden Umständen man aus diesen Daten zwar die richtigsten statistischen Schlüsse über viele Gesichtspunkte wird ziehen können, aber über zwei absolut nicht, nämlich nicht über die Dauer und nicht über die Lethalität der einzelnen Krankheitsformen. Wo die Diagnose so erfolgt, wie im Wiener allgemeinen Krankenhause, nämlich auf Grund der Identitätslehre, kann auch die Statistik nichts Anderes thun, als sich auf denselben Standpunkt stellen, d. h. alle Blatternfälle, mögen sie wie lange immer gewährt haben, in *eine* Rubrik zusammenfassen. Thut man aber dies, so ergibt sich für die Geimpften eine—nicht wie Reitz in Folge einer unrichtigen Rechnung constatirt, doppelt, sondern—2½-fach *grössere Geschützthcit* als für die Ungeimpften.

Bezüglich des Wiedener Spitals bin ich aber zu folgendem Ergebnisse gelangt. Primarius *Hermann* beruft sich mit Emphase auf seine siebenjährige Beobachtung vom Jahre 1858 bis 1864 und führt bittere Klage darüber, dass man diese Statistik, weil sie zu impfgegnerischen Resultaten führe, todtschweige. Der Direktor des Spitals, Lorinser, dem seine eigenen Erfahrungen doch auch nicht fremd sein konnten,

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[1] Siehe meinen citirten Vortrag in der *Deutschen Vierteljahresschrift für öffentliche Gesundheitspflege* 1887.

[2] Die Daten beziehen sich auf das Jahr 1871 und lauten folgendermassen: *Geimpfte:* Variola vera—92 erkrankt, 28 verstorben; Variola modificata—217 erkrankt, keiner verstorben. *Ungeimpfte:* Variola vera—77 erkrankt, 22 verstorben; Variola modificata—16 erkrankt, keiner verstorben. *Zusammen:* Erkrankt—309 Geimpfte, 93 Ungeimpfte; Verstorben—28 Geimpfte, 22 Ungeimpfte. Demnach starben im Ganzen von Geimpften nur 9.1 pCt., von Ungeimpften aber 23.6 pCt. (Reitz berechnet irriger Weise 11.3 und 25 pCt.); desgleichen an Variola vera von Geimpften 30.4 pCt., von Ungeimpften 28.5 pCt.

ist überdies einer der Wortführer der Impfgegner: Grund genug, um mir die Statistik dieses Spitals ganz besonders interessant zu machen. Um nun mein statistisches Gewissen zu beruhigen, habe ich nicht bloss diese sieben, sondern noch alle weiteren, bis zum heutigen Tage reichenden Jahresberichte des Wiedener Krankenhauses und des mit demselben verbundenen Wiener Pockenspitals durchgenommen und kann Ihnen in Folge dessen die Versicherung geben, dass die in denselben niedergelegten reichen Erfahrungen den glänzendsten Beweis *für* die Schutzkraft der Kuhpockenimpfung abgeben. Ich will—ohne Sie mit den Details dieser längeren, an anderer Stelle ohnehin ausführlicher mitgetheilten Untersuchung zu ermüden—nur das Hauptresultat dahin zusammenfassen, dass 1) im Laufe jener fünf Jahre, auf die sich Hermann beruft, und auf Grund seiner eigensten, von ihm angerufenen Berechnungen im Wiedener Krankenhause dreimal so viel Ungeimpfte als Geimpfte starben! dass ferner 2) indem ich die Berechnung bis auf die jüngste Zeit ausdehnte, ich zu dem noch drastischeren Ergebnisse gelangte, dass daselbst 9 Procent der Geimpften, hingegen *fünfzig Procent der Ungeimpften* starben! Wie Direktor und Primarius dieses Spitals zur Behauptung gelangen konnten, dass die Erfahrungen in ihrem eigenen Spitale *gegen* den Schutz der Impfung sprächen, ist mir unergründlich. Nach den bisherigen massenhaften Zeugnissen der Spitalspraxis zu Gunsten des Impschutzes ist anzunehmen, dass alle Mittheilungen über impfgegnerische Resultate der Spitalspraxis sich in's Gegentheil verkehren dürften, wenn die Möglichkeit eines Zurückgreifens auf die Quellen geboten ist.

THEORETISCHE EINWÄNDE GEGEN DIE BRAUCHBARKEIT VON SPITALSBEOBACHTUNGEN.

Leider stellen sich aber der Spitalsstatistik andere Bedenken entgegen. Impffreunde wie Impfgegner begegnen sich nämlich in der Ansicht, dass man aus den Ergebnissen der Spitalsbeobachtungen keine verlässlichen Schlüsse ziehen dürfe. Dies behaupten nicht nur Lorinser[1] und Vogt, sondern auch Imffreunde wie Farr,[2] Marson, der Direktor des Londoner Pockenspitals und Nachfolger Gregory's,[3] Bohn u. A. mehr. Der letztgenannte sagt z. B. in seinem ausgezeichneten Handbuche der Vaccination (S. 297): "Mit Recht weisen die Impfgegner alle aus der Hospitalspraxis gezogenen Schlüsse zurück, weil das Hospital nur ein falsches Spiegelbild der variolösen Vorgänge in einer Bevölkerung entwerfen kann und weil die Impfverhältnisse seiner Kranken die unsichersten seien." Der vielcitirte Medicinalstatistiker Oesterlen steht nicht an, zu erklären, "dass fast alle Angaben der Krankheitslehre deshalb "höchst zweifelhaft, wo nicht durchaus falsch und unbrauchbar werden," weil man kein Bedenken trägt, die Erfahrungen seiner Privatpraxis, oder aus Spitälern zu Grund zu legen. "Wie könnte man hoffen, durch Zählung einzelner Fälle an jener künstlichen, rein zufälligen, bruchstückweisen Krankenbevölkerung in Spitälern, Clientelen u. dergl. je einen sicheren Aufschluss zu erhalten über die wirkliche Häufigkeit einer Krankheit, über die relative Frequenz je nach wechselnden Umständen bei den verschiedenen Altersklassen des Volkes?"[4] Ueberdies wird den Spitalsdaten auch noch entgegengehalten,

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[1] Bedenken gegen die Impfung: "Spitäler bieten ein nur sehr unvollkommenes und einseitiges Bild von Epidemieen."

[2] III. Annual report of the Registrar of Births and Deaths.

[3] Siehe Medical-chirurgical Transactions of the Royal Medical and Chirurgical Society. London. Volume 36. "It will, perhaps, be objected, *and very properly*, that the experience of an hospital, as regards the mortality, is not the best criterion by which to judge of the true value of vaccination."

[4] Oesterlen. Handbuch der medicinischen Statistik. Tübingen 1865. S. 77. Man vergleiche auch S. 39. "Uns fehlen bis jetzt noch so ziemlich alle vergleichbaren Mittelwerthe. Statt dessen beruhen die meisten Angaben der medicinischen Statistik auf mehr oder wenig

dass dieselben nur die schwersten Fälle repräsentiren, demnach keine richtigen Schlüsse gestatten.

Auch diese auf die Lethalität Geimpfter Bezug habenden Daten, nämlich jene der Spitalspraxis, werden also als unbrauchbar bezeichnet. Ich behalte mir vor, auf die nicht unwichtige Frage, wie weit Spitalsbeobachtungen speciell in der Impffrage brauchbar seien, später noch zurückzukommen. An dieser Stelle aber, wo ich nur über die bisherige Entwicklung und den gegenwärtigen Stand der Impfstatistik zu referiren habe, begnüge ich mich, zu constatiren, dass gegen die Verwendung hospitalärer Lethalitätsziffern von Freund und Feind gleichmässig Einwendungen erhoben werden.

LETHALITÄTS- UND MORBIDITÄTS-COËFFICIENTEN BEIM MILITÄR.

Es erübrigt schliesslich noch eine Gruppe der zur Zeugenschaft aufgerufenen Beobachtungen, nämlich jene, welche beim Militär gemacht wurden. Dieser Bruchtheil der Bevölkerung ist einer genauen statistischen Controle zugänglich und derselben in einigen Staaten auch faktisch unterworfen. Für die Impffrage kommen in erster Reihe jene Staaten in Betracht, welche in ihren Armeen den Revaccinationszwang eingeführt haben, und unter diesen ist es wieder Deutschland, welches in dieser Beziehung zumeist citirt wird. Auch in den gehaltvollen "Zwanzig Briefen" Kussmaul's[1] werden die von den Impfergebnissen der preussischen Armee herbeigeholten Beweise zu den schlagendsten gezählt. "Ueber die Wirksamkeit der Revaccination besitzen wir eine Menge der zuverlässigsten Erfahrungen, die allein ein Buch zu füllen im Stande wären. Ich begnüge mich, daraus die grossartigsten hervorzuheben, die Ergebnisse nämlich jener ungeheueren Versuchsreihe, welche wir den k. preussischen Militärärzten verdanken. Es gibt keinen glänzenderen Beweis für die grosse Schutzkraft der Jenner'schen Erfindung gegen die Gefahren der Blatternkrankheit, als die Ergebnisse der Vaccination und Revaccination in der k. preussischen Armee im Laufe von bald 40 Jahren." Die Revaccination wurde in der preussischen Armee im Jahre 1831 eingeführt, aber erst im Jahre 1835 faktisch angewendet. Vor Einführung der Revaccination war die preussische Armee oft von den Blattern heimgesucht worden. So starben an den Pocken in den Jahren

	1825—34	496	Mann	
aber	1835—44 nur	39	"	ferner
von	1845—54	13	"	
	1855—64	12	"	
	1865	1	"	
	1866 (Kriegsjahr)	8	"	

In den Jahren 1847, 55, 56, 58 und 63 starb in der Armee *Niemand* an den Pocken, während in diesen Jahren die Civilbevölkerung durch die Pocken bald mehr, bald minder grosse Verluste erlitt.

"Wenn man erwägt," fährt Kussmaul fort, "dass es sich in der Armee hauptsächlich um die Altersklasse von 20—25 Jahren handelt, welche erfahrungsmässig nach der Impfung in der Kindheit die Anlage zur Blatternkrankheit wieder in hohem Maasse erlangt hat (Heim), sowie dass die Ansteckung durch das Zusammenleben in der Kaserne ungemein erleichtert wird, so kann man ein glänzenderes Resultat gar nicht erwarten. In der That blieb die Armee inmitten grosser Pockenepidemieen, welche die Civilbevölkerung des Landes zuweilen schwer heimsuchten, fast wie gefeit und unbeschädigt.

"Dieselben günstigen Ergebnisse erzielte die gesetzliche Revaccination in den Armeen Bayerns, Badens, Hannovers und anderer Länder, sowie in den Armeen und

zufällig gegriffenen, wo nicht ganz werthlosen Verhältnisszahlen aus Spitälern, Krankenvereinen, vielleicht einzelnen Orten u. s. w."

[1] Kussmaul. "Zwanzig Briefe über Menschenpocken- und Kuhpocken-Impfung." Freiburg, 1870. S. 62.

Flottenmannschaften Schwedens, Dänemarks u. s. w. Nach dem Berichte des königl. bayerischen Kriegsministeriums im Blaubuche verlor z. B. die bayerische Armee seit dem Jahre 1844, wo die Revaccination eingeführt wurde, bis 1855 keinen Mann an den Pocken."

Aber selbst diese höchst überzeugende Argumentation wurde von den Impfgegnern angegriffen. Man braucht kein zu grosses Gewicht darauf zu legen, wenn die oben angeführte Ziffer des Jahres 1866 von Belitzky und Löhnert als zu niedrig angegeben wird.[1] Uebrigens lässt sich aus den Protokollen der deutschen Impf-Commission vom Jahre 1885 die Sterblichkeit der preussischen (beziehungsweise deutschen) Armee vom Jahre 1867 ab noch weiter, und zwar in Parallele mit der Pockensterblichkeit der Civilbevölkerung, verfolgen. Es ergibt sich hieraus, dass auf je 100,000 Personen an Pocken verstorben sind:

	Preussische Civilbevölkerung.	Preuss. (bez. deutsche) Armee.
1867	43.17	0.79
1868	18.81	0.40
1869	19.42	0.40
1870 } 1871 }	260.73	} 60.99
1872	262.37	5.65
1873	35.65	3.01
1874	9.52	0.
	649.47	71.24

(Wir brechen die Zusammenstellung hier ab, da mit dem Jahre 1874 das allgemeine Impfzwangsgesetz in ganz Deutschland in Kraft trat, auf dessen Erfolge wir noch zurückkommen werden.) Wie man sieht, sind im Verlaufe der oben angeführten acht Jahre von je 100,000 Militärpersonen an Pocken 71.24 gestorben, hingegen im Kreise der weniger streng geimpften Civilbevölkerung nahezu zehnmal mehr, nämlich 649.67.

Diese Ziffern machen ohne Zweifel den Eindruck einer glänzenden Rechtfertigung der Impftheorie; trotzdem muss zugegeben werden, dass hieraus gezogene Schlüsse absolut falsche wären. Die Militärbevölkerung besteht aus lauter Erwachsenen, die Civilbevölkerung hingegen zu einem Achttheil aus unter fünfjährigen, zu einem weiteren Achttheil aus 5—10jährigen Kindern, und es ist bekanntlich gerade das Kindesalter, welches der Gefahr, an Blattern zu erkranken und zu sterben, am meisten ausgesetzt ist. Eine Vergleichung der Militär- und Civil-Bevölkerung wäre demnach bloss dann statthaft, wenn man beiderseits dieselben Altersklassen einander entgegenstellte, und selbst dann wäre zu berücksichtigen, dass das Militär eine Auswahl der kräftigsten und widerstandsfähigsten Männer repräsentirt, während alle Schwächlichen und Untauglichen im Kreise der Civilbevölkerung verbleiben (wohingegen andererseits freilich wieder der Umstand in Betracht zu ziehen wäre, dass die schlechtere Verköstigung der Soldaten und deren Unterbringung in grossen Kasernen die Erkrankungs-Chancen des Militärs vergrössert). Man darf es demnach nicht verargen, wenn die Impfgegner sich gegen solche Vergleichungen heterogener Grössen aussprechen und wenn Vogt z. B. sich darüber verwundert, dass die Impffreunde nicht einsehen wollen, wie "ein preussischer Soldat eine andere Persönlichkeit sei, als ein Säugling und ein Schwächling." (Vogt. "Der alte und der neue Impfglaube." Bern, 1881. Seite 267.)

Das kaiserlich deutsche Gesundheitsamt hat aber auf diesen Einwurf die richtige Antwort gegeben, indem es (in seiner Vorlage vom 6. Juni 1883 an den deutschen

[1] Dr. Toni (Pseudonym für Löhnert), "Bureaukraten-Statistik und Impfzwang," Berlin 1875, bemerkte, dass sich diese Zahl nur auf die immobile Armee beziehe, während, wie angeblich noch viele Zeugen bestätigen können, in Prag allein von der mobilen Armee eine bedeutend grössere Anzahl an Pocken starb.

Bundesrath) nicht mehr Armee mit Civil, sondern Armee gegen Armee mit einander vergleicht. Hier entfallen dann die vorher geltend gemachten Einwürfe und verdienen daher die diesbezüglichen Daten die grösste Aufmerksamkeit.

Das deutsche Gesundheitsamt vergleicht die Pockenerkrankung und Pockensterblichkeit der vollkommen revaccinirten, preussischen (beziehungsweise deutschen) Armeen mit jener der nur mangelhaft revaccinirten österreichisch-ungarischen und französischen Armee, und ergeben sich hierbei die in der nachfolgenden Tabelle verzeichneten lehrreichen Resultate, denen wir unsererseits noch die drei letzten Rubriken (Lethalitätsberechnung), sowie einige (in der Tabelle des Gesundheitsamtes fehlenden) Angaben für die österreichisch-ungarische Armee[1] beigefügt haben.

Die sich in der Tabelle ergebenden Schlussziffern sind untereinander deshalb nicht vergleichbar, weil von der französischen Armee gerade aus der grossen Epidemiezeit 1870 und 1871 die Angaben fehlen, ferner auch für das Jahr 1882 keine Daten ausgewiesen sind. Die Vergleiche können sich demnach nur auf die 10 Jahre von 1872 bis 1881 erstrecken.

Hierbei ergibt sich nun, dass auf je 100,000 Mann

von der österreichisch-ungarischen Armee	704	erkrankten,	43	starben,
" " französischen "	131	"	13	"
hingegen in der am besten geimpften preussischen Armee.....	25	"	0.87	"

Aus den hier veröffentlichten Angaben lässt sich ferner auch ein, wenn auch nur indirekt beweisender, so doch sehr lehrreicher Beitrag dafür gewinnen, ob die Impfung wirklich nur vor Erkrankung schütze, einmal von Blattern Ergriffene aber, gleichviel ob geimpft oder nicht geimpft, gleichmässig hinweggerafft würden? Es ergibt sich nämlich, dass dies bei den Heeren der genannten drei Staaten durchaus *nicht* der Fall war. Während nämlich von je 1000 Erkrankten im preussischen Heere nur 35 starben, erlagen im österreichisch-ungarischen Heere 61, und im französischen 103 der Krankheit. Es bleibt aber immerhin ein bemerkenswerthes Factum, dass im österreichisch-ungarischen Heere zwar eine grössere Morbidität, aber trotzdem eine bedeutend geringere Lethalität als im französischen herrscht.

Die verschwindend geringe Morbidität und Mortalität der deutschen Armee legt also ein glänzendes Zeugniss für die grossartige Schutzkraft der Impfung ab. Was man solchen Beobachtungen allein entgegenhalten könnte, wäre der Umstand, dass auch diese sich wieder nicht auf eine ganze Bevölkerung, sondern nur auf einen kleinen — und zwar nach Geschlecht, Alter und Gesundheit auserlesenen — *Theil* derselben beziehen — und so bleibt denn die Hauptfrage nach dem Einflusse der Impfung auf die Morbidität und Mortalität ganzer Bevölkerungen noch immer offen.

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[1] Siehe "Militär-Statistisches Jahrbuch für die Jahre 1880—82." Wien, 1885. Seite XVII.

MORBIDITÄT, MORTALITÄT UND LETHALITÄT

der preussischen, österreichisch-ungarischen, und französischen Armeen in den Jahren 1867-1882.

Jahr.	Morbidität.			Mortalität.			Lethalität.		
	Auf je 100,000 Mann.			Auf je 100,000 Mann.			Auf je 100,000 Erkrankte.		
	Preussen.	Österr.-Ungarn.	Frankreich.	Preussen.	Österr.-Ungarn.	Frankreich.	Preussen.	Österr.-Ungarn.	Frankreich.
1867	74.24	?	231.14	0.79	?	18.22	1.06	?	7.88
1868	38:74	?	632.99	0.40	?	42.82	1.04	?	6.76
1869	43.42	?	372.79	0.40	?	22.75	0.92	?	6.10
1870	} 1,280 44	687.2	?	} 60.99	17.28	?	} 4.76	2.51	?
1871		815.8	?		40.1	?		4.91	?
1872	161.35	1,798.	60.0	5.65	101.4	10.7	5.50	5.64	17.83
1873	43.52*	1,658.	27.5	3.01*	109.	4.0	6.92	6.57	14.55
1874	8.34	1,003.	39.7	0.	67.	3.3	0.	6.68	8.31
1875	6.42	336.5	141.83	0.	21.5	17.82	0.	6.39	12.56
1876	6.35	274.7	230.47	0.	10.4	28.23	0.	3.78	12.25
1877	4.89	412.	222.26	0.	25.5	19.62	0.	6.19	8.83
1878	4.58	344.	213.09	0.	15.4	20.14	0.	4.48	9.45
1879	2.12	303.8	115.6	0.	22.7	8.9	0.	7.47	7.69
1880	6.93	475.3	153.6	0.	25.2	14.9	0.	5.29	9.70
1881	4.5	434.2	111.2	0.	29.	7.9	0.	6.70	7.10
1882	2.2	423.	?	0.	27.6	?	0.	6.54	?
Jahres-Durchschnitt für die ganze Periode.									
	105.50	689.65	196.32	4.45	38.62	16.87	4.22	5.60	8.58
Desgleichen für 1872-1881.									
	24.85	703.07	181.52	0.86	42.71	13.55	3.46	6.07	10.03

* 5 Quartale; von hier abstatt des Kalenderjahres Militärjahr vom 1. April-31. März.

SIEBENTES KAPITEL.

ALLGEMEINE EINWÄNDE DER IMPFGEGNER GEGEN DIE LOGIK DER IMPFSTATISTIK.

1. Fälschung der statistischen Ergebnisse durch tendentiöse Verrechnung der zweifelhaften Fälle. Letztere haben sich aber als nicht in's Gewicht fallend herausgestellt.
2. Die Ungeimpften repräsentiren von Haus aus eine schwächere Gesammtheit und zwar

a) *weil die Kinder enthaltend.* Hieraus Postulat der Unterscheidung nach Altersklassen und Verwerfung aller bisherigen impffreundlichen Ergebnisse. Nachweis, dass auch die Statistik der Impfgegner diesem Einwande unterworfen, und dass die angeregte Verbesserung schon früher, und zwar von Impffreunden eingeführt wurde. Berufung auf die impffeindlichen Ergebnisse der nach Altersstufen fortschreitenden Arbeiten von Müller und Keller.

Kritik der Müller'schen Arbeit. Nachweis der Unverlässlichkeit derselben.

Kritik der Keller'schen Arbeit. Aufsehen, welches die impffeindlichen Resultate derselben erregten. Revision der Keller'schen Arbeit durch den Verfasser. Dieselbe erweist sich als unverlässlich in ihren Quellen, überdies aber als durch Keller tendentiös entstellt.

b) *weil die Ungeimpften von Haus aus schwächer sind* und deshalb allen Krankheiten häufiger unterliegen. Dieses Argument macht alle Blattern-Statistik unmöglich. Lösung dieser Schwierigkeit im zweiten Theile durch die Methode der "Intensitäts-Berechnung".

In den bisherigen Abschnitten haben wir die Thesen der Impfvertheidigung Revue passiren lassen und gleichzeitig auch jene Argumente der Impfgegner angeführt, durch welche sie jene zu bekämpfen und abzuwehren suchen. Wir haben hierbei gefunden, dass die indirekten Beweise der Impfvertheidigung nicht ganz ausreichen, und dass das Schwergewicht in Folge dessen in den direkten Beweisen von der geringeren Gefährdung der Geimpften zu suchen wäre. Wir haben aber gesehen, dass sich diese ersehnten direkten Beweise für ganze Bevölkerungen bisher noch nirgends herstellen liessen; dass aber, soweit diese für künstliche Bevölkerungs-Complexe herstellbar sind — so für Hospitalsbevölkerung und für Armeen —, die Thatsachen sehr zu Gunsten der Impfung sprechen. Freilich konnten wir nicht umhin, diese Beweise, eben deshalb, weil selbe sich auf künstlich ausgewählte Gesammtheiten beziehen, als ergänzungsbedürftig anzuerkennen. Die Impfgegner gehen aber noch weiter, indem sie die ganze Logik dieser direkten Beweisführung geradezu für falsch erklären. Wir übergehen nun zu diesen allgemeinen logischen Einwänden der Impfgegner gegen die Beweiskraft der Impfstatistik.

1) VERRECHNUNG DER ZWEIFELHAFTEN FÄLLE.

Bei allen direkten Beweisen handelt es sich in erster Reihe darum, den Kreis der zu beobachtenden Geimpften und Ungeimpften zu fixiren, zu schliessen. Es wird nun darauf hingewiesen, dass es nicht immer möglich sei, den Impfzustand der Erkrankten genau zu constatiren, sondern dass überall eine gewisse Menge zweifelhafter Fälle übrig bleibe, die jedoch nirgends als solche ausgewiesen würde. Solcherart konnte es geschehen, dass derartige zweifelhafte Fälle durch impffreundliche Beobachter, je nach dem Ausgange, einmal zu den Geimpften, ein andermal zu den Ungeimpften gezählt und so die Resultate der Impfstatistik tendentiös beeinflusst wurden.[1] Dieser Einwurf führte zu der — im Wesen eigentlich nicht bedeutenden — Verbesserung, dass man diese zweifelhaften Fälle getrennt auszuweisen begann. Es stellte sich nun hierbei heraus,

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[1] So sagt z. B. Lorinser (*Wittelshöfer's medic. Wochenschrift*, 1886, Seite 228): "Die Aerzte, in deren Interesse ja die Aufrechterhaltung des Impfdogmas liegt, sind immer mehr geneigt, einen an Blattern Verstorbenen zu den Ungeimpften zu zählen."

dass deren Anzahl viel geringer zu sein pflegt, als man angenommen, dass ferner deren Lethalität sich zwischen jener der Geimpften und Ungeimpften zu bewegen pflegt, dass also deren Zu- oder Abrechnung das Gesammtergebniss nicht empfindlich beeinträchtigt. Immerhin mag man auch ferner in der Vaccinations-Statistik die zweifelhaften Fälle gesondert nachweisen.

Mehr auf den Kern der Sache gehen aber die folgenden Einwände der Impfgegner.

2) SCHWÄCHERE WIDERSTANDSKRAFT DER UNGEIMPFTEN.

Soweit es unter den obschwebenden erschwerenden Verhältnissen möglich ist, Lethalitätsziffern für Geimpfte und Ungeimpfte festzustellen, haben die Resultate fast ausnahmslos zu Gunsten der Geimpften gelautet. Wie überzeugt man auch von der Mangelhaftigkeit dieser Daten sein mochte, musste man durch den übereinstimmenden Inhalt derselben doch zu Gunsten der Impfung gestimmt werden. Hier setzen nun die Impfgegner mit dem Argumente ein, dass die grosse Anzahl der erkrankten und verstorbenen Ungeimpften eigentlich dadurch zu erklären sei, dass die Gesammtheit der Ungeimpften schon von Haus aus eine Gesammtheit der Schwächeren bilde, weshalb von diesen, nicht nur an Blattern sondern auch an allen anderen Krankheiten, unbedingt mehr sterben müssten. Die Ungeimpften rekrutirten sich aus den ärmeren und ungebildeteren Klassen,[1] ferner enthielten dieselben die Kinder, namentlich aber fast alle Säuglinge; überdies aber würden eben alle kränklichen und schwachen Kinder der Impfung entzogen, die Gesunden aber geimpft. Unter solchen Umständen wäre die grössere Blatternsterblichkeit der Ungeimpften nicht eine Folge der unterlassenen Impfung, sondern einerseits der stärkeren Besetzung des Kindesalters, in welchem die Sterblichkeit bekanntlich am grössten ist und in welchem der Mensch nicht nur den Blattern, sondern auch in allen übrigen Krankheiten am raschesten unterliegt,—andererseits der Armuth und schwächeren Constitution, welche ebenfalls die allgemeine Widerstandskraft herabsetzen. Betrachten wir diese beiden Einwürfe für sich.

α) UNTERSCHEIDUNG DER ALTERSKLASSEN.

Unter den Geimpften gibt es gar keine kleinen Kinder, ganz gewiss keine Säuglinge, unter den Ungeimpften befindet sich aber das ganze Heer der zarten Säuglinge und Kinder. Wäre man in der Lage, die Kinder auf beiden Seiten ausser Rechnung zu lassen, wer weiss—meinen die Impfgegner—wie günstig dies die Pockenmortalität der Ungeimpften beeinflusste. Man erklärte in Folge dessen jede Pocken-Statistik für absolut werthlos, wenn in derselben nicht eine Klassifizirung nach Altersstufen durchgeführt würde. Zumindest müssten die Kinder von den Erwachsenen getrennt werden. Lorinser[2] aber bestand auf noch eingehenderer Auseinanderhaltung der Altersklassen, und zwar für das erste Lebensjahr nach Quartalen, für das Kindesalter nach einzelnen Jahren, und für das spätere Alter nach Jahrzehnten. Ein solches Dokument aber, hiess es zur Zeit als man diese Forderung aufstellte, sei in der ganzen Impf-Literatur nicht aufzutreiben (Vogt) und dieselbe eben deshalb werthlos.

Die Forderung der Auseinanderhaltung der Altersklassen ist im Principe vollkommen berechtigt. Man kann nur Gleiches mit Gleichem vergleichen und es ist klar, dass hinsichtlich der Sterblichkeit einjährige Säuglinge und dreissigjährige Männer nicht

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[1] "Das Geheimniss, warum geimpfte Kinder besser daran sind, als ungeimpfte, liegt aber darin, dass Eltern, welche ihren Kindern eine grössere Sorgfalt zuwenden können, in der Regel dieselben auch impfen lassen, während andere Eltern nicht nur die Impfung, sondern ihre Kinder überhaupt vernachlässigen." Lorinser in *Wittelshöfer's Wochenschrift.* 1886. Seite 226.

[2] Lorinser. "Bedenken gegen die Impfung." *Wittelshöfer's medicinische Wochenschrift.* 1873.

als gleichartig betrachtet werden können. Die Forderung nach Auseinanderhaltung der Alterklassen ist übrigens auf dem ganzen Gebiete der Demographie eine so durchgreifende, so allgemein anerkannte, dass es mit Recht Wunder nehmen muss, dass die Impf-Statistik hierauf keine Rücksicht genommen. Der Grund mag wohl darin zu suchen sein, dass sich mit dieser Frage die Aerzte mehr als die Fach-Statistiker beschäftigt haben. In Erwiderung des hieraus gegen die Impfvertheidigung geschmiedeten Vorwurfes mögen aber zwei Bemerkungen nicht unerwähnt bleiben.

Es möge vor Allem bemerkt werden, dass die Impfgegner selbst diese Unterscheidung bis in die letzte Zeit auch nicht angewendet; man vergleiche z. B. Lorinser's eigenen Spitalsberichte, desgleichen die Abhandlungen seines Primarius für Hautkrankheiten, Hermann, u. A. m. Es reducirt sich solcherart dieser Angriff der Impfgegner gegen die Impffreunde auf einen für Freund und Feind gleichmässig geltenden Verbesserungsvorschlag. Aber zweitens entspricht es auch nicht den Thatsachen, als ob diese Verbesserung erst von Impfgegnern hätte entdeckt werden müssen.[1] Marson z. B., Direktor des Londoner Pocken-Spitals, ein unentwegter Vertheidiger der Impftheorie, hat in seiner früher citirten Arbeit schon vor Jahrzehnten die Lethalität der Blatternkranken nach Altersklassen gesondert und ist hierbei zu Resultaten gelangt, die bei jeder Altersklasse überaus deutlich für den Impfschutz sprechen.[2]

Thatsache ist es aber, dass diese Einführung nicht genügende Beachtung fand, ja es scheint, dass dieselbe in Vergessenheit gerathen. Mir wenigstens ist nicht bekannt, dass die diesbezüglichen Angriffe der Impfgegner durch Producirung entsprechend aufgearbeiteter Statistiken abgewiesen worden wären. Selbst im Wiener allgemeinen Krankenhause erfolgte die Blattern-Statistik bis zum Jahre 1872 ohne Berücksichtigung der Altersklassen, und erst in Folge des Drängens der Impfgegner finden wir im Jahre 1873 diese wichtige Unterscheidung zum ersten Male in die Statistik dieser hervorragenden Heilanstalt eingeführt. Vogt konnte, indem er sich auf diese Forderung stützte, noch im Jahre 1879, ohne eine Widerlegung zu erfahren, erklären, dass die ganze Impfstatistik mangels der Alters-Unterscheidung unbrauchbar sei und dass es gegenwärtig nur zwei Statistiken gäbe, welche dieser Kardinalforderung entsprächen, jene nämlich von Müller in Berlin und von Keller in Wien, welche beide aber auf diesem Wege übereinstimmend zu dem Ergebnisse führten, dass die Impfung eigentlich unnütz, wenn nicht gar schädlich sei. Wir wollen uns nun mit diesen angeblich einzig richtigen Impfstatistiken beschäftigen.

MÜLLER'S Arbeit über die Berliner Blatternepidemie v. J. 1871 erschien i. J. 1872 in *Eulenberg's Vierteljahresschrift*. Am Schlusse der daselbst veröffentlichten Tabellen meint der Verfasser, dass dieselben den Beweis für einen entschiedenen Schutz der Vaccination ergäben. Auch Prof. Auspitz hat im Vertrauen auf die Richtigkeit dieser

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[1] So z. B. vindicirt auch Kolb (Impffrage, Seite 32) dieses Verdienst den Impfgegnern Oidtmann und Löhnert. Desgleichen bemerkt Vogt (Für und wider das Impfen, Seite 156): "Es bleibt das grosse Verdienst von Löhnert, diesen Grundsatz auf unser Thema angewendet zu haben."

[2] Marson in den "Medical and Chirurgical Transactions." Volume 36. Es ergibt sich hier, dass auf je hundert Erkrankte verstarben:

Im Alter von	Von Ungeimpften.		Von Geimpften.
0— 5 Jahren	50 Procent,		(Nur 7 Fälle, davon 2 gestorben.)
5—10 "	27 "		13 Procent.
10—15 "	23 "		5 "
15—20 "	26 "		6 "
20—25 "	40 "		9 "
25—30 "	45 "		10 "
30—40 "	47 "		13 "
40—60 "	69 "	(bloss 26 Erkrankte)	17 "

Bemerkung diese Daten in dem Referate des niederösterreichischen Landes-Sanitätsrathes als hohen Werth besitzende bezeichnet. Wenn man aber die diesbezüglichen Zahlenangaben untersucht, so findet man, dass dieselben, so wie sie von Müller publicirt worden sind, über den Einfluss der Vaccination gar keinen Schluss erlauben. Man muss erst die Ziffern-Colonnen umstellen, durch Subtractionen die nöthigen Gesammtheiten der Geimpften und Ungeimpften erhalten und für jede Altersklasse Percentualberechnungen vornehmen, bis man die Zahlen dazu bringt, ein Urtheil über diese Frage abzugeben. Hätte Müller diese Operation durchgeführt, so wäre er zu dem überraschenden Schlusse gekommen, dass seine Angaben gar nichts für den Schutz der Impfung beweisen. Er selbst hat dies merkwürdiger Weise unterlassen, und so blieb es einem schärferen Beobachter, Lorinser, überlassen, diese Rechnung kurz nach Veröffentlichung der Original-Arbeit anzustellen und deren für seinen impfgegnerischen Standpunkt werthvolle—und gewiss auch für Müller überraschende —Resultate der Welt zu verkünden. Als dann im Jahre 1873 auf dem internationalen medicinischen Congresse zu Wien der daselbst anwesende Medicinalrath Müller sich veranlasst sah, seine eigenen Angaben als unrichtige zu recociren, hatte die Impfvertheidigung hierdurch unläugbar eine unliebsame Schlappe erlitten.[1]

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[1] Ohne der wissenschaftlichen Autorität des Herrn Dr. Müller nahe treten zu wollen, bin ich doch genöthigt, zu bemerken, dass seine Verlässlichkeit in statistischen Dingen Vieles zu wünschen übrig lässt. Hierfür spricht schon genügend die oben angeführte Thatsache. Ferner ist zu bemerken, dass z. B. nach Guttstadt (*Zeitschrift des preussischen statistischen Bureaus*, 1873) die von Müller angegebene Anzahl der zu jener Zeit in Berlin an Blattern Verstorbenen viel zu niedrig ist, nachdem dieselbe nicht 3552, sondern fast um die Hälfte mehr (!) nämlich 5084 betrug. Die Müller'sche Arbeit scheint mir aber nicht nur hinsichtlich ihrer Vollständigkeit, sondern auch hinsichtlich ihrer Beschaffenheit unverlässlich. Es ergibt sich mir dies aus einer Zusammenstellung der durch ihn selbst veröffentlichten Lazareth-Daten. Wie bekannt rekrutiren sich die Ungeimpften zumeist aus den ärmeren Klassen: bei einer vergleichenden Zusammenstellung des Impfzustandes der in den Spitälern Verstorbenen mit den in Wohnungen Verstorbenen, wird also unter den Spitalsverstorbenen die grössere Anzahl Ungeimpfter zu finden sein. Nun sind nach Müller verstorben

	Geimpfte,	Ungeimpfte,	zusammen
in ganz Berlin	2419	1133	3552
in den vier Lazarethen	889	91	980
Zieht man die Letzteren ab, so erhält man für die in Wohnungen Verstorbenen	1530	1042	2572

Es ergibt sich also solcherart das höchst unwahrscheinliche Resultat, dass während von den Spitalsverstorbenen nicht weniger als 91 Procent geimpft waren, von den in Wohnungen Verstorbenen nur kaum 60 Procent geimpft gewesen wären. Es wäre nun allerdings möglich, diesen grossen Unterschied damit zu erklären, dass in den Spitälern keine Kinder aufgenommen zu werden pflegen und dieser Umstand die verhältnissmässig grosse Anzahl der in den Wohnungen verstorbenen Ungeimpften verursacht haben mochte. Nun ist aber nachgewiesen, dass in die Lazarethe auch untereinjährige Kinder aufgenommen wurden (man vergleiche z. B. Dr. Lothar Meyer's Bericht über die Pockenkrankheit im Berliner städtischen Pockenkrankenhause in den Jahren 1870 und 1871, in Göschen's deutscher Klinik). Wenn man aber auch annimmt, dass in die Spitäler gar kein Kind unter einem Jahre aufgenommen worden wäre, und so sämmtliche im ersten Lebensjahre Verstorbene, nämlich 99 Geimpfte und 437 Ungeimpfte, zusammen 536, in den Wohnungen verstorben wären, so verbleiben selbst nach Abzug dieser Personen noch immer 1431 Geimpfte und 605 Ungeimpfte, zusammen 2006 als in Wohnungen verstorben, d. i. noch immer nur 71 Procent Geimpfte in den Wohnungen gegen die 91 Procent Ungeimpfter in den Spitälern. Hier muss also wieder irgend ein derber Irrthum unterlaufen sein. Ferner findet sich auch bei Prüfung der über die Lazarethe mitgetheilten Daten, dass die Summen der einzelnen Rubriken Müller's mit dem von ihm ebendaselbst angegebenen Totale nicht stimmen. Nach alle dem darf also behauptet werden, dass die Müller'schen Angaben, nicht nur quantitativ, sondern auch qualitativ genommen, lückenhaft zu sein scheinen.

KELLER. Einen noch grösseren Stoss sollte die Impftheorie aber erleiden, als Keller mit seinen Daten über die Blatternsterblichkeit der Bediensteten der österreichischen Staatsbahn hervortrat,[1] bei deren Aufarbeitung er nicht nur die Altersklassen nach Lorinser's Schema rigoros auseinanderhielt, sondern auch noch darauf Gewicht gelegt hatte, dass neben Geimpften und Ungeimpften auch die zweifelhaften Fälle, ferner die Geblatterten und Revaccinirten besonders ausgewiesen würden.

Keller gelangte nun zu dem alle Welt höchlichst überraschenden Ergebnisse, dass von Geimpften in manchen Altersklassen viel mehr als von Ungeimpften an Blattern stürben. Ein solches Resultat musste nicht wenig dazu beitragen, das Vertrauen in dem Impfschutz zum Schwanken zu bringen. Die Beobachtungen Keller's wurden nicht nur in Oesterreich, sondern auch in Deutschland, selbst im deutschen Parlamente, in der Schweiz, in England[2] und überall, wo man Beweismaterial gegen die Impfung suchte, als entscheidende Argumente angeführt, und zwar um so lieber, als die Keller'schen Daten zugleich eine lehrreiche Illustration dafür bieten, wie Blatternstatistiken, welche alle Fälle, ohne Unterscheidung der Altersklassen, in *eine* Summe zusammenfassen, nach dem Inhalte dieser Hauptergebnisse sehr wohl zu Gunsten der Impfung sprechen können, während bei einem Eingehen auf die einzelnen Altersstufen sich dennoch entgegengesetzte Resultate ergeben mögen. Nach den Hauptsummen starben nämlich

	von 2069	erkrankten	Geimpften	317=**15.32**	Procent,
hingegen von	1095	"	Ungeimpften	271=**24.74**	"
ferner von	92	"	Revaccinirten	16=17.39	"
ferner von	19	"	Geblatterten	5=26.39	"
ferner von	110	"	Zweifelhaften	16=14.54	"
insgesammt von	3385	"		625	

also von Ungeimpften um die Hälfte mehr als von Geimpften. Löst man aber die Erkrankten in ihre einzelnen Altersgruppen auf, so ergibt sich, dass die grösste Sterblichkeit das früheste Kindes-, bez. Säuglingsalter betrifft, dessen Angehörige natürlich zumeist ungeimpft sind. Die grosse Sterblichkeit entpuppte sich solcherart als grosse Sterblichkeit der Säuglinge: sie ist eine Folge des Kindesalters und nicht der Nichtimpfung. Lässt man nun in Berücksichtigung dieses Umstandes die Kinder ausser Betracht, so ergibt sich z. B. für die Ueberzweijährigen fast gar kein Unterschied mehr. Es starben nämlich

von 1939	erkrankten	Geimpften	255=**13.15**	Procent
und von 695	"	Ungeimpften	93=**13.38**	"

ja, bei weiterer Auftheilung der Ueberzweijährigen gelangt man zu dem höchst bemerkenswerthen Ergebnisse, dass von den Geimpften in einzelnen Altersklassen sogar mehr starben; so starben z. B. von je 100

	Geimpften		Ungeimpften	
im Alter von 4— 5 Jahren	20	Procent, bez. nur	14.93	Procent
" " " 5—10 "	18.84	" " "	8.90	" u. s. f.

Bei der grossen Bedeutung, welche diesen Zahlen innewohnt und welche nur erhöht

[1] Die erste Veröffentlichung (Resultate des Jahres 1872) erfolgte in der *Allg. Wiener medicinischen Zeitung*, 1873. Die zweite, für das Jahr 1873, ein Jahr darauf ebendaselbst, vermehrt durch eine Specificirung der Hauptergebnisse (ohne Altersangabe) nach den einzelnen ärztlichen Bezirken. Von beiden Aufsätzen wurden Separatabdrücke in sehr beschränkter Auflage (Wien, Verlag des Verfassers) veranstaltet. Die Ergebnisse des dritten Beobachtungsjahres wurden in *Wittelshöfer's medicinischer Wochenschrift* (1876, Nos. 33 und 34) veröffentlicht; ebendaselbst findet sich auch die Recapitulation für alle drei Beobachtungsjahre.

[2] S. z. B. Milne's "The Mitigation Theory of Vaccination," London, 1886, welche impfgegnerische Streitschrift ausschliesslich aus einer populären Paraphrase der Beobachtungen Keller's besteht.

wird, wenn man die bekannte stramme Organisation des Dienstes der österreichischen Staatsbahn in Betracht zieht; bei der Rolle ferner, welche die Keller'schen Angaben in dem Kampfe um die Vaccination spielen, schien es mir der Mühe zu lohnen, diesen wichtigen Beobachtungen bis auf die Quelle nachzugehen und dieselben, wenn nur irgendwie möglich, einer Ueberprüfung zu unterziehen. Ich begab mich zu diesem Zwecke nach Wien, musste aber zu meiner unangenehmen Ueberraschung erfahren, dass Keller, als er aus seinem Amte schied, die betreffenden Akten unrechtmässiger Weise mit sich genommen hatte. Da Dr. Keller inzwischen auch mit Tod abgegangen war, und sich über den Verbleib des Urmaterials gar nichts ermitteln liess, versuchte ich den letzten Ausweg, indem ich mich an die aus jener Zeit noch lebenden Bahnärzte mit der Bitte wendete, mir eventuell vorhandene Copien ihrer seinerzeitigen Berichte zuzusenden, auf welchem Wege ich wenigstens einige Bruchstücke des werthvollen Materials zu retten hoffte. Dies ist mir auch insofern gelungen, als von neunzehn zur Zeit noch lebenden Bahnärzten acht die Güte hatten, meiner Bitte zu entsprechen, und ich in Folge dessen in die Lage kam, Keller's Daten für 549 Fälle zu reconstruiren.

Ich werde sogleich auf den Inhalt dieser Daten zurückkommen. Ich muss nur im Vorhinein noch bemerken, dass ich bei Beschäftigung mit diesem Materiale zur Ueberzeugung gelangte, dass dasselbe durchaus nicht so verlässlich ist, als man im Vertrauen auf Keller's amtliche Stellung angenommen. Aus den dem internationalen ärztlichen Congress zu Washington vorgelegten Originalbriefen war ersichtlich, dass die Ansicht, als ob Keller ursprünglich Impffreund gewesen und erst unter der Pression seiner Beobachtungen zum Impfgegner geworden wäre — eine Ansicht, die auch der wohlwollende Kolb theilte — eine irrige gewesen. Keller war nicht nur von Haus aus Impfgegner, sondern war dessen impffeindliche Tendenz seinen Streckenärzten so sehr bekannt, dass, wie dies in vorgelegten Briefen zugestanden wird, demselben von mancher Seite möglichst nach seinem Geschmack präparirte Daten übermittelt wurden! Ferner ist der ebendaselbst vorgewiesenen Copie eines Kranken-Protokolls der österreichischen Staatsbahn zu entnehmen, dass in demselben *gar keine Rubrik für den Impfzustand der Behandelten vorfindlich ist*, dass also die Constatirung des Impfzustandes höchstens *nach* dem Erlasse des Chefarztes allgemein werden konnte, vorher aber von den wenigsten Aerzten vorgenommen wurde. Da nun der betreffende Direktions-Erlass erst vom 15. *November* 1872 datirt ist, die Statistik Keller's aber die Pockenpidemie des ganzen Jahres 1872, von Jänner ab, umfasst, so kann — wenigstens für die Mehrzahl der Fälle — den diesbezüglichen, nachträglichen Angaben der Aerzte keine Authenticität zugesprochen werden, zumal wenn man bedenkt, wie beim grossen Wechsel der Bahnbediensten eine nachträgliche Befragung der betreffenden Personen in den meisten Fällen zur Unmöglichkeit wurde.[1]

Die Quelle selbst ist also durchaus nicht verlässlich. Trotz dieser Mängel aber, welche in ihrer Gesammtwirkung eine impfgegnerische Tendenz dieser Statistik her-

[1] Dieser Umstand erklärt auch die grosse Anzahl der zweifelhaften Fälle in meiner Tabelle, was eben daher resultirt, dass mangels einer entsprechenden Rubrik für den Impfzustand, die von mir aufgeforderten Aerzte in vielen Fällen nicht im Stande waren, darüber genauen Bescheid zu geben, ob der Patient geimpft gewesen oder nicht. (In der Altersklasse von 15—40 Jahren betragen die zweifelhaften Fälle nahezu ein Viertheil sämmtlicher Fälle!) Ferner wurde bereits darauf hingewiesen, dass die Lethalität der zweifelhaften Fälle naturgemäss *zwischen* jene der Geimpften und Ungeimpften zu stehen kommen müsste. Da nun Keller für diese zweifelhaften Fälle eine *geringere* Sterblichkeit als für die Geimpften oder Ungeimpften nachweist, so möchte man schon hieraus folgern — was später noch deutlicher bewiesen werden soll — dass bei Keller eine tendentiöse Verschiebung der Kathegorien, und zwar in der Richtung stattfand, dass eine beträchtliche Anzahl geheilter Geimpfter in die Rubrik der zweifelhaften Fälle eingestellt wurde.

vorbringen mussten, ergaben die von mir nachträglich noch zusammentragbaren Daten das nachfolgende *zu Gunsten* der Impfung sprechende Resultat:

Blattern-Statistik von acht Bahnstrecken der österreichischen Staatsbahn-Gesellschaft (1872–73).

Alter.	Geimpfte.			Nicht Geimpfte.			Revaccinirte.			Geblatterte.			Zweifelhafte.			Zusammen.		
	Erkrankte.	Geheilte.	Gestorbene.	Erkrankte.	Geheilte	Gestorbene.	Erkrankte.	Geheilte.	Gestorbene.	Erkrankte.	Geheilte.	Gestorbene.	Erkrankte.	Geheilte.	Gestorbene.	Erkrankte.	Geheilte.	Gestorbene.
0— 3 Monate.....	1	1	—	18	4	14	—	—	—	—	—	—	—	—	—	19	15	14
4— 6 "	1	1	—	18	12	6	—	—	—	—	—	—	—	—	—	19	13	6
7— 9 "	—	—	—	13	9	4	—	—	—	—	—	—	—	—	—	13	9	4
10—12 "	3	3	—	9	5	4	—	—	—	—	—	—	—	—	—	12	8	4
0— 1 Jahr	5	5	—	58	30	28	—	—	—	—	—	—	—	—	—	63	35	28
1— 2 "	4	2	2	12	7	5	—	—	—	—	—	—	—	—	—	16	9	7
2— 3 "	10	8	2	7	4	3	—	—	—	—	—	—	—	—	—	17	12	5
3— 4 "	9	7	2	13	10	3	1	1	—	—	—	—	—	—	—	23	18	5
4— 5 "	14	11	3	9	7	2	—	—	—	—	—	—	—	—	—	23	18	5
5—10 "	89	71	18	27	22	5	3	3	—	1	1	—	1	1	—	121	98	23
10—15 "	69	68	1	8	7	1	—	—	—	—	—	—	2	1	1	79	76	3
15—20 "	104	101	3	6	6	—	—	—	—	2	2	—	3	—	3	115	109	6
20—30 "	80	77	3	7	6	1	1	1	—	—	—	—	6	6	—	94	90	4
30—40 "	42	36	6	1	1	—	—	—	—	1	1	—	15	14	1	59	52	7
40—50 "	13	12	1	—	—	—	—	—	—	—	—	—	8	7	1	21	19	2
50—60 "	4	4	—	—	—	—	—	—	—	—	—	—	1	1	—	5	5	—
Unbekanntes Alter	8	8	—	—	—	—	—	—	—	—	—	—	—	—	—	8	8	—
Zusammen.....	451	410	41	148	100	48	5	5	—	4	4	—	36	30	6	644	549	95
Ueber 2 Jahre.....	422	403	39	78	63	15	5	5	—	4	4	—	36	30	6	563	505	60
			8.82 %			19.23 %												

Wenn man also auch von den zwei ersten Lebensjahren absieht, findet sich trotz der konstatirten Unverlässlichkeit des Materials noch immer, dass von hundert geimpften Kranken 8.82% starben, von hundert Ungeimpften aber **19.23** %, demnach mehr als zweimal so viel!

Fragen wir uns nun, wieso es kommen konnte, dass Keller aus diesen selben Berichten der Bahnärzte impffeindliche Ergebnisse zusammenstellte, so muss ich Ihnen hierauf die verblüffende Aufklärung geben, dass Keller die ihm von seinen Aerzten zugeschickten statistischen Ausweise tendentiös corrigirte! Unter den mir eingegangenen acht Berichten der Bahnärzte *ist kein einziger*, den Keller unverändert übernommen hätte: in jedem einzelnen wurden von ihm Aenderungen angebracht, und zwar alle in

dem Sinne, um die Lethalität der Geimpften zu erhöhen und jene der Ungeimpften zu erniedrigen!

Ich weiss sehr wohl, eine wie schwere Beschuldigung ich hiermit ausspreche, und fühle das Gewicht derselben um so schwerer, als Keller todt ist und sich nicht mehr vertheidigen kann. Eben deshalb halte ich mich verpflichtet, den ganzen Gang meiner Untersuchung kontrolirbar zu machen, und habe zu diesem Zwecke auch die betreffenden Originalbriefe mit mir gebracht und lege Ihnen dieselben zur Einsicht und beliebigen Ueberprüfung vor.

In meinem über die Statistik der Wiener impfgegnerischen Schule in der ungarischen Gesellschaft für Hygiëne gehaltenen Vortrage bin ich auf die Details dieser Untersuchung und auf den Inhalt jedes einzelnen der acht Berichte eingegangen. Es genüge also, hier nur so viel zu sagen, dass schon in diesen wenigen Briefen wir wiederholt der Versicherung begegnen, dass die Impfung sich vom wohlthätigsten Einflusse gegen die Blattern-Erkrankung erwiesen habe. Ferner möge, um zu beweisen, wie Keller mit der Statistik seiner Bahnärzte umgesprungen, die Anführung eines der acht Fälle genügen.

Dr. Pichler, Werksarzt in Steierdorf, sendet mir unterm 21. Februar 1887 die hier vorgewiesene Copie seiner an Keller eingesandten Originaltabelle über die Blatternstatistik der 5500 Seelen zählenden Colonie Steierdorf; wie Sie aus dieser Tabelle ersehen, sind daselbst bei den Geimpften nur einer von je 25 Kranken, bei den Ungeimpften aber einer von je Dreien gestorben, nämlich von 38 Kranken dreizehn. Nach Keller hätte aber die Sterblichkeit der Ungeimpften nur $\frac{1}{5}$ betragen, indem nämlich bei ihm die Anzahl der ungeimpften Kranken von 38 auf 68 corrigirt erscheint.

In Folge dieser auffälligen Verschiedenheit schrieb ich am 1. März nochmals an Herrn Dr. Pichler, und indem ich die Wichtigkeit seiner für Keller gravirenden Aussage hervorhob, ersuchte ich ihn, nochmals nachzusehen, ob nicht seinerseits ein Irrthum untergelaufen. Hierauf erfolgte am 4. März die Anwort, dass dies nicht der Fall, sondern dass die Angaben Keller's unrichtig seien. "Die Ihnen eingesendeten Daten," heisst es, "sind *wahrheitsgetreu* und stimmen vollkommen mit meinen Kranken- und Todten-Matrikeln." Da Dr. Pichler seine Berichte seiner Zeit überdies auch an die Comitatsbehörde gesendet hatte, ist Freunden und Vertheidigern Keller's die Möglichkeit geboten, auch an dieser Stelle nachzuforschen. Dieselben dürften hierbei noch die fernere Entdeckung machen, dass Keller nicht nur die Zahlen der Ungeimpften, sondern auch jene der Geimpften "corrigirte", hier aber freilich in umgekehrtem Sinne, indem er nämlich die Anzahl der Verstorbenen vergrösserte, jene der Erkrankten aber verringerte, hierdurch also den Sterblichkeits-Coëfficienten der Geimpften künstlich erhöhte.

Aehnliche Aenderungen finden sich in allen übrigen mir zugekommenen Berichten. Die impffeindlichen Ergebnisse Keller's beruhen folglich auf Fälschung der Thatsachen.

So ist es also um diesen Mauerbrecher der impfgegnerischen Statistik bestellt; in dieser — Gottlob in der Geschichte der Statistik beinahe beispiellosen — Weise wurden Daten fabrizirt, die der Welt als die vertrauenswürdigste und gewissenhafteste Impf-Statistik vorgelegt wurden, die in den verschiedensten Staaten so viel Staub aufwirbelten und der Impffrage so grossen Schaden zufügten. Wenn keine andere, so werden die diesbezüglichen Nachforschungen doch zumindest jene Frucht tragen, dass die Impfgegner von nun an verzichten werden, sich noch weiter auf die bisher so unendlich hoch gehaltenen Daten Keller's zu beziehen.[1]

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[1] *Nachschrift.* — Ich habe die ganze auf diesen Gegenstand Bezug habende Correspondenz, sammt Belegen, dem internationalen medicinischen Congress zu Washington (1887) vorgelegt und ersucht, die Richtigkeit meiner Angaben einer Prüfung zu unterziehen. Es wurde in Folge

2. GERINGERE WIDERSTANDSKRAFT DER UNGEIMPFTEN.

Der nächste Einwand der Impfgegner, wonach nämlich die Gesammtheit der Ungeimpften schon von Haus aus eine Gesammtheit der Armen und Schwachen repräsentire, ist unbedingt einer der schwersten Streiche, die gegen den Impfschutzbeweis geführt werden können, und zwar um so mehr, als die Stichhaltigkeit dieses Einwandes eine ganz augenscheinliche ist.[1]

Dass die Mortalitäts-Verhältnisse der Ungeimpften ungünstiger sind, wird — so sagt Lorinser[2] — von keinem der Impfgegner bestritten; die Ursache hiervon liegt aber nicht in der Nichtimpfung, sondern in den soeben angeführten übrigen Verhältnissen.

Es wird also der Vaccinations-Statistik jener Vorwurf gemacht, Ursache und Wirkung geradezu verwechselt zu haben, — ein Irrthum, dem wir auch bei anderen statistischen Beweisgängen zu begegnen pflegen. (Man denke z. B. an die aus dem höheren Lebensalter der Verheiratheten, auf die lebenserhaltende Kraft der Ehe gezogenen Schlüsse, bei welchen übersehen wird, dass schwache, kränkliche Personen nicht zur Ehe zu schreiten pflegen, dass diese also nicht deshalb früher sterben, weil sie ledig geblieben, sondern dass sie ledig blieben, weil sie früher zu sterben fürchten.)

Mit diesem schwerwiegenden kritischen Einwande wird aber eigentlich der Impfstatistik aller Boden unter den Füssen entzogen. Die glänzendsten statistischen Beweise dafür, um wie viel seltener Geimpfte an Pocken sterben, als Ungeimpfte, verwandeln sich in ungreifbare Nebelbilder und lassen stets dem Zweifel Raum, ob die ungünstigeren Verhältnisse bei den Ungeimpften eine Folge der Nichtimpfung oder jener allgemeinen Schwächlichkeit seien, welche sowohl die Nichtimpfung als den frühen Tod gleichmässig als Wirkungen nach sich ziehen!

Eine Lösung dieses Problems wäre nur möglich, wenn man nachweisen könnte, wie gross die Lebensgefährdung Ungeimpfter anderen Krankheiten gegenüber ist. Es wäre hierdurch ein Maass für die durch die schwächere Constitution bedingte Lebensgefährdung dieser Individuen geboten und die Nichtimpfung könnte dann bloss für die über dieses Maass hinaus sich ergebende specielle Uebersterblichkeit an den Blattern verantwortlich gemacht werden. Ein solches statistisches Dokument existirt aber meines Wissens in der impfstatistischen Literatur bisher ebenfalls nicht, und ich glaube daher, keine überflüssige Arbeit gethan zu haben, wenn ich im zweiten Theile dieser Schrift, auf Grund der erwähnten neuen Methode, auf diese wichtige Frage Antwort zu bieten versuche. So aber, wie die Dinge heute stehen, muss zugegeben werden, dass der hier erwähnte Einwand der Impfgegner den direkten Beweisen der Impfvertheidigung eine bisher nicht überwundene Schwierigkeit entgegensetzt und selbst den glänzendsten Lethalitätsergebnissen der Spitalsbeobachtungen mit totaler Entwerthung droht.

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dessen zu diesem Behufe ein Specialcomité entsendet. Nach eingehender Prüfung der Schriftstücke sah dieses Comité sich zu dem harten Ausspruche genöthigt, die Arbeit Keller's als eine Fälschung hinzustellen. Der Motivenbericht findet sich in den Verhandlungen ("Proceedings") der Section für allgemeine Medicin.

[1] So sagt z. B. Flinzer über die Impfverhältnisse von Chemnitz: "Unter den besser situirten Klassen der Gesellschaft, ja auch noch in den mittleren Schichten, wird die Schutzimpfung in einem Grade durchgeführt, der nur wenig zu wünschen übrig lässt. Die ungünstigsten Verhältnisse bieten die armen Klassen dar und hier treten wieder die in irgend einer Weise Verkümmerten besonders grell hervor." Auf Seite 16 seiner Arbeit konstatirt dann Flinzer wieder, dass jene Klassen, in denen die meisten Erkrankungen vorkommen, eben die ärmsten sind.

[2] Wittelhöfer's *Wiener medicinische Wochenschrift*, Jahrgang 1878, No. 38—42.

ACHTES KAPITEL.

IMPFSCHÄDEN.

Die Impfung soll nicht nur nutzlos, sondern geradezu schädlich sein. Vor Allem, weil durch dieselbe die Blattern in Permanenz erhalten werden sollen. Fernere Anklagen:

1) SCHÄDLICHKEIT DES IMPFAKTES

a) AN SICH. Schon zu Jenner's Zeiten behauptet. Sacco's Abwehr. Scrupulöse Auffassung der böhmischen Impfcommission. Präcisirung der Frage. Die Vaccination ist eine Operation, kann also ausnahmsweise zum Tode führen. Trotzdem können wohlthätige Operationen nicht abgeschafft werden.

b) SCHÄDLICHE PHYSIOLOGISCHE FOLGEN DER IMPFUNG:

α) *Deplacirung der Sterblichkeit nach Altersklassen.* Es starben vorher nur Kinder an Blattern, gegenwärtig aber, in Folge der Einimpfung des Virus, auch viele Erwachsene, was einen grösseren Verlust involvire. Falsch, weil ja alle hygienischen Massregeln nur so viel erreichen können, das Sterbealter möglichst hinauszuschieben.

β) *Deplacirung der Todesursachen:* gegenwärtig mehr exanthematische Krankheiten. Gregory's Vicariirungsprincip; führt consequenter Weise zum Fatalismus. Weeber's Behauptung, dass in England die Hautkrankheiten gegenwärtig viel mehr Kinder ergreifen, widerlegt. Nittinger's Behauptung von der in Schweden durch die Impfung hervorgerufenen Zunahme des Typhus widerlegt.

2) INOCULATION VON KRANKHEITEN DURCH DIE IMPFUNG.

Möglichkeit derselben bis in die jüngste Zeit geläugnet. Gegenwärtig allseitig zugegeben. Es handelt sich also darum, den Nutzen der Impfung gegen die durch dieselbe möglichen Schäden abzuwiegen. Bei dem gegenwärtigen Stande der Statistik weder das eine noch das andere möglich. Lösung dieser Aufgabe im zweiten Theile.

3) SONSTIGE BEHAUPTETE IMPFSCHÄDEN.

Theologische Bedenken. Die Impfung soll die physische, ja sogar die moralische Degenerirung des Menschengeschlechtes, Selbstmorde, Pessimismus u. s. w. verschuldet haben. Die Delirien von Nittinger und Verdé d l'Isle.

Die bisher angeführten Argumente der Impfgegner beschränken sich einerseits auf eine Kritik der Seitens der Impfvertheidigung angewendeten Methoden, andererseits auf Negation des behaupteten Nutzens der Impfung. Wir übergehen nun zu den positiven Angriffen der Impffeinde, zu jenen Thatsachen und Behauptungen, welche beweisen sollen, dass die Impfung nicht nur nutzlos, sondern geradezu schädlich sei. In diese Cathegorie wäre auch die Behauptung zu rechnen, dass die Blattern durch die Impfung aufrecht erhalten werden, dass diese Krankheit eigentlich schon längst ausgerottet wäre, würde sie nicht durch Einimpfung stets lebendig gehalten. Diese schwere Anklage reduzirt sich aber eigentlich auf die Morbiditätsfrage: ist die Einimpfung die Ursache der Blatternfälle, so werden Geimpfte häufiger an Blattern erkranken müssen als Ungeimpfte. Da wir auf die Beantwortung dieses direkten Beweises erst im zweiten Theile eingehen können, mag die Behandlung der oberwähnten Klage der Impfgegner vorläufig in Schwebe gelassen bleiben.

Die übrigen Anklagen auf durch die Impfung verursachte Schäden lassen sich unter drei Klassen bringen.

1. SCHÄDLICHKEIT DER IMPFOPERATION.

Gefährlichkeit des Impfaktes, und zwar *a*) weil die unmittelbaren Folgen der Impfoperation an sich und *b*) weil die physiologischen Nachwirkungen des Impfaktes gesundheitsschädlich wären.

a) Die Impfgegner behaupten, dass der Impfakt selbst, durchaus nicht so harmlos sei, wie er von den Impffreunden hingestellt wird, sondern dass die Einimpfung des

thierischen Giftes der Kuhpocke die Menschen krank mache und eventuell schon für sich allein den Tod herbeiführen könne. Auch Professor Bock in Leipzig ist der Ansicht, dass die Impfung durchaus nicht so ungefährlich sei, als die meisten Aerzte glauben.[1]

Schon zu Zeiten Jenner's wurden Klagen über die Schädlichkeit der Impfoperation laut. Die Impf-Vertheidigung setzte sich aber, in ihrem ersten Freudentaumel ob der grossen Entdeckung, hierüber gar leicht hinweg. So begnügte sich z. B. Sacco, die Ansicht, dass die Impfung Krankheit, ja selbst den Tod nach sich ziehen könne, damit ad absurdum zu führen, dass er sagte: gewiss könne die Impfung Niemanden unsterblich machen; auch Geimpfte müssten also erkranken und auch einmal sterben; daraus folge aber nicht, dass die Krankheit oder gar der Tod die Folge der Impfung wären. Stellenweise wurden aber solche Besorgnisse ernster genommen. So bildet z. B. die Rigorosität, mit welcher die böhmische Impfcommission solche Bedenken entgegennahm, einen ernsten Gegensatz zu jener sophistisch-sanguinischen Auffassung, der wir bei Sacco und so vielen seiner Zeitgenossen begegnen.

Es ist sehr bemerkenswerth, dass zu einer Zeit, wo die Impfung in ganz Europa als göttlicher Segen begrüsst wurde, eine zur Einführung und Verbreitung dieser segensreichen Erfindung niedergesetzte Behörde, alle Krankheitsfälle, welche während des Verlaufes der Vaccination zustossen, mit einem, eines Impfgegners würdigen Pessimismus beurtheilt. In vielen Fällen, wo die betreffenden Impfärzte, wenn sie einen ungünstigen Verlauf oder ausnahmsweise selbst einen Todesfall zu beklagen hatten, das Missgeschick auf andere Ursachen zurückzuführen versuchen, besteht die genannte Commission mit einer gewissen Hartnäckigkeit darauf, die Impfung, als die Ursache dieser Unfälle hinzustellen.[2] Auch Ross machte den medicinischen Schriftstellern schon vor mehreren Decennien den Vorwurf, dass die Mehrzahl derselben die Möglichkeit durch das Impfen hervorgerufener Krankheiten läugne, trotzdem doch das Vorkommen derselben "jedem Praktiker bekannt sei", und bekennt derselbe, dass das Bestreben, die Impfung je allgemeiner zu machen, unbewusst zu einer Unterschätzung oder einem Uebersehen der ungünstigen Indicien geführt habe.

Präcisiren wir aber doch, warum es sich eigentlich handelt.

Die Einimpfung der Kuhpocke in den menschlichen Körper fällt unter den Begriff der Operation und die Schmerz- und Fieber-Erscheinungen, welche derselben

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[1] Bock. "Vier Bücher vom gesunden und kranken Menschen." Leipzig, 1886. II. Buch.

[2] *Geschichte der Vaccination in Böhmen*, auf hohen Befehl herausgegeben von der in Schutzpockenimpfungs-Angelegenheiten niedergesetzten königlich medicinischen Polizei-Commission (auf dem Titelblatte das Siegel: "königlich böhmische Schutzpocken-Commission"). Prag, 1804. Siehe z. B. Seite 81, wo die Impfung sogar für Beinfrass verantwortlich gemacht wird. In einem anderen Falle, wo nach der Impfung Lungenentzündung mit tödtlichem Ausgange eintrat, bemerkt die Commission, dass dieser Todesfall von den Impfgegnern "nicht ohne Grund" der Wirkung des Kuhpocken-Impfstoffes zugeschrieben werden könnte. Man vergleiche ferner den eclatanten Fall des Kindes Haubner, Seite 133—152. Dasselbe wurde im Alter von drei Monaten geimpft. Es finden sich später am Körper Abscesse, von denen der Arzt glaubt, dass dieselben bereits früher entstanden seien; die Commission weist diese Voraussetzung zurück. Am sechsten Tage bereits grosse Abscesse mit Jauchblasen und brandigem Geruche; die Commission befürchtet, dass die Vaccination hieran nicht ganz unschuldig sei. Sechs Wochen nach der Impfung stirbt das Kind. Der verzweifelte Arzt beruhigt sich damit, dass die örtlichen Erscheinungen an der Impfstelle milde waren, die tödtlich verlaufenen Abscesse an den Extremitäten auftraten, also kaum von der Impfung herrühren mochten. Die Commission entgegnet, dass auch der syphilitische Virus gewöhnlich nicht an der Impfstelle zu wirken pflege; wer unbefangen urtheilt, müsse für die Abscesse die Impfung verantwortlich machen! Schliesslich wird ganz direkt erklärt, die Impfung könne in diesem Falle nicht von aller Schuld freigesprochen werden.

folgen, unter den Begriff der Krankheit. Es lässt sich also nicht läugnen, dass man die Menschen durch die Impfung krank mache. Es ist auch nicht ausgeschlossen, dass hie und da ein schwächliches Individuum dieser Operation zum Opfer fallen könnte. Gewissenhafte Vertheidiger der Impfung, wie Kussmaul oder Bohn, läugnen dies auch nicht. So sagt zum Beispiel Letzterer :[1] "Nimmermehr soll bestritten werden, dass die milde Kuhpockenerkrankung ausnahmsweise eine bedrohliche Richtung einschlagen kann, so dass sie zur ernsten Erkrankung wird, der hie und da ein Impfling zum Opfer fällt. Würde man aber die Forderung aufstellen, dass eine Operation, trotzdem sie in fast allen Fällen nicht bloss ohne alle schädlichen Folgen bleibt, sondern auch Gesundheit und Leben rettet, dennoch deshalb nicht unternommen werden dürfte, weil durch dieselbe unter 100,000 Fällen einmal der Tod eintreten könnte, so würde man damit jedwede Operation verdammen und unmöglich machen."

Kussmaul ("Zwanzig Briefe über Menschenpocken- und Kuhpocken-Impfung") sagt (Seite 89) hierüber sehr treffend : "Ohne Todesfälle geht es bei den leichtesten Verletzungen und den geringfügigsten Krankheiten nicht ab. Ein Nadelstich, eine Blutegelwunde, ein einziger Holzsplitter im Fleisch, der Umlauf am Finger, können zum Tode der vorher gesundesten, kräftigsten Personen führen. Rothlauf, Brand, Pyämie (Eitervergiftung des Blutes), Starrkrampf sah man zu ganz unbedeutenden Verletzungen sich gesellen. So wird auch die Vaccination zuweilen zum Tode führen können. Handelt es sich doch um kleine Wunden, in die ähnlich wie beim Bienenstich, eine giftige Substanz gebracht wird, das Kuhpockengift nämlich, das nach einigen Tagen die Kuhpockenkrankheit hervorruft, an welcher dann der ganze Organismus theilnimmt."....."Man ersieht hieraus, dass die Kuhpockenkrankheit nicht immer eine ganz leichte und unbedeutende Krankheit darstellt. Sie erregt zuweilen heftigere Zufälle und, ich läugne es nicht, sie führt sogar ausnahmsweise den Tod während ihres Verlaufes herbei. Es wäre Unrecht, dem Publikum, das bei dieser Frage so wesentlich interessirt ist, die Wahrheit zu verschweigen und die möglichen üblen Folgen der Impfung geringer hinzustellen als sie wirklich sind ; was mich betrifft, so werde ich rückhaltslos bei der vollen Wahrheit verbleiben. Aber auf der anderen Seite ist es unverantwortlich vor Gott und den Menschen, die Gefahren der Impfung auf's Aergste zu übertreiben und ihre grossen Vortheile mit fanatischer Unvernunft ganz abzuläugnen."

Bei consequenter Anwendung des impfgegnerischen Princips wäre eigentlich auch alles Medicamentiren zu verbieten. Auch die Einfliessung eines Medicamentes repräsentirt einen gewaltsamen Eingriff in den organischen Haushalt der Natur. Die wirksamsten Medicamente, wie Atropin, Morphin, Cocain, Chloralhydrat, Digitalis etc., sind entschiedene Gifte. Vernünftigerweise wird es sich also bloss um die Abwägung der gegenseitigen Vor- und Nachtheile eines Medicamentes oder einer Operation handeln können. Die diesbezügliche Klage der Impfgegner wäre also nur dann als berechtigt zu betrachten, wenn man beweisen könnte, dass der Impfakt selbst mehr Menschen das Leben kostet als rettet, eine Behauptung, welche selbst die Impfgegner nicht aufstellen

b) SCHÄDLICHE PHYSIOLOGISCHE FOLGEN DER IMPFUNG.

Durch Einführung der Kuhpocken-Impfung soll die Sterblichkeit nicht vermindert, sondern vergrössert, beziehentlich verschoben werden, und zwar soll diese Deplacirung nach zwei Richtungen erfolgt sein : *a*) einerseits sollen im vorigen und zu Beginn dieses Jahrhunderts nur Kinder an Blattern gestorben sein, während in dem Maasse, als die aus Kindern bestehende geimpfte Bevölkerung heranwuchs, die Blattern auch stets

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[1] Bohn. Handbuch. Seite 340.

höhere Altersklassen ergriffen hätten, wodurch der Menschheit grösserer Schaden zugefügt wurde; überdies aber soll β) die Sterblichkeit nur den Namen gewechselt haben, indem der geimpfte Theil der Menscheit statt an Blattern, gegenwärtig an Masern und Scharlach stirbt, und sollen diese exanthematischen Krankheiten gegenwärtig auch mehr Menschenleben wegraffen als die Blattern, an deren Stelle dieselben getreten.

α) DEPLACIRUNG DER STERBLICHKEIT NACH ALTERSKLASSEN.

Carnot behauptete (*Gazette médicale*, 1839), dass in Paris die Anzahl der Todesfälle unter den 20—30jährigen im Laufe der letzten vier Decennien durch die Impfung bedeutend gesteigert worden sein soll. Ihm schlossen sich Bayard (*Gazette des Hôpitaux*, 1853, 11. Feb.) u. A. an.

Selbst für den Fall, als dieses Argument ein berechtigtes wäre, ist es für unsern Zweck nicht nöthig, demselben näher zu treten, da diese Deplacirung, falls sie wirklich besteht, schliesslich doch in einer Steigerung der Morbidität und Mortalität ihren Ausdruck finden müsste, demnach unter diesen Gesichtspunkten ohnehin in Behandlung käme. Der Hinweis darauf, dass die Blattern dadurch, dass sie gegenwärtig höhere Altersklassen ergreifen, der Menschheit grösseren Schaden zufügen als früher, wo angeblich bloss Kinder durch dieselben hinweggerafft wurden, darf nicht als zutreffend betrachtet werden. Da alle Menschen doch einmal sterben müssen, so lässt sich, von einem gewissen Standpunkte aus, der Zweck aller hygienischen Massregeln als eine Deplacirung der Sterblichkeit vom Kindesalter auf je höhere Altersklassen auffassen. Nach dem Gedankengange der Impfgegner müsste man jenen Zustand für den beneidenswerthesten halten, wo alle Kinder bereits in den ersten Tagen sterben, da doch durch gute Ernährung, sorgfältige Pflege u. s. w. schliesslich doch nicht mehr erreicht werden kann, als dass dieselben erst im Alter von 60—70 Jahren sterben. Im Geiste der obigen Deduction wäre dies freilich als ein grosser nationalökonomischer Verlust zu beklagen. Ferner hat Lotz, wie mir scheint sehr richtig, auf eine zweite Erklärung der Thatsache hingewiesen, dass gegenwärtig mehr Erwachsene den Pocken erliegen. Gegenwärtig besteht nämlich die Mehrzahl der Geschützten aus Geimpften, während im vorigen Jahrhundert die Mehrzahl derselben aus Geblatterten bestand. Die heftige Pockenerkrankung ist nun, wenn auch ein sehr gefährliches, so doch, falls glücklich überstanden, gewiss wirksameres Schutzmittel als die Vaccination.

Das Argument von der

β) DEPLACIRUNG DER TODESURSACHEN

leidet an ähnlichen Schwächen, überdies daran, gar nicht bewiesen werden zu können.

GREGORY. — Zu einer Zeit, wo der Impfzwang in England noch nicht verbreitet war, hat Gregory[1] die Theorie von der Vicariirung der exanthematischen Krankheiten aufgestellt, wonach nämlich die Gesammtzahl der an Pocken, Masern, Scharlach und Keuchhusten Verstorbenen sich stets ungefähr auf der gleichen Höhe erhalten und innerhalb dieses Budgets bloss ein Virement für die einzelnen Titel der Todesursachen stattfinden soll.

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[1] Gregory. "Vorlesungen über Ausschlagsfieber." Uebersetzt von Helft. Leipzig, 1845. Casslet, früher Berichterstatter der Academie de médecine über die Impffrage, hat im Jahre 1849 die Behauptung aufgestellt, dass die Blattern ein nothwendiges Reinigungsfieber seien, und Bayard (l. c.) findet, dass seit Einführung der Impfung der Typhus stark zugenommen habe— eine Ansicht, die in Deutschland in Nittinger und Oidtmann enragirte Verfechter fand. Farr bemerkte hierauf mehr witzig als erschöpfend, dass es wahr sei, dass gegenwärtig andere Infectionskrankheiten häufiger geworden seien, weil Kinder, die von Blattern hinweggerafft wurden, nicht mehr an einer anderen Krankheit sterben können.

Die von Gregory zur Bekräftigung seiner diesbezüglichen Anschauung angeführten statistischen Daten sind aber durchaus nicht zwingende. Die Frage, ob die Zunahme anderer Todesursachen eine *Folge* der durch die Impfung abnehmenden Blattern sei, ist übrigens gar nicht zu lösen. Bei dem Dunkel, in welches die Aetiologie der Krankheiten, namentlich aber der infectiösen, gehüllt ist, wird es als ein sehr bedenklicher Schluss betrachtet werden müssen, die unbekannte Ursache des häufigeren Auftretens der Krankheiten A, B, C, D in dem Nachlassen der Krankheit E finden zu wollen! Eine consequente Anwendung des Vicariirungsprincips auf die Sanitätspolizei würde zum Aufgeben aller prophylaktischen, ja selbst aller therapeutischen Massregeln, demnach zu einem ausgesprochenen Fatalismus führen müssen, während auf der anderen Seite das Factum, dass es denn doch gelungen sei, ein Mittel zur Unterdrückung der Blattern zu finden, nur dazu anspornen kann, ein gleiches Mittel auch für die Unterdrückung der übrigen Krankheiten zu suchen.

WEEBER. — Gregory hat durch seine Theorie von der Vicariirung der exanthematischen Krankheiten durchaus nicht die Impfung für das Auftreten anderer Exantheme verantwortlich machen wollen. Nicht so die Impfgegner. So hat z. B. Weeber in den Verhandlungen der deutschen Impfcommission i. J. 1884 statistische Daten angeführt, denen zu Folge im Laufe der letzten drei Decennien sich in England die Menge der an Hautkrankheiten verstorbenen Kinder verdoppelt, der an Scropheln verstorbenen verdreifacht haben soll, während die Syphilisfälle eine noch grössere Steigerung aufweisen sollten.

Ich weiss nicht, woher Weeber diese Daten schöpfte. Aus den officiellen Berichten des Registrar of Births and Deaths[1] lässt sich gerade Entgegengesetztes herauslesen, nämlich:

es sind verstorben von je 100,000 Einwohnern

	im Allgemeinen			im Alter von 0—5 Jahren		
	1851—60,	1861—70,	1871—80,	1851—60,	1861—70,	1871—80,
an Blattern	221	163	236	1034	654	527
" Masern	412	440	378	2798	2998	2568
" Scharlach	876	972	716	4191	4624	3489
" Diphtherie	109	185	121	431	767	472
" Typhus und Fieber	908	885	484	1417	1248	651
" Krebs	317	387	473	22	13	13
" Phthysis	2679	2475	2116	1305	968	767

Es ergibt sich also, namentlich bei den Kindern, eine Abnahme aller verzeichneten Krankheiten; nur bei Diptherie findet eine geringe Zunahme statt, für die aber die Impfung verantwortlich zu machen noch Niemand beigefallen ist. Das Gleiche gilt für die Gesammtbevölkerung, nur dass hier bei den Erwachsenen eine bedeutende Zunahme des Krebses zu konstatiren ist.

NITTINGER. — Um zu beweisen, dass seit Einführung der Impfung der Typhus an Stelle der Blattern getreten sei, führt Nittinger das Factum an, dass in Schweden starben von 1749—1758 (*vor* Einführung der Impfung) an Blattern 71,251, an Typhus 39,412, von 1809—1818 aber (*nach* Jenner) an Blattern zwar nur 6984, hingegen an Typhus **19,731**!

Die Zunahme der Typhusfälle ist wirklich eine überraschende und es sollte uns deshalb nicht wundern, wenn solche Daten auf die meisten Leser den beabsichtigten Eindruck einer durch die Impfung hervorgerufenen Deplacirung der Todesursachen hervorbrächten. Wir wollen nun diesen klassischen Fall als logisch-statistisches Exercitium benützen, um zu zeigen, wie grosse Vorsicht solchen Zahlenbeweisen gegenüber geboten ist. Bei der nachfolgenden Prüfung des, auf den ersten Anschein

[1] Supplement to the 45th Annual Report of the Registrar General of Births, Deaths and Marriages, Seite CXII. bis CXVI.

so gewichtigen Nittinger'schen Beweises zerfällt derselbe in Nichts, und zwar aus folgenden Gründen:

1. In Schweden wird seit 1801 geimpft; die gesetzliche Einführung der Impfung erfolgte aber erst 1816. Man dürfte also billigerweise nur das Jahr 1801, noch richtiger 1816, als Scheidepunkt wählen. Nittinger wählte merkwürdigerweise das Jahr 1809. Dieses eigenthümliche Verfahren wird begreiflich, wenn man erfährt, dass dieses Jahr das vielleicht seit Jahrhunderten grösste Typhusjahr Schwedens repräsentirt! Es starben nämlich in diesem einen Jahre 21,171 an Typhus (in den folgenden neun Jahren jährlich 6–9000).

2. Nittinger vergleicht nicht die Periode vor und nach Einführung des Impfzwanges, sondern hebt je zwei Jahrzehnte heraus. Wohlweislich wählte er nun aus der prävaccinatorischen Periode jenes Jahrzehnt, in welchem die wenigsten Typhusfälle vorfielen, hingegen aus der vaccinatorischen jenes, wo die meisten. Ein Opponent könnte mit demselben Rechte die nachfolgenden Perioden wählen:

1765—74 (vor der Einführung) Blattern 62,990, Typhus 73,718; 1861—70 (nach der Einführung) Blattern 8676, Typhus 22,714.

Hier wären also nicht nur Blattern, sondern auch Typhus auffällig vermindert.

3. Das Hervorheben ausgesuchter Perioden ist immer als verdächtig zu betrachten. Eine loyale statistische Untersuchung hätte die ganze Zeit vor und nach Einführung der Impfung miteinander verglichen. Eine solche Vergleichung würde — man vergleiche die schwedische Tabelle am Ende des dritten Kapitels — beweisen, dass die Typhusfälle *vor* Einführung der Zwangsimpfung häufiger waren.

4. Auch in diesem Falle aber könnte die prävaccinatorische Zeit nicht hinter das Jahr 1774 zurückgehen (wie dies Nittinger thut), weil bis dahin in der Rubrik der Blattern *auch die Masern enthalten waren.*

5. Die ganze Vergleichung ist aber überdies deshalb unmöglich, weil in der schwedischen Statistik die Bedeutung der Rubrik "Typhus" fortwährend wechselte und dieselbe stets noch eine Reihe anderer Krankheiten in sich schloss. Nur für die Zeit von 1861 bis 1880 ist unter dieser Rubrik ausschliesslich Typhus zu verstehen. Von 1881 bis auf den heutigen Tag wird in Schweden auch Nephritis zu dieser Rubrik gerechnet; vom Jahre 1774 bis 1801 umfasste diese Rubrik aber auch noch *alle* Fieber und Wechselfieber; von 1812 ab, also durch achtzehn Jahre, sogar *Scharlach* und *Masern*, die erst im Jahre 1821 wieder ausgeschieden wurden!

6. Angenommen, dass alle diese Schwierigkeiten nicht bestünden, angenommen, dass nach Einführung des Impfzwanges in Schweden wirklich mehr Leute an Typhus gestorben wären als in der prävaccinatorischen Zeit — was durchaus nicht der Fall ist — wäre noch immer nicht erwiesen, dass dieser Umschwung durch die Impfung hervorgebracht wurde. Es konnten in der postvaccinatorischen Zeit sehr wohl ganz andere Ursachen auftreten, welche die Verbreitung der zweiten Krankheit hervorriefen. Wenn z. B. in der postvaccinatorischen Periode irgend ein Staat die Beaufsichtigung der öffentlichen Häuser aufhöbe, so würde dies unbedingt eine Vermehrung der Syphilisfälle nach sich ziehen. Die Impfgegner würden dann beweisen, dass unter dem Regime des "Impfsegens" viel mehr Syphilisfälle vorkommen als vor der Entdeckung Jenner's.

Wie man also sieht, steht die Behauptung von der durch die Impfung hervorgerufenen Deplacirung der Krankheiten auf sehr schwachen Füssen und lässt sich dieselbe auf den bisher betretenen Wegen eigentlich gar nicht beweisen.

2. INOCULATION VON KRANKHEITEN DURCH DIE IMPFUNG. IMPF-SYPHILIS.

Die Beantwortung der Frage, ob durch die Impfung fremde Krankheiten inoculirt werden können, bietet, wenn die Antwort eine präcise sein soll, die grössten logischen Schwierigkeiten. Ist es schon im Allgemeinen eigentlich unmöglich, zu beweisen, das

ein Y wirklich die Wirkung des vorhergegangenen X sei; haben wir selbst dort, wo es möglich ist, eine Erscheinung auf ihre allereinfachsten Elemente zu reduciren, streng genommen doch nie mehr vor uns, als dass ein Y auf X zu folgen pflege, durchaus aber nicht, dass das Y die Wirkung des X sei, und verwandelt sich daher schon bei den allereinfachsten Experimenten das Propterea in ein blosses Postea: um wie viel schwieriger muss sich die Beantwortung dieser Frage bei einem lebenden Organismus gestalten, wo eine Unzahl, zum Theile unbekannter, Kräfte mitwirkt, wo ferner, wie bei der Impfung, zwischen der vermeintlichen Ursache und deren Wirkung auch noch ein längerer Zeitraum, oft viele Jahre, verstreichen, während dessen sowohl diese Kräfte, als auch eine Reihe von äusseren Einwirkungen die eigentliche Ursache der späteren Erkrankung sein konnten. Wenn man überlegt, wie der Beweisgang beschaffen sein müsste, der klar darzulegen hätte, dass, weil bei einem geimpften Kinde später, oft erst nach Jahren, Rachitis, Syphilis und dergleichen ausgebrochen, die Ursache dieser Erkrankung in der Impfung zu suchen sei: wird man einsehen, wie schwierig, ja — theoretisch genommen — beinahe unmöglich, eine solche Beweisführung ist. Die Schwierigkeit besteht aber auch noch bei unmittelbarer Aufeinanderfolge von Impfung und Krankheit. In Fällen, wo bei geimpften Kindern unmittelbar nach der Impfung Syphilis ausbrach, haben hervorragende medicinische Autoritäten die Ursächlichkeit der Impfung dadurch in Abrede gestellt, dass sie auf die Möglichkeit einer latenten Syphilis hinwiesen, die durch den Hautreiz nur zum Durchbruch kam. Aehnliches gilt für Srophulosis. "Wird ein Kind nach der Impfung scrophulös" — sagt ein angesehener Professor der Wiener Schule — "so befürchte ich nicht, Scropheln eingeimpft zu haben, sondern bin überzeugt, dass ich ein scrophulöses Kind geimpft habe."

Auf dem gewöhnlichen Wege der statistischen Beweisführung dürfte man in dieser Frage wohl nie zu einem positiven Resultate gelangen. Im besten Falle liesse sich eruiren, ob in gutimpfenden Ländern die Anzahl der als überimpft betrachteten Krankheiten gegen das Vorkommen in schlechtimpfenden überwiege, wobei es aber noch immer vollkommen offen bleibt, ob diese Abweichungen nicht durch klimatische, hygienische, culturelle und andere Verhältnisse hervorgerufen seien. Auf diesem Wege wird man also weder durch eine auf das ganze Leben sich erstreckende Beobachtung der Individuen, noch durch die statistische Massenbeobachtung ganzer Bevölkerungen, zu einem positiven Beweise gelangen, geschweige dass ein solcher Beweis die Möglichkeit einer zahlenmässigen Abwägung der Vor- und Nachtheile der Impfung gewährte.

Es scheint mir aber trotzdem nicht ausgeschlossen, dass die Statistik im Stande sei, einen derartigen positiven Beweis zu liefern, doch bedarf es hierzu einer neuen Methode. Hierüber im zweiten Theile meines Referates Näheres. Hier möge vorläufig der gegenwärtige Stand der Streitfrage präcisirt werden.

Bei dem heutigen Stande der Wissenschaft wird die Behauptung, dass durch Einimpfung organischer Säfte in einen fremden Organismus auch Krankheiten überimpft werden könnten, keinem Zweifel unterliegen. Nicht so stand es um diese Frage zu Beginn des Jahrhunderts, ja sogar bis zum Anbruche des letzten Decenniums. So erklärte noch im Jahre 1857 Simon im englischen Blaubuch[1]: "Die Möglichkeit, dass durch die Impfung Krankheiten hervorgerufen würden, nehme in der ärztlichen Lehre und in der ärztlichen Conversation ungefähr denselben Platz ein, als die Annahme von Krankheiten, die durch Hexerei und bösen Blick hervorgerufen werden." Gleicherweise fiel noch im Jahre 1864 in der Verhandlung des Wiener Doktoren-

[1] Siehe englisches Blaubuch, Seite 43: "Diseases produced by vaccination occupy in medical teaching and medical conversation about the same space as diseases produced by witchcraft and the evil eye."

Collegiums Seitens eines angesehenen Arztes die Bemerkung, es sei "Aberglaube", dass mit der Impfung auch Krankheiten eingeimpft würden.[1] Seaton, diese anerkannte Autorität auf dem Gebiete der Vaccination, erklärte noch im Jahre 1873, dass er an eine durch Impfung übertragene Syphilis nicht glauben könne, und noch im Jahre 1875 läugnete Farr, zum Mindesten für zymotische Krankheiten, dass diese durch die Impfung übertragen würden.[2]

Die Impfgegner hingegen haben von jeher behauptet, dass mit der Vaccination auch Krankheiten mitübergeimpft würden. Wenn man bedenkt, dass unter den Impfgegnern sich nicht nur Aerzte, sondern auch Familienväter befinden, die Zeugen davon waren, dass in ihrer Umgebung, oft bei ihren eigenen Kindern, nach der Impfung Krankheiten, selbst Syphilis, zum Ausbruch kamen, ja letztere sogar auf Ammen und Mütter übertragen wurde, so wird man den Eifer begreifen, mit welchem die Impfgegner alle Fälle von Impf-Syphilis zu sammeln und zur grösstmöglichen Publicität zu bringen bestrebt waren. Begreiflich ist auch die grosse Aufregung, welche sich der Betroffenen bemächtigen musste, wenn die Wissenschaft ihre traurigen Erfahrungen für leeres Gewäsche erklärte und ignorirte und diese Behauptungen die längste Zeit als Entstellungen und Märchen hinstellte. Die Zeiten haben sich aber geändert und heute anerkennt die medicinische Wissenschaft in der allerernstesten Form die Existenz der Impf-Syphilis, wie auch die des Impf-Rothlaufs. Bohn[3] hat eine lange Reihe der Fälle von Impf-Syphilis zusammengestellt; man findet daselbst auch jene, besonders bedeutsamen Fälle aufgeführt, wo die gegen den betreffenden Impfarzt eingeleitete Untersuchung ohne Bestrafung desselben endete, weil kein Kunstfehler vorlag. Hieraus ist also zu ersehen, dass es kein entschiedenes Merkmal für die Erkennung der Syphilis am Stammimpfling gibt, und dass demnach die Uebertragung von Syphilis trotz aller pflichtgemässen Obsorge erfolgen kann.

Die Pariser Akademie der Medicin wurde im Jahre 1856 durch Depaul's sensationelle Behauptung aus ihrer Ruhe geschreckt, wonach Syphilis durch Vaccination durchaus nichts Seltenes sei. In Folge dieser Anregung entbrannte im Schoosse dieser gelehrten Körperschaft ein zehnjähriger Krieg um die Frage der Existenz oder Nichtexistenz einer Impfsyphilis, bis im Jahre 1866 diese Debatte damit ihren Abschluss fand, dass die Möglichkeit der Uebertragung von Syphilis durch die Impfung *einstimmig* angenommen wurde.

Auch Kussmaul[4] anerkannte schon im Jahre 1870 die Thatsache der Uebertragbarkeit der Syphilis durch die Impfung.

Unter solchen Verhältnissen fällt der Schwerpunkt der Frage darauf, wie gross eigentlich jene Schädigung sei, welche durch die Einimpfung von Krankheiten der betreffenden Bevölkerung zugefügt wird. Wenn man dieser Belastung der Impfung gegenüber den Nutzen, welchen dieselbe gewährt, in's Haben einstellt, liesse sich solcher Weise, wie dies Kolb vorgeschlagen, die Bilanz der Impftheorie ziehen, ob nämlich deren Nutzen oder Schaden überwiegend sei. Diese Bilanz zu ziehen ist, wie bereits erwähnt, bei dem gegenwärtigen Stande der Statistik unmöglich. Wir haben aus den vorhergehenden Abschnitten ersehen, dass es unmöglich ist, den Nutzen der Impfung, d. h. die geringere Morbidität, oder geringere Lethalität der Geimpften festzusetzen. Andererseits lässt sich, wie eben erwähnt, auch die Grösse des durch die

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[1] Sitzung des medicinischen Doktoren-Collegiums, 11. Juni 1864. Siehe *Wittelshöfer's Wiener medicinische Wochenschrift*, Seite 456.

[2] Farr. Letter in the Supplement to the *Annual Report of the Registrar General*. Seite 15.

[3] Bohn. Handbuch. Seite 306 ff. Vergleiche auch Heid. "Zur Frage der Uebertragung der Syphilis durch die Schutzpockenimpfung." Stuttgart, 1867.

[4] Kussmaul. "Zwanzig Briefe." Seite 96.

Impfung verursachten eventuellen Schadens nicht constatiren. Ich hoffe, dass die im zweiten Theile zu besprechende neue Methode uns nach beiden Richtungen hin den Schlüssel zur Lösung bieten werde.

3. WEITERE BEHAUPTETE IMPFSCHÄDEN.

Im dem Bisherigen haben wir versucht, die in den Schriften der Impfgegner zerstreuten Klagen wegen Impfschädigung ihrem Inhalte nach unter einige Hauptgruppen zu bringen. Die Reihe der Seitens des Impfangriffes in's Treffen geführten Anklagen ist hiemit aber noch lange nicht erschöpft. Von den theologischen Bedenken, wonach die Impfung eine vermessene "Einengung jenes Weges sei, den Gott die Menschheit führt, dass diese also nur Empörung gegen Gott sei",[1] können wir wohl getrost absehen; von diesem Standpunkte aus wäre die ganze Therapie und die ganze Hygienie, ja jede Vorsicht und Fürsorge im Leben eine Empörung gegen Gott und eine "Einengung seiner Wege". Wohl aber soll die Impfung noch eine Fülle von Unglück über die Menschheit gebracht haben. Die durchschnittliche Lebensdauer des Menschen soll im Laufe dieses Jahrhunderts abgenommen haben und die Ursache hiervon soll die Impfung sein. Nittinger[2] beschuldigt die Impfung, "die Statur, den Knochen- und Muskelbau verkümmert, die Körperformen abgeartet zu haben." Wenn die Militäraushebung eine grössere Anzahl von Untauglichen ergibt, so findet Nittinger, dass auch hieran die Impfung die Schuld trage. Ist sie es doch, der er die Zunahme der künstlichen Geburten in Württemberg, ja sogar die der Geisteskrankheiten zuschreibt. Selbst die Kinder im Mutterleibe schmachten unter den Folgen der Impfung und das häufigere Vorkommen von Doppelgliedern soll in den drei Stichen mit der Impflancette seine natürliche Erklärung finden. So reich die Phantasie Nittinger's im Gespenstersehen ist, wird er hierin von seinem Vorgänger Verdé de Lisle doch noch übertroffen: "Das Menschengeschlecht degenerirt und die Ursache hiervon ist die Impfung. Die Kurzsichtigkeit, Kahlköpfigkeit, die pessimistische Weltanschauung, die Selbstmorde, der Rückgang in der Wissenschaft, in der Malerei, in der Dichtkunst — all' dies ist Jenner's Werk."[3]

Es ist schwer, solchen Argumenten gegenüber ernst zu bleiben. Es kann auch nicht die Aufgabe der Wissenschaft sein, allen Ausgeburten einer impffreundlichen oder impfgegnerischen Phantasie, den Dilirien unkritischer Köpfe, durch Dick und Dünn zu folgen. Bevor Jemand auf die Anklagebank gerufen und über ihn ein ernstliches Gerichtsverfahren eröffnet wird, muss doch vor Allem wenigstens die Wahrscheinlichkeit eines Thatbestandes festgestellt sein. Man muss es also Jenen, welche gegen die Impfung so unglaubliche Anklagen erheben, überlassen, vorerst einige Beweise zu erbringen, durch welche zumindest die Wahrscheinlichkeit des Thatbestandes dargelegt wird; dann erst wird es an der Zeit sein, solche Anklagen ernst zu nehmen.

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[1] Siehe die Rede des Prälaten Moser in den Verhandlungen der württembergischen Kammer vom Jahre 1858.

[2] Siehe Nittinger. "Der Sieg wider die Impfung." Seite 211—235.

[3] M. Verdé de Lisle (de la dégénérescense physique et morale de l'espèce humaine l'éterminé par le vaccin; Paris 1855). "L'espèce humaine dégenère, aux puissantes races des siècles passés a succédé une génération petite, maigre, chétive, chauve, myope, dont le caractère est triste, l'imagination, sèche, l'esprit pauvre ..... Aprés Voltaire, après Beaumarchais.... le triste spectacle d'une foule de petits personnages qui ne peuvent élever leur presumption plus haut que la collaboration et la critique.... En musique, à défaut des Gluck, des Mozart, des Boieldieu.... le metier nous donne les nombreux arrangeurs. En peinture, aprés les Rubens, les Van Dyck.... il nous faut tomber sans transition de la puissance de Géricault à la patience de Meissonier."

ZWEITER THEIL.

NEUE BEITRÄGE ZUR FRAGE DES IMPFSCHUTZES.

NEUNTES KAPITEL.

DARLEGUNG DER ANGEWENDETEN METHODE.

Recapitulation der Hauptpunkte des Impfstreites. Die Argumentation der Impfvertheidigung lässt sich eigentlich in der Frage concentriren, ob Geimpfte seltener von Blattern befallen werden. Hierzu nöthig, den Impfzustand der Lebenden zu kennen; demnach Angelpunkt der ganzen Impfvertheidigung: *Das Problem der lebenden Gesammtheiten.* Darlegung desselben. Lösung durch Berechnung der relativen Intensität. — Drei Beweise für die Richtigkeit der Berechnungsweise: Empirischer Beweis. — Graphischer Beweis. — Algebraischer Beweis.

Recapituliren wir nun den ganzen Gang des Impfstreites, wie er sich auf dem Boden der Statistik vollzog, so finden wir, dass die Impfvertheidigung sich hierbei auf folgende drei Hauptpunkte concentrirte: auf das Argument der seit Einführung der Vaccination abgenommenen Pandemieen, auf jenes der geringeren Morbidität und schliesslich auf das der geringeren Mortalität, bez. Lethalität der Geimpften; dass ferner von den, Seitens der Impfgegner aufgeführten Argumenten die Anklage wegen Deplacirung der Mortalität als unberechtigt abzulehnen wäre, während die Thatsache, dass die Impfung an und für sich nicht als absolut gefahrlos zu betrachten sei, wie auch, dass durch dieselbe Krankheiten hervorgerufen, bez. überimpft werden könnten, gegenwärtig im Princip zwar zugegeben wird, die Bedeutung dieser Nachtheile aber, Seitens der Impfvertheidigung, als den durch die Vaccination gebotenen Vortheilen gegenüber verschwindend gering hingestellt wird, ohne dass aber eine ziffermässige Aufstellung dieser Bilanz möglich gewesen wäre.

Bei einer kritischen Beurtheilung des Vertheidigungs-Systems ergeben sich neben einigen entschieden behaupteten Positionen verschiedene Schwächen und Lücken, und zwar eben an den wichtigsten Punkten. So ermangelt 1) der Hinweis, dass seit Einführung der Impfung die Blattern abgenommen hätten (historischer Beweis) insolange des direkten Beweises, als nicht nachgewiesen wird, dass diese Abnahme wirklich durch die Impfung verursacht wurde. Der causale Zusammenhang zwischen der Abnahme der Blattern und Einführung der Vaccination ist eben nicht genügend hergestellt, die Möglichkeit, dass diese Abnahme anderen Ursachen zugeschrieben werden könnte, nicht vollkommen widerlegt. Dasselbe gilt, wenn auch nur in schwächerem Maasse, 2) von der Behauptung, dass in gut impfenden Staaten die Blattern seltener seien (geographischer Beweis). In den beiden genannten Fällen ist übrigens der ganze Beweisgang nur ein indirekter. Von den zwei direkten Beweisen, nämlich jenen der geringeren Morbidität und Mortalität der Geimpften, ist 3) der Beweis einer geringeren Morbidität der Geimpften in Folge des Problems der Stammgesammtheiten, nämlich der Unmöglichkeit, die Anzahl der geimpften und ungeimpften Lebenden (beziehentlich Gesunden) zu kennen, nicht herzustellen, während 4) der Beweis für die geringere Lethalität der Geimpften nicht für ganze Bevölkerungscomplexe, sondern nur für einzelne Klassen der Bevölkerung, nämlich für einige Heere und für Spitalsbevölkerungen durchgeführt ist; in beiden Fällen hat man es also nur mit ausgewählten Bruchtheilen der Bevölkerung zu thun, und zwar einerseits mit einer Auswahl nach Geschlecht und Alter (Soldaten), andererseits mit einem ganz zufällig zusammengesetzten Bevölkerungsgemengsel (Spitäler). Für ganze Bevölkerungscomplexe sind

solche Beweise nicht durchgeführt, da dieselben an der oben angeführten Schwierigkeit des Problems der lebenden Gesammtheiten scheitern.

Diesem Systeme der Vertheidigung gegenüber ist unter den Angriffen jenem das grösste Gewicht beizulegen, wonach selbst für den Fall, als die Morbidität und Mortalität der Geimpften als günstiger anerkannt würde, man darauf hinweist, dass *a*) die Geimpften unter allen Verhältnissen eine Auswahl der widerstandsfähigeren, die Ungeimpften eine Auswahl der schwächlicheren Elemente repräsentiren, also in Folge dieses Umstandes und nicht in Folge der Nichtimpfung häufiger erkranken und sterben. Dieses Argument entzieht eigentlich sämmtlichen auf die geringere Morbidität, Mortalität, oder Lethalität der Geimpften Bezug habenden statistischen Beweisführungen, auch wenn sie vollkommen wären, den Boden unter den Füssen und scheint den ganzen statistischen Beweis über den Nutzen der Impfung zu einem sterilen zu machen. Es kommt hierzu *b*) die Klage wegen der *Impfschäden*, das ist die Möglichkeit, dass die Impfung durch Ueberimpfung der Krankheiten mehr Schaden als Nutzen stifte. Die technischen Schwierigkeiten, welche das Problem der Stammgesammtheiten einer ziffermässigen Präcisirung des Impfnutzens entgegenstellt, scheinen für grössere Bevölkerungscomplexe unüberwindbar. Aber selbst wenn dies nicht der Fall wäre, träten einer Aufstellung der zwischen Nutzen und Schaden der Vaccination zu ziehenden Bilanz noch jene logischen Hindernisse in den Weg, welche es so schwierig machen, den Causalnexus zwischen einer in einer früheren Zeit vorgenommenen Impfung und einer in oft bedeutend späterer Zeit erfolgenden Erkrankung festzustellen.

Ich werde es nun versuchen, in Nachfolgendem einige neue Beiträge vorzuführen, die mir geeignet erscheinen, diese Schwierigkeiten zu lösen, oder doch ihrer Lösung um einen entschiedenen Schritt näher zu bringen. Als die Hauptschwierigkeit hat sich uns hiebei wiederholt das Problem der Stammgesammtheiten erwiesen, und ich will deshalb mit diesem den Beginn machen.

PROBLEM DER LEBENDEN GESAMMTHEITEN.

Dasselbe besteht, wie bereits erwähnt, darin, dass uns die Impfverhältnisse der Erkrankten oder Verstorbenen wohl zugänglich sind, die der Lebenden und Gesunden aber nicht. Nachdem nun aber der Morbiditäts-, bez. Mortalitäts-Coëfficient ein Bruch ist, in dessen Zähler die beobachteten Erkrankungs-, bez. Sterbefälle, in dessen Nenner aber die Gesammtzahl aller lebenden Geimpften oder Ungeimpften eingestellt werden muss, so ist es klar, dass man in solchen Fällen, wo bloss der Zähler des Bruches (die Anzahl der Erkrankungs-, bez. Todesfälle) bekannt ist, der Nenner (Gesammtheit der Lebenden) aber nicht, unmöglich einen solchen Coëfficienten berechnen kann. Diese Schwierigkeit gilt übrigens für eine Reihe statistischer, namentlich demologischer Fragen. Am nachdrücklichsten wird dieselbe aber in der Mortalitäts-Statistik betont. Es gilt daselbst als ein allgemeiner Grundsatz, dass ohne Kenntniss der in der Gesammtheit der Lebenden herrschenden Verhältnisse sich gar kein richtiger Schluss ziehen lasse.[1] Geht man aber der Entstehung dieses Axiomes nach, so findet man, dass der Ursprung derselben in den Mortalitätstabellen zu suchen ist. Es sind an zwei Jahrhunderte verstrichen, seit der berühmte Astronom Halley es zum erstenmal unternahm, das Maass der menschlichen Sterblichkeit zu erforschen, und zu diesem Behufe, auf Grund der ihm zugesandten Alterstatistik über die in Breslau Verstorbenen, eine Mortalitäts-Tabelle entwarf. Eine solche Tabelle hat den Zweck, anzuzeigen, wieviel aus einem Kreise gleichaltriger Lebender (also der 1, 2, 3,

[1] Wenn z. B. von 100 Geimpften 10 sterben, so ist die Mortalität der Geimpften $\frac{10}{100}$; bietet uns aber die Statistik nur den Zähler dieses Bruches, lässt sich aus diesem allein freilich Nichts aussagen.

u. s. f., bis Hundertjährigen) im Laufe eines Jahres sterben. Kennt man, wie dies gegenwärtig durch die Volkszählungen ermöglicht ist, die Anzahl der in jeder Altersklasse Lebenden, und andererseits, wie dies aus den Todtenlisten schon seit längerer Zeit möglich ist, die Anzahl der aus jeder Altersklasse Verstorbenen, so lässt sich diese Proportion aus den Daten feststellen. Halley hatte jedoch nur Todtenlisten zur Verfügung und seine Mortalitäts-Tabelle beruht einzig und allein auf den Altersverhältnissen der Verstorbenen. Sobald man diese Lücke in den Berechnungen, das Hypothetische in den Voraussetzungen Halley's, erkannte, musste seine Methode als unrichtig erkannt und verlassen werden.

Aehnliche Trugschlüsse wiederholen sich, wenn man z. B. die Sterblichkeit einzelner Berufsklassen untersucht. Es existirt hierüber eine grosse Literatur, die aber fast ausschliesslich auf der Zahl der den einzelnen Berufsarten angehörigen *Verstorbenen* basirt. Man kann bei Westergaard[1] des Näheren nachlesen, wie fast diese ganze Literatur aus obigem Grunde unbrauchbar ist, und es liegt übrigens auf der Hand, dass wenn unter den Verstorbenen x mal mehr Taglöhner als Aerzte zu finden waren, dies noch durchaus Nichts für die grössere Sterblichkeit der Taglöhner beweist, da ja auch im Kreise der Lebenden x mal mehr Taglöhner leben mögen als Aerzte. Die Berichte hervorragender Krankenhäuser, von Klinikern ersten Ranges redigirt, wimmeln aber noch immer von solchen Nachweisungen über die grössere oder geringere Sterblichkeit einzelner Berufsarten, namentlich bei Besprechung epidemischer Krankheiten. Auch diese Berechnungen sind sammt und sonders unbrauchbar, weil man die Anzahl der Stammgesammtheit, d. i. die der Lebenden, nicht kennt.

Eben dieses Problem der Stammgesammtheiten macht aber auch die Frage nach der Morbidität oder Mortalität der Geimpften und Ungeimpften scheitern. Man kennt nämlich die Anzahl der lebenden Geimpften und Ungeimpften nicht, geschweige dass man die Vertheilung der Geimpften und Ungeimpften nach Altersklassen, oder dass man die Anzahl der im Kreise je einer Altersklasse vorgekommenen Erkrankungsfälle genau kennen würde.

Man hat nun auf Grund solcher Betrachtungen die Forderung nach der Kenntniss der Anzahl der Lebenden, wenigstens für das Gebiet der Mortalitäts-Statistik, generalisirt. Um aber zu entscheiden, ob mit Recht oder Unrecht, ist es nothwendig, auf einige logische Grundbedingungen des statistischen Schliessens einzugehen. Es wurde auf diesem Wege an einer anderen Stelle[2] nachgewiesen, dass diese Generalisirung nicht statthaft war, und dass es Fragen gibt, bei denen von dem genannten Axiom Umgang genommen werden kann. Ich habe meinen diesbezüglichen Ausführungen nichts Wesentliches hinzuzufügen, und indem ich mich auf dieselben bloss berufe, kann ich es mir genügen lassen, hier nur soviel zu beweisen, dass die uns gegenwärtig beschäftigende Frage ebenfalls in die Reihe jener Fälle gehört, wo die Kenntniss der Gesammtheit der Lebenden nicht unbedingt nothwendig ist.

Würde es sich nur darum handeln, die *Häufigkeit* der Blatternfälle bei Geimpften und Ungeimpften zu constatiren, d. i. den Coëfficienten zu erkennen, dann wäre es natürlich unbedingt nothwendig, die Anzahl der im Kreise der Bevölkerung lebenden Geimpften und Ungeimpften zu beweisen. Wenn z. B. von 1000 Geimpften 100 an Blattern erkranken, und von 1000 Ungeimpften 200, so wäre der Morbiditätscoëfficient

[1] Westergaard. "Die Lehre von der Mortalität und Morbidität." Aus dem Dänischen. Jena, 1881.

[2] Siehe meine Mittheilungen in *Wittelshöfer's medicinischer Wochenschrift*, No. 14 bis 18 und No. 22 bis 24, 1886; auch als Separatabdruck erschienen. (Körösi. "Armuth und Todesursachen." Wien, 1886. Seidl & Sohn.) Man vergleiche auch die den Abschluss dieser Polemik bildende Erwiderung in No. 41 und 42 von *Wittelshöfer's medicinischer Wochenschrift* desselben Jahrganges.

für die Geimpften $\frac{100}{1000}$, für die Ungeimpften $\frac{200}{1000}$, wobei immer vorausgesetzt werden muss, dass die Gesammtheit der Lebenden bereits eine solche Auswahl von Individuen repräsentirt, die — mit Ausnahme der Impfung — bezüglich aller Faktoren, von denen wir voraussetzen, dass diese auf Blatternerkrankung von Einfluss sind (z. B. das Alter), keine Unterschiede aufweisen. Dieser Coëfficient gibt uns also an, wie *häufig* die Blatternfälle bei Geimpften und Ungeimpften vorgefallen. Nun bietet aber die Kenntniss der Häufigkeit der Fälle eben gar keinen Anhaltspunkt zur Beurtheilung der Schutzkraft der Impfung: sind es doch eben die Impfgegner, die fortwährend betonen, dass die Gesammtheit der Ungeimpften eine Gesammtheit der Schwächeren repräsentire; dass also deshalb nicht bloss die Blattern, sondern auch die Tuberculose und alle anderen Todesursachen aus diesem Kreise mehr Opfer fordern, als aus dem der gesünderen und wohlhabenderen, mit einem Worte widerstandsfähigeren Geimpften; dass es demnach für die Frage des Impfschutzes noch gar nichts bedeute, wenn selbst bewiesen werden könnte, dass die Blatternfälle bei den Ungeimpften *häufiger* seien: die *wirkliche Ursache* der Sterblichkeit sei nicht in der Impfung, sondern in den übrigen Lebensverhältnissen zu suchen. Um nun auf diesen Einwand zu antworten, müsste man vor Allem die normale Morbidität oder Mortalität der Ungeimpften gegenüber der normalen Morbidität oder Mortalität der Geimpften festsetzen, und dann erst untersuchen, ob die specielle Morbidität oder Mortalität, nämlich die an Blattern, sich relativ höher oder niedriger stelle.[1] Man übergeht also hier, und zwar mit gutem Grunde, von der gewöhnlichen

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[1] Da es von Wichtigkeit ist, klar einzusehen, dass eine Ab- oder Zunahme des Coëfficienten noch durchaus nicht identisch mit der Ab- oder Zunahme der wirkenden Ursache ist, dürfte es nicht überflüssig sein, sich die Sache an Beispielen klar zu machen. Man nehme z. B. das Factum von der Zunahme der städtischen Bevölkerungen, welches man etwa dem Einflusse der Eisenbahnen, den gesteigerten culturellen Bedürfnissen etc. zuschriebe. Es sei nun in einem Lande die städtische Bevölkerung in der ersten Periode von 1 Million auf $1\frac{1}{2}$, in der zweiten von $1\frac{1}{2}$ auf $2\frac{1}{2}$ Millionen angewachsen, der Zuwachs-Coëfficient betrage also für die erste Periode 50 pCt., für die zweite 66 pCt. Bei nur oberflächlicher Betrachtung der Ergebnisse wäre man nun leicht geneigt, zu behaupten, diese Ziffern bewiesen die stets steigende Attractionskraft der Städte: das Steigen des Coëfficienten zeige dies ganz klar. Wie aber, wenn in demselben Staate die Landbevölkerung in der ersten Periode von 1 Million auf 2 und in der nächsten von 2 auf 4 Millionen gestiegen wäre? Man sieht, dass in diesem Falle nicht die für die Zunahme der Städte günstigen *speciellen* Factoren, sondern die für die Volkszunahme des ganzen Staates wirksamen *allgemeinen* Factoren die Ursache waren, dass auch in den Städten die Bevölkerung zunahm. Wenn aber diese allgemeinen Factoren eine Zunahme von 100 pCt., die speciell für die Städte wirksamen Factoren aber nur eine Zunahme von 66 pCt. aufweisen, so wird man zu dem ganz entgegengesetzten Schlusse gezwungen, dass die letzteren Factoren eigentlich einen hindernden Einfluss ausübten; für den Fall, als diese entfallen wären, hätten nämlich die Städte ebenfalls eine Zunahme von 100 pCt. aufweisen müssen. Acceptirt man jenes Maass des Anwachsens als Einheit, d. i. als Maassstab, das auf dem flachen Lande bestand, so wird man durch Vergleichung des Provinz-Coëfficienten (=100) mit jenem der städtischen (=66 pCt.) zu den Ergebnissen gelangen, dass die normale Entwicklung des Bevölkerungszuwachses durch die Factoren des Stadtlebens um 33 pCt. herabgedrückt wurde. Zu diesem Ergebnisse gelangten wir aber, wohlgemerkt, auf Grund der Coëfficientenrechnung, also noch mit Hülfe der Gesammtheit der Lebenden, welche den Nenner jedes Coëfficienten bildet. Vorläufig wolle man nur dies festhalten. Man wird später sehen, dass in gewissen Fällen — so auch bei der Impffrage — bei der Vergleichung zweier Coëfficienten ein und dieselbe Gesammtheit der Lebenden im Nenner der zwei Brüche vorkommt, demnach entfallen kann. Dieses Ausfallen der Lebendenzahl ist also eine blosse Folge dessen, dass man logisch *gezwungen* ist, die landläufigen Folgerungen aus *einem* Einzelcoëfficienten aufzugeben und wäre es deshalb unrichtig, den Vorgang so aufzufassen, als ob die Gesammtheit der Lebenden nur deshalb, und zwar in künstlicher und unberechtigter Weise, aus dem Wege geschafft worden wäre, weil man den Knoten, der nicht zu lösen war, habe zerhauen müssen.

Frage nach der Häufigkeit der Fälle auf jene nach der *relativen* Häufigkeit, die im weiteren Verlaufe als "relative Intensität" bezeichnet werden soll. Diese relative Intensität ist also eine zweite Relation, die aus der ursprünglichen Relation, nämlich jener des gewöhnlichen Coëfficienten, herausgerechnet werden kann. Der grosse Vortheil, der sich bei Anwendung der neuen Relation bietet, liegt aber darin, dass dieselbe zwar aus dem Coëfficienten, der auch die Anzahl der Lebenden enthält, berechnet werden *kann*, aber nicht unbedingt aus diesem berechnet werden *muss*, sondern auch ohne Kenntniss dieses Coëfficienten, und *ohne Kenntniss der Anzahl der Lebenden*, constatirt werden kann. Es lässt sich dies im Allgemeinen, sowohl auf algebraischem, als auch auf graphischem Wege beweisen, kann aber auch empirisch durch einfache Nachrechnung der sehr elementaren Rechnungsoperationen eingesehen werden. Ich will, mit Rücksicht auf das Misstrauen, das man von einigen Seiten dieser Methode entgegenbrachte, welche auf das Fundamentaldatum, nämlich auf das der Lebendenzahl verzichten zu können erklärte, alle diese Beweise aufführen, wogegen man mir zu Gute halten wolle, wenn ich hie und da gezwungen sein werde, in Wiederholungen zu verfallen. Es möge vor Allem die Richtigkeit der anzuwendenden neuen Methode auf dem letztgenannten, leichtest verständlichen Wege nachgewiesen werden.

EMPIRISCHER BEWEIS.

Stellen wir uns hier vor Allem auf den, keinerlei Einwänden ausgesetzten, Standpunkt, dass man die Anzahl der Lebenden kenne. Nehmen wir an, dass im Orte A lauter Geimpfte, im Orte B lauter Ungeimpfte wohnen, und zwar soll es in A 10,000, in B 5,000 Personen geben. Wir nehmen ferner an, dass diese Personen hinsichtlich aller Beziehungen, welche auf die Blattern-Erkrankung von Einfluss sein können, und welche statistisch erfassbar sind, vollkommen gleich geartet wären, dass sie also desselben Geschlechtes, desselben Alters etc. sind, mit Ausnahme eines Umstandes: nämlich, dass die Einwohner in A geimpft, die von B ungeimpft sind. Im Laufe eines Jahres sterben nun an allen Todesursachen zusammen in A 200 und in B ebensoviel Menschen. Diese Sterbeziffern an und für sich lassen selbstverständlich gar kein Urtheil darüber zu, ob die Sterbefälle in A oder B häufiger waren. Zur Kenntniss der Häufigkeit bedarf man eben unbedingt der Anzahl der Stammgesammtheit, d. i. in diesem Falle der Gesammtheit der Lebenden. Solcher Art gestaltet sich also der Sterblichkeits-Coëfficient für A $\frac{200}{10000} = 20\frac{0}{00}$ und für B $\frac{200}{5000} = 40\frac{0}{00}$, wir können daher sagen, die Todesfälle seien in B doppelt so *häufig*.

Nun tritt aber eine neue Frage auf. Man will wissen, ob die Impfung nicht auf das Auftreten *einer bestimmten* Todesursache (z. B. der Blattern) von Einfluss ist. Hier spaltet sich der Umfang der zu beobachtenden Wirkung in zwei Theile: einerseits Sterblichkeit im Allgemeinen, andererseits Sterblichkeit an Blattern, und die Untersuchung bewegt sich um jene Frage, ob die Impfung irgend einen speciellen Einfluss auf das Auftreten *dieser einen* Todesursache habe, d. h. man wirft die Frage auf, ob zwischen der Impfung und der Häufigkeit der Blatternkrankheit ein specieller Causalnexus bestehe. Nun soll sich das folgende Resultat ergeben haben, das, wohlgemerkt, noch immer von der Anzahl der Lebenden ausgeht: es sterben von je 10,000 Lebenden

	in A (Geimpfte)	in B (Ungeimpfte)
an Blattern	100	200
an Krankheiten der Respirations-Organe	50	100
an Krankheiten der Verdauungs-Organe	20	40
an sonstigen Todesursachen	30	60
also an allen Todesursachen	$\frac{200}{10000} = 20\frac{0}{00}$,	$\frac{200}{5000} = 40\frac{0}{00}$.

Wir fragen nun: wird man aus obigen Beobachtungen behaupten können, dass die

grössere Häufigkeit der Blattern in B eine specielle Wirkung der Nichtimpfung sei? Gewiss nicht, denn es zeigt sich doch bei jeder Todesursache einzeln und bei allen zusammen dasselbe Verhältniss, nämlich, dass in B die Sterblichkeit eine um 100 % höhere ist, und die Blatternsterblichkeit weicht auch nur um diese 100 % von jener in A ab. Eine specielle Beeinflussung der Blatternsterblichkeit durch die Impfung würde also nur dann behauptet werden können, wenn die Häufigkeit der Blattern — stets auf tausend *Lebende* berechnet — von dem Normalstande nach oben oder unten abweichen würde. Nur in dem Falle also, dass es z. B. hiesse, die Anzahl der Gestorbenen betrug

	in A (10,000 Geimpfte)	in B (5000 Ungeimpfte)
an Blattern,.....	100 = 10 ‰	150 = 30 ‰
an allen anderen Todesursachen	100 = 10 ‰	50 = 10 ‰
Zusammen.....	200 = 20 ‰	200 = 40 ‰,

könnte man mit Recht behaupten, dass bei den Ungeimpften, neben der für alle Todesursachen erkennbaren allgemeinen grösseren Sterblichkeit noch eine speciell erhöhte für Blatternsterblichkeit bestehe. Denn während im Durchschnitte die Sterblichkeit der Ungeimpften eine doppelt grössere (= 200 %) ist, erscheint dieselbe bei Blattern verdreifacht (= 300 %); die durch Nichtimpfung verursachte Steigerung der Blatternmortalität betrüge in diesem Falle 50 % (nämlich der Steigerung von 200 % auf 300 % entsprechend). Nur solcher Art würde also dann der Beweis hergestellt sein, dass zwischen Nichtimpfung und Blatternerkrankung wirklich eine specielle und bestimmte Causalität bestünde. Würden im Gegentheil an Blattern von 10,000 Lebenden in A 100, in B 150 sterben, so könnte man für diese grössere *Häufigkeit* der Blatternfälle in B nicht die Ungeimpftheit verantwortlich machen, denn selbst für den Fall, wenn die Impfung gar keinen Einfluss auf die Blattern ausübte, müssten in B, in Folge der daselbst herrschenden sonstigen ungünstigen Lebensverhältnisse (welche aus der doppelt so grossen Ziffer aller Todesfälle zusammen ersichtlich sind), auch den Blattern eine doppelt so grosse Quote erliegen, als in A. Statt dieser zu erwartenden Steigerung von 100 % zeigt sich aber nur eine solche von 50 %; in diesem Falle hätte also die Nichtimpfung die normal zu erwartende Blatternmortalität geradezu herabgedrückt. Man ersieht hieraus am besten, wie wenig aus der grösseren *Häufigkeit* allein auf den Bestand eines *Causalnexus* geschlossen werden kann.

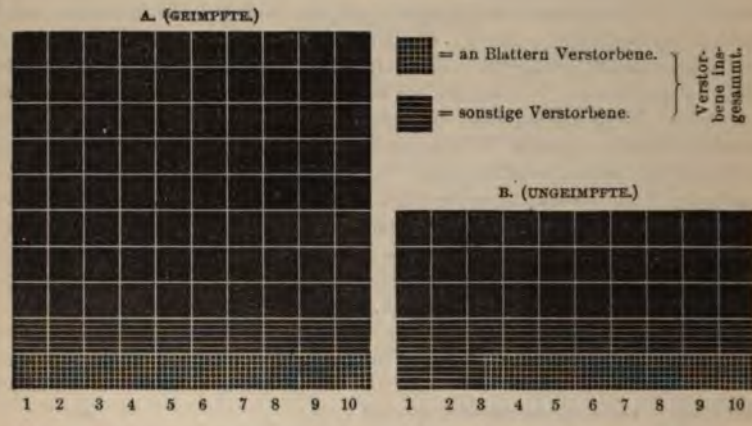
Alles hier Gesagte wird auch nach der gewöhnlichen statistischen Auffassung zugestanden werden müssen, da wir ja bisher die geforderte Basis aller Mortalitätsberechnungen, nämlich die Anzahl der Lebenden, nicht verlassen haben. Gelingt es aber, zu beweisen, dass sich die oben auf Grund der Lebenden berechnete Abweichung von 50 % auch aus den blossen Sterbeziffern, mithin ohne Kenntniss der Lebendenziffer, berechnen lässt, so wäre damit bewiesen, dass das Problem der Gesammtheit der Lebenden durchaus keine allgemeine Geltung hat, und für den gegenwärtigen Fall auch nicht anwendbar wäre.

Dass aber die Zahl der Lebenden nicht gekannt zu werden braucht, und dass sich diese Steigerung, beziehentlich Minderung, (deren procentualen Werth ich mit dem Namen der relativen Intensität bezeichne), auch aus den blossen Sterbe-, beziehungsweise Erkrankungs-Ziffern berechnen lässt, ergibt sich aus der nachfolgenden Wiederholung des zweiten Beispiels, wo aber die bisher als bekannt angenommene Bevölkerungsziffer als unbekannt betrachtet und deshalb für dieselbe ein Fragezeichen eingestellt wird:

	In A	In B
Anzahl der Lebenden.....	?	?
Anzahl der Verstorbenen insgesammt.....	200	200
Darunter Anzahl der an Blattern Verstorbenen.....	100	150.

Hier wird sich folgender einfacher logischer Schluss ergeben: Für den Fall, als die Nichtimpfung von gar keinem Einflusse auf das Auftreten der Blatterntodesfälle wäre, müssten, da an allen Todesursachen zusammen ebensoviel Geimpfte als Ungeimpfte starben, auch auf 100 Blatternfälle der Geimpften 100 Fälle der Ungeimpften kommen. Statt dieses zu erwartenden Standes von 100 Fällen in B finden wir aber deren 150, wodurch nicht nur bewiesen ist, dass die Nichtimpfung eine Steigerung der Blatterntodesfälle hervorgerufen, sondern auch das Maass dieser Steigerung mit 50 % fixirt werden kann. Diese 50 % verstehe ich unter dem Ausdrucke der relativen Intensität. Stellt man nun in A und B die Bevölkerungsziffer wieder ein und berechnet die relative Intensität auf dieser Grundlage, so gelangt man zu dem nämlichen Resultate, woraus also ersichtlich ist, dass man zur Berechnung dieses Ergebnisses die Anzahl der Lebenden nicht zu kennen braucht. (Man kann auch folgendermaassen folgern: In A betrug der Procentsatz der Blattern 50 % der Todesfälle, in B 75, folglich Steigerung [= relative Intensität] 50 %.)

GRAPHISCHER BEWEIS.

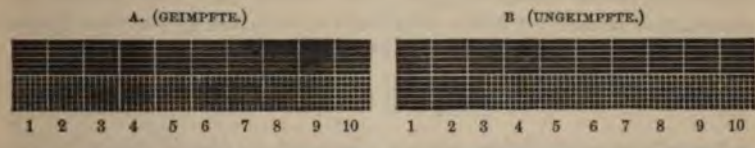


Die zwei Vierecke A und B repräsentiren die Anzahl[1] der Lebenden, und zwar ist, wie sich aus der Anzahl der eingezeichneten Quadrate ergibt, die Anzahl der in A Lebenden (Geimpfte) doppelt so gross, als der in B Lebenden (Ungeimpfte). Die Anzahl der im Laufe eines Jahres Verstorbenen ist durch Schraffirung, und die der hierunter an Blattern Verstorbenen durch doppelte Schraffirung ausgedrückt. Eine Vergleichung der doppelt schraffirten Quadrate (Blatterntodte) mit der Gesammtheit (*Lebende*) zeigt, dass bei den Geimpften $\frac{100}{1000} = 10$ %, bei den Ungeimpften $\frac{75}{500} = 15$ %, an Blattern starben. Die grössere *Häufigkeit* der Blatternfälle bei Ungeimpften ist also ausgemacht. Kann man aber behaupten, dass die *Ursache* dieser grösseren Häufigkeit in der Nichtimpfung liege? Das ist eine ganz neue Frage, welche sich aus dem Mortalitäts-Coëfficienten der Blattern allein absolut nicht beantworten lässt. Es ist doch möglich, dass diese Uebersterblichkeit auch durch andere Ursachen hervorgerufen wurde, und wissen wir ja bereits, dass die Impfgegner für die Uebersterblichkeit der Ungeimpften wirklich andere Ursachen, nämlich deren geringere Widerstands-

[1] Wenn jedes Quadrat 10 Menschen repräsentirt, so zählt A 1000, B 500 Einwohner.

fähigkeit im Allgemeinen, geltend machen. Man ist demnach gezwungen, eine ganz neue Untersuchung anzustellen, nämlich die allgemeine Widerstandsfähigkeit der Geimpften und Ungeimpften zu untersuchen, um dann auf die Frage übergehen zu können, ob das gefundene Normalverhältniss bei den Blattern eine Zu- oder Abnahme aufweise. In unserem Falle muss man also vorerst den allgemeinen Sterblichkeits-Coëfficienten bei Geimpften und Ungeimpften vergleichen. Man findet, dass derselbe bei Ersteren 20%, bei Letzteren 40% beträgt, dass also von den Ungeimpften überhaupt — sagen wir mit den Impfgegnern, in Folge ihrer schwächeren Widerstandsfähigkeit — eine doppelt so grosse Anzahl starb. Nachdem nun in A 10% der gesammten 1000 Lebenden an Blattern gestorben, hätten erwartungsgemäss in B 20%, das ist von den 500 Lebenden 100, schon für jenen Fall den Blattern erliegen müssen, als die Nichtimpfung ohne jeden Einfluss blieb. In Wirklichkeit findet man aber in B bloss 75 Blatternverstorbene (15% der Lebenden), demnach speciell bei Blattern eine Depression der erwartungsmässigen 100 Fälle um 25%. Trotzdem also die Blatterntodesfälle bei den Ungeimpften häufiger gewesen, könnte in einem solchen Falle, nicht bloss nicht behauptet werden, dass dies eine Folge der Nichtimpfung sei, sondern wir müssten unter den angeführten Umständen im Gegentheile constatiren, dass sich die Ungeimpften speciell den Blattern gegenüber günstiger verhielten.

Alle diese Berechnungen galten unter der Voraussetzung, dass die Anzahl der Lebenden bekannt sei. Nehmen wir nun an, es sei dies nicht der Fall; es seien z. B. die Zählungsregister, welche auch die Anzahl der unter den Lebenden gezählten Geimpften und Ungeimpften enthielten, zu Grunde gegangen und man habe zur Beurtheilung der obschwebenden Frage nur mehr die Todtenlisten zur Verfügung, oder, um bei der graphischen Darstellung zu verbleiben, es würden uns bloss die schraffirten Stellen der Zeichnung vorliegen:



Aus dieser Zeichnung ersehen wir nun, dass, während in A von den 200 Verstorbenen 100 den Blattern erlagen, mithin 50% aller Sterbefälle, in B die Blatterntodten bloss $\frac{75}{200}=37\frac{1}{2}\%$ betrugen. Eine Vergleichung dieser beiden Verhältnisszahlen (50 : 37.5) zeigt uns, dass die Depression der Blatternsterblichkeit in B 25% beträgt. Wir haben also auch ohne die geringste Ahnung von der Anzahl der Lebenden zu haben, das gleiche Resultat gewonnen, zu welchem wir vorher auf Grund der Lebendenanzahl gelangten.

ALGEBRAISCHER BEWEIS.

Indem wir zu diesem übergehen, möge noch eines Umstandes gedacht werden, den wir bisher der Einfachheit halber unberücksichtigt gelassen.

Wir haben nämlich in den bisherigen Berechnungen den durchschnittlichen Mortalitäts-Coëfficienten als jenes Maass betrachtet, an welchem der specielle Mortalitäts-Coëfficient der Blattern gemessen wurde. Nun sind aber in dem Durchschnitte auch die Blattern mit enhalten und ist es deshalb richtiger, den Blattern-Coëfficienten mit dem Coëfficienten aller jener Krankheiten, welche mit der Impfung absolut in keinen Zusammenhang gebracht werden können, oder der Einfachheit halber mit dem Coëfficienten aller Nichtblatternfälle, in Verhältniss zu setzen.

Im vorliegenden Falle, wo den Blattern, gegenüber den übrigen Todesfällen, doch nur eine untergeordnete Bedeutung zukommt, wird die Einführung dieser Unterscheidung zu keinem bemerkenswerthen Unterschiede führen; im Principe aber ist es immer richtiger, den Theil mit seinem contradictorisch entgegengesetzten Theile und nicht mit dem Ganzen zu vergleichen. Nehmen wir an, es handle sich um eine häufigere Todesursache, etwa Tuberculose, welcher ungefähr die Hälfte aller Verstorbenen angehört, so wird es auch schon auffälliger sein, in wie grossem Maasse die bei Tuberculose bestehenden speciellen Verhältnisse das Gesammtbild aller Todesfälle beeinflussen müssen, und um wieviel richtiger es in Folge dessen sei, bei allen Parallelen nicht die Tuberculose mit der Gesammtheit aller Todesursachen, in welcher die Tuberculose selbst wieder mit enthalten ist, sondern mit der Gesammtheit aller *übrigen* Todesursachen, mit Ausschluss der Tuberculose, in Vergleich zu bringen. In dem nachfolgenden algebraischen Beweise wird dieser letztgenannte Vorgang befolgt.

Man bezeichne

	die Lebenden	die an Blattern Verstorbenen	die an sonstigen Krankheiten Verstorbenen
bei Nichtgeimpften mit	L_1	B_1	N_1
bei Geimpften "	L_2	B_2	N_2
insgesammt "	L	B	N.

Die gewöhnliche Coëfficientenberechnung zieht nun ganz unberechtigter Weise Schlüsse aus dem Verhältnisse der Blatternsterblichkeit bei Geimpften und Ungeimpften, das ist vergleicht $\frac{B_1}{L_1}$ mit $\frac{B_2}{L_2}$,

$$\frac{B_1}{L_1} : \frac{B_2}{L_2} = \frac{B_1 L_2}{L_1 B_2},$$

ohne zu berücksichtigen, dass die normale Sterblichkeit der Ungeimpften $\frac{N_1}{L_1}$ schon an sich grösser als jene der Geimpften $\frac{N_2}{L_2}$ ist. Zur Vermeidung der sich hieraus ergebenden Fehlschlüsse müsste vor Allem festgestellt werden, um wieviel denn die *normale* Sterblichkeit der Ungeimpften höher als jene der Geimpften stehe, also

$$\frac{N_1}{L_1} : \frac{N_2}{L_2} = \frac{N_1 L_2}{L_1 N_2}.$$

An diesem Normalverhältnisse messen wir dann erst die Abweichung in der Blatternsterblichkeit, $= \frac{B_1 L_2}{L_1 B_2}$,

$$\text{also } \frac{B_1 L_2}{L_1 B_2} : \frac{N_1 L_2}{L_2 N_1} = \frac{B_1 L_2 L_1 N_2}{N_1 L_2 L_1 B_2}.$$

In diesem Bruche erscheinen die Lebenden L_1 und L_2 sowohl im Zähler, als auch im Nenner, entfallen demnach, wodurch der Bruch

$$= \frac{B_1 N_2}{N_1 B_2}$$

verbleibt, den man in das Verhältniss $\frac{B_1}{N_1} : \frac{B_2}{N_2}$ auflösen kann.

N_1 *und* N_2, *sowie* B_1 *und* B_2 *sind aber blosse Sterbeziffern*, woraus ersichtlich ist, *dass man auch auf Grund von Sterbeziffern allein zu dem gleichen Resultate gelangen kann, als ob man von der Anzahl der Lebenden ausgegangen wäre.*[1]

---

[1] Es möge hier noch die allgemeine Erwägung Raum finden, dass, nachdem wir angenommen haben, es seien bei Geimpften und Ungeimpften alle auf die Blattern wirkenden Ursachen gleich und dieselben nur hinsichtlich ihres Impfzustandes verschieden,—wir berechtigt sind, die Verschiedenartigkeit der Blatternsterblichkeit bei Geimpften und Ungeimpften als Wirkungen des Impfzustandes zu betrachten. Es ist aber nicht zu übersehen, dass diese im Allgemeinen richtige Folgerung, in der Wirklichkeit durch den Hinzutritt begleitender Umstände afficirt

werden kann. Denken wir uns, dass die Gesammtheit der Geimpften durch irgend einen *nicht mit der Impfung zusammenhängenden Umstand* in neue Beziehungen gebracht werde, etwa dass man die Ungeimpften aus den Schulen ausschlösse. In Folge *dieses* Umstandes wird es dann unter den Geimpften mehr lateinisch Sprechende, mehr Aerzte, mehr Kurzsichtige, vielleicht auch mehr Geisteskranke geben; es wäre aber ganz unvernünftig, die Impfung als Ursache dieser Erscheinungen anzunehmen. (Auf die Berücksichtigung dieser begleitenden Umstände habe ich auch schon bei meinen, mit Hülfe eben dieser Methode angestellten Untersuchungen über den Einfluss der Wohnungen auf die Todesursachen — Enke, 1884 — hingewiesen.) Diese Bemerkung soll nicht die angewendete Methode vor Missdeutungen schützen, denn die obschwebende logische Schwierigkeit ist im Allgemeinen auf alle Causaluntersuchungen, also auch auf das Gebiet aller statistischen Folgerungen anwendbar. Auch wenn man von der Zahl der Lebenden ausgeht, wird man Gefahr laufen, in Folge ähnlicher begleitender Ursachen, falsche Causalerklärungen aufzustellen! Die angewendete neue Methode braucht begreiflicherweise doch nur ebensoviel zu leisten, als die bisherige unanfechtbare zu leisten versprach: die der Causaleinsicht des menschlichen Geistes durch die Natur desselben gezogenen engen Grenzen können durch keine logische Prozedur aufgehoben werden.

ZEHNTES KAPITEL.

ANWENDUNG DER NEUEN METHODE ZUR LÖSUNG DER PROBLEME DER VACCINATIONS-STATISTIK.

1) Einfluss der Vaccination auf die Pockenmorbidität.
2) " " " " " Pockenmortalität.
3) " " " " " Pockenlethalität.
4) " " " " " Ueberimpfung von Krankheiten, und zwar: *Syphilis, Rothlauf, Lungentuberculose, Croup, Diphtheritis, Rachitis, Scrophulose, Typhus, Cholera, Masern, Scharlach, Hautkrankheiten.*
5) Bilanz der Schutzpockenimpfung.
Schluss.

TABELLEN:

No. 1. Impfzustand der im Jahre 1886 in vier Budapester und in fünfzehn ungarischen Provinzspitälern behandelten Personen, nach Krankheiten und mit Unterscheidung des Alters.
No. 2. Todesursachen in Budapest und in neun ungarischen Provinzstädten, nach Impfzustand und Alter (1886).
No. 3. Impfzustand der in der Ambulanz des Budapester Stephanie-Kinderspitals i. J. 1874 behandelten Kinder, nach Krankheiten und mit Unterscheidung des Alters.

Wir haben im vorigen Kapitel gesehen, dass es möglich sei, auch ohne Kenntniss der Gesammtheit der Lebenden ein richtiges Urtheil über den Schutz der Impfung zu bilden, und wollen nun zur praktischen Verwerthung dieser neuen Methode, den in der Vaccinations-Statistik constatirten Lücken und als unlösbar bezeichneten Problemen gegenüber, schreiten. Den entschiedensten Nutzen wird uns die neue Methode

1) in der Mortalitätsfrage bieten. Freilich sind solche Daten, wie sie bisher vorlagen, und aus denen man nur entnehmen kann, wie viele von den *Blatterntodten* geimpft waren, unverwendbar. Um den Einfluss der Impfung nach der im Vorhergehenden dargelegten neuen Methode zu erkennen, müsste man auch wissen, wie gross die Anzahl der Geimpften unter den an anderen Todesursachen Verstorbenen ist. Es wäre also nothwendig, nicht nur, wie bisher bei Blatterntodesfällen, sondern bei jedem Todesfalle den Impfzustand des Verstorbenen zu registriren. Selbst Unglücksfälle und Selbstmorde wären nicht auszunehmen; so befremdlich diese Forderung auf den ersten Blick auch scheinen mag, wird man bei näherem Erwägen doch finden, dass diese Fälle gerade die dankbarsten sind, weil hier auch nicht der leiseste Zusammenhang zwischen Impfung und Todesursache bestehen kann. Ich habe nun seit dem 1. April 1886 die Einrichtung für Budapest getroffen, dass bei jedem Verstorbenen, ohne Ausnahme, der Umstand, ob derselbe geimpft gewesen oder nicht, bez. ob die Impfung zweifelhaft sei, angegeben werde. (Geblatterte wurden als Geimpfte betrachtet.) Diese Aufzeichnungsweise functionirt sehr befriedigend; die Todtenbeschauer erklären, dass in der Mehrzahl der Fälle Impfung oder Nichtimpfung verlässlich constatirt werden könne, und ist in Folge dessen die Zahl der zweifelhaften Fälle auch eine geringe. Da diese Einführung mit der grossen Blatternepidemie des Jahres 1886 zusammenfällt, hat diese solcher Art der Statistik ihren Tribut zollen müssen. Die Ergebnisse dieser Beobachtungen enthält die Tabelle No. II (S. 332 ff.). Ueberdies war ich so glücklich, auch noch die behördlichen Autoritäten von neun anderen ungarischen Städten dafür zu gewinnen, auf ihrem Gebiete ähnliche Beobachtungen anzustellen und sind die Ergebnisse derselben in der nachfolgenden Tabelle No. II ebenfalls mitgetheilt.

2) Werthvolle Aufschlüsse werden wir in dieser neuen Methode ferner auf dem Gebiete der *Morbiditätsfrage* verdanken. Nachdem nun ein Anmeldungszwang für

alle Erkrankungsfälle bisher noch nirgends existirt, müssen wir uns bei unseren Untersuchungen auf die Spitalsstatistik beschränken. Zur Durchführung der neuen Methode ist es also unbedingt nothwendig, dass nicht nur bei den Blatternkranken, sondern bei *allen* in's Spital Aufgenommenen der Impfzustand registrirt werde. Auch hier dürfte es auffallend erscheinen, bei oculistischen und chirurgischen Fällen danach zu fragen, ob der Betreffende geimpft sei, da ja die Impfung auf diese Krankheiten von gar keinem Einflusse sein kann. Eben dieses Moment der absolut ausgeschlossenen Causalität ist es aber, welches, wie in der Mortalitäts- so auch in der Morbiditätsfrage, den diesbezüglichen Angaben eine ganz besondere Bedeutung verleiht. Die diesbezüglichen Nachweisungen finden sich in Tabelle No. I.

3) Dass der auf Spitalsbeobachtungen beruhende Theil unseres Materials auch über die Lethalität der Blattern bei Geimpften und Ungeimpften Auskunft bietet, ist klar. Lethalitätsangaben sind in den Spitalsaufzeichnungen zwar schon reichlich vorhanden. Wir werden aber mit Hülfe der neuen Methode diese Lethalitätsdaten in einer bisher noch nicht versuchten Richtung verwerthen können, indem wir nämlich die grössere Gesammtlethalität der pockenkranken Ungeimpften in ihre zwei Factoren: *a*) allgemeine ungünstigere Lebensverhältnisse der Ungeimpften, *b*) specieller Einfluss der Nichtimpfung, zerlegen werden können, wodurch wir in die Lage versetzt sein werden, die auf den ersten Factor Bezug habenden Berufungen der Impfgegner zu prüfen und ziffermässig zu präcisiren.

4) Soll die neue Methode uns auch über jenes bisher als geradezu unlösbar angesehene Problem hinweghelfen, wie nämlich der Zusammenhang oder Nichtzusammenhang zwischen Impfung und den nach der Impfung auftretenden Krankheiten hergestellt werden soll. Nach dem bisher Gesagten wird man schon errathen, wie dies anzustellen. Es werden nämlich vor Allem die normalen Mortalitätsverhältnisse Geimpfter und Ungeimpfter zu constatiren sein. An diesen lassen sich dann der Reihe nach alle jene Krankheiten messen, gegen welche ein Verdacht ausgesprochen würde, dass sie durch Vaccination übergeimpft zu werden pflegen. Die grosse Schwierigkeit z. B., ob die bei einem erwachsenen Kinde aufgetretene Rachitis eine Folge der in der Jugend vollzogenen Impfung sei—eine Frage, deren Beantwortung fast unmöglich erscheint—, findet mit Hülfe der neuen Methode doch ihre Erledigung. Ist nämlich die Impfung die Ursache der Rachitis, so wird es bei geimpften Kindern mehr rachitische geben als bei ungeimpften, und so fort auch für alle andern, mit Recht oder Unrecht, als überimpfbar bezeichneten Krankheiten. Und zwar kann diese Beobachtung nach zwei Richtungen erfolgen, nämlich aus den Spitalsbeobachtungen hinsichtlich der Morbidität, und aus den Todtenlisten hinsichtlich der Mortalität.

Solcher Art bliebe von den im ersten Theil angeführten controversen Fragen der Impfvertheidigung und des Impfangriffes nur mehr ein Problem in Schwebe, nämlich, ob die Abnahme der Blattern nur zufällig mit der Einführung der Impfung zusammenfiele oder eine nothwendige Folge derselben sei (der historische Beweis). Falls aber einmal bewiesen sein sollte, dass die Impfung wirklich einen Schutz gegen Blatternerkrankung und Blatterntod bietet, so kann es nicht anders sein, als dass mit der Einführung der Impfung und mit je grösserer Verbreitung derselben ein desto grösserer Schutz geschaffen werden *musste*. Nach einem direkten Beweise von der Schutzkraft sind die indirekten — und hiermit auch der historische — überflüssig.

1. EINFLUSS DER IMPFUNG AUF DIE MORBIDITÄT.

Eine allgemeine Morbiditätsstatistik gehört freilich in den Bereich der Unmöglichkeit, insolange man die Aerzte nicht verhalten kann, jeden Erkrankungsfall ihrer Praxis, auch bei nichtinfectiösen Krankheiten, der Behörde anzuzeigen, — ein Verlangen, welches bisher noch nirgends erreicht, ja nicht einmal gestellt wurde. Wo ein Anmeldungszwang für Infectionskrankheiten besteht, wäre es mit Rücksicht auf die

vorgeschlagene Beobachtungsweise wünschenswerth, bei allen Anmeldungen auch den Impfzustand anzugeben; hierdurch würde man wenigstens für diesen wichtigen Theil der Morbiditätsstatistik Anhaltspunkte in Bezug auf die Impffrage gewinnen. Unter den obwaltenden Umständen aber können die Erkrankungen nur jener Bevölkerungsklassen beobachtet werden, deren Morbiditätsstatistik controlirbar ist, also jener, welche die Spitäler aufsuchen.

Hier ist es am Platze, auf jene Einwände zurückzukommen, welche der Spitalstatistik gegenüber erhoben werden. Diese Einwände sind an und für sich insoferne berechtigt, als in den Spitälern statt der ganzen Bevölkerung nur ein Bruchtheil derselben beobachtet werden kann; aber für die Lösung der Impffrage ist dieser Umstand doch nur von untergeordneter Bedeutung, und zwar deshalb, weil die Auswahl Seitens der Spitäler im ungünstigen Sinne erfolgt, indem nur die ärmsten, widerstandsschwächsten Elemente daselbst Aufnahme finden. Wenn also die Impfung sich selbst bei diesen von günstiger Wirkung erweist, so kann nur angenommen werden, dass bei Mitberücksichtigung auch der besser situirten Klassen, die Resultate sich noch günstiger gestalten müssten. Die gegen die Spitalstatistik erhobenen Bedenken repräsentiren, vom impffreundlichen Standpunkte, keine Verbesserung, sondern nur eine Verschlechterung der Resultate. Wenn die Impfvertheidigung also sich schon mit dem Bruchtheile der ihr gebührenden Anerkennung begnügt, liegt für den Impfangriff gewiss keine Ursache vor, darauf hinzuweisen, dass bei Zuziehung aller Bevölkerungselemente die Ergebnisse für die Impfung sich noch günstiger gestalten müssten.

Ich übergehe nun auf meine neuen Beobachtungen.

Die Beschaffung des Materials erfolgte in der Weise, dass jene Spitalverwaltungen, welche die Freundlichkeit hatten, mein Unternehmen zu unterstützen, vom 1. April 1886 für jeden Kranken ein Kärtchen des folgenden Inhaltes ausstellten und mir mit Schluss der Semester zusendeten :—

Laufende Nummer..... Alter.....

Todesursache.....

Geimpft? Nicht geimpft? Zweifelhaft?

Wohlhabend? oder arm?

Geschlecht: männlich, weiblich.

Leider war es mir nicht möglich, die reichste Quelle der pockenstatistischen Beobachtungen, nämlich jene der Budapester Communalspitäler, im Interesse dieses Problems zu verwerthen: in Folge zu grosser Ueberhäufung mit laufenden Agenden konnte meinem Ansuchen, für jeden Kranken die oben erwähnte Karte auszufüllen, nicht willfahrt werden. Nur dem freundlichen Interesse, das Secundärarzt Wesselovszky vom St. Rochus-Spital dieser Arbeit widmete und das denselben bewog, die nicht weniger als 4261 Fälle repräsentirenden Krankenaufnahmen der Monate November und December für mich zu excerpiren, ist es zu danken, dass wenigstens zwei Monatsaufnahmen dieses grössten unserer Communal-Spitäler in dieser Arbeit berücksichtigt werden konnten.

Solcherweise beschränkten sich die Beobachtungen in der Hauptstadt bloss auf vier Spitäler; auf das der barmherzigen Brüder, auf das israelitische Spital, auf das Stephanie-Kinderspital und überdies auf zwei Monate des St. Rochus-Spitals. Da im Stephanie-Spital Blatternkranke nicht aufgenommen werden, bot dieses nur eine geringe Ausbeute, nämlich nur solche Blatternfälle, welche die bereits aufgenommenen

Kranken befielen. Wohl aber würden die Aufzeichnungen dieses Spitals eine überaus seltene Fundgrube für die in Rede stehende Untersuchung in Folge jenes Umstandes abgeben, dass daselbst schon seit seiner Gründung (1839), also seit einem halben Jahrhundert, für *alle* Kranke, sowohl in der Ambulanz als in der Klinik, die Fragen nach dem Impfzustande gestellt und protokollirt wurden. Die Protokolle dieses Spitals repräsentiren demnach mehr als eine Viertel Million von Krankheitsfällen, für welche dieser wichtige Umstand stets verlässlich notirt erscheint. Bei meiner Ueberbürdung mit anderen Arbeiten konnte ich es nicht unternehmen, mich einer Durcharbeitung dieses Massenmaterials zu unterziehen, wohl aber habe ich für eines der grössten Epidemiejahre, 1874, die klinischen Fälle (707) und die ambulatorischen (6802) aufgearbeitet und in dem vorliegenden Werke auch verwerthet. Das reiche Material, welches die Protokolle des Stephanie-Kinderspitals bergen, möge einem sich für die Frage interessirenden Medicinalstatistiker hiermit bestens empfohlen sein.

Ausser den genannten vier hauptstädtischen Spitälern war ich noch so glücklich, die Direktoren von 15 Provinzspitälern für die Frage zu interessiren, und statte ich den unten genannten Herren hiemit meinen besten Dank für die freundliche und bereitwillige Unterstützung ab, mit der sie mein Vorhaben durch Einführung der erwähnten Kartenaufzeichnungen zu fördern die Güte hatten. Die in Tabelle No. I (Seite 318 ff.) vereinigten Morbitätsbeobachtungen erstrecken sich auf folgende Spitäler:

		MITARBEITER:
A. IN BUDAPEST.		
Städtisches St. Rochus-Spital (2 Monate)	4261 Kranke,	Dr. Weszelovszky,
Barmherzige Brüder-Spital	1211 "	Dr. Theodor Csáky,
Stephanie-Kinderspital	864 "	Direktor Dr. Joh. Bókay,
Israelitisches Spital	393 "	Prof. Dr. Jul. Böke, Vorsitzender der Spitals-Commission.
B. IN DER PROVINZ.		
Pozsony (Pressburg), allgemeines Krankenhaus	1980 "	Direktor Dr. Carl Kanka,
Pozsony (Pressburg), städtisches Krankenhaus	1083 "	" Dr. Béla Tauscher,
Arad	1677 "	" Dr. Matavovsky,
Miskolcz	1464 "	" Dr. Josef Popper,
Nagy-Szeben (Hermannstadt)	1418 "	" Dr. Ambrosius Süssmann,
Szeged (Szegedin)	1312 "	" Dr. Matthias Singer,
Nagy-Kálló	983 "	" Dr. Rudolf Kállay,
Györ (Raab)	735 "	" Dr. Ludwig Petz,
Temesvár	640 "	" Dr. Gedeon Bécsi,
Nyitra (Neutra)	547 "	" Dr. Carl Thuróczy,
Vácz (Waitzen), Strafhaus-Spital	486 "	" Dr. Béla Rákosi,
Székes-Fehérvár (Stuhlweissenburg)	467 "	" Dr. Franz Major,
Brassó (Cronstadt)	457 "	" Dr. Josef Fabritius,
Trencsén (Trentschin)	447 "	" Dr. Josef Strossner,
Uj-Vidék (Neusatz)	149 "	" Dr. Franz Sztrohne.
Zusammen	20,574 Kranke,	

worunter

0— 1 jährige	223
1— 5 "	739
5—20 "	4421
20—30 "	6478
30—40 "	3348
40—60 "	3979
über 60 "	1341
unbekanntes Alter	45.

Die Tabelle No. I enthält nun für über 40 Krankheiten die Summe der Behandelten und Verstorbenen nach dem Impfzustande (ungeimpft, geimpft bez. geblattert, zweifelhaft), mit Unterscheidung des Alters (0—1, 1—5, 5—20, 20—30, 30—40, 40—60, über 60 Jahre, Alter unbekannt). Die 40 Krankheiten sind in 10 grosse Gruppen zusammengefasst; bei diesen, sowie für die Hauptsumme, ist überdies die Unterscheidung zwischen hauptstädtischen und Provinzspitälern durchgeführt.

IMPFZUSTAND DER IM JAHRE 1886 IN VIER BUDAPESTER UND IN FÜNFZEHN UNGARISCHEN PROVINZSPITÄLERN BEHANDELTEN PERSONEN, NACH KRANKHEITEN UND MIT UNTERSCHEIDUNG DES ALTERS.

I. KRANKHEITEN DES NERVENSYSTEMS.

Krankheiten.		0-1 Jahr.			1-5 Jahre.			5-20 Jahre.			20-30 Jahre.			30-40 Jahre.			40-60 Jahre.			Über 60 Jahre.			Unbekanntes Alter.			Summe der über Einjährigen.			
		Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Total.
Hirnhautentzündung (Meningitis).	Behandelte.	1	...	...	4	3	...	7	2	2	6	...	1	8	1	...	5	...	1	...	...	...	...	...	...	30	6	4	40
	Verstorbene.	1	...	...	3	2	...	6	...	1	4	...	1	3	...	...	3	...	1	...	...	...	...	...	...	19	2	3	24
Gehirnentzündung (Encephalitis)	Behandelte.	...	...	...	2	...	...	1	...	...	1	1	...	1	...	...	2	...	...	1	...	...	...	...	1	8	1	1	10
	Verstorbene.	...	...	...	2	...	...	...	...	...	1	1	...	...	...	...	2	...	...	...	...	...	...	...	1	5	1	1	7
Gehirnschlag und Lähmung (Apoplexy and Paralysis).	Behandelte.	...	1	...	1	1	...	6	1	...	6	...	...	10	1	1	22	4	1	22	3	3	1	...	...	68	10	5	83
	Verstorbene.	...	...	...	...	...	...	1	...	...	3	...	...	3	...	...	13	...	...	15	...	...	1	...	...	36	3	3	42
Krämpfe (Convulsions)	Behandelte.	...	2	...	...	1	...	2	...	...	1	...	1	...	...	...	...	...	...	...	...	...	...	...	...	3	1	1	7
	Verstorbene.	...	2	...	...	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	1	...	5
Andere Nervenkrankheiten.	Behandelte.	...	1	...	6	2	...	116	14	5	144	24	7	149	24	11	153	25	16	40	14	2	...	...	...	608	103	41	751
	Verstorbene.	...	1	...	1	1	...	9	1	1	11	2	...	14	4	3	17	6	3	7	6	1	...	...	...	59	20	8	82
Summe der Nervenkrankheiten. Budapest	Behandelte.	1	3	...	9	6	...	52	6	1	38	6	4	48	5	3	46	1	4	21	2	1	...	...	...	214	26	13	253
	Verstorbene.	1	2	...	4	3	...	6	...	...	2	...	1	9	1	...	10	...	...	10	...	1	...	...	...	41	4	2	47
Provinz	Behandelte.	...	1	...	4	1	...	80	11	6	120	19	5	120	21	9	136	28	14	42	15	4	1	...	1	503	95	39	637
	Verstorbene.	...	1	...	2	1	...	10	1	2	17	3	...	11	3	3	25	8	5	12	7	2	1	...	1	78	23	13	114
Zusammen	Behandelte.	1	4	...	13	7	...	132	17	7	158	25	9	168	26	12	182	29	18	63	17	5	1	...	1	717	121	52	890
	Verstorbene.	1	3	...	6	4	...	16	1	2	19	3	1	20	4	3	35	8	5	22	7	3	1	...	1	119	27	15	161

II. KRANKHEITEN DES GEFÄSSSYSTEMS.

Krankheiten.		0-1 Jahr.			1-5 Jahre.			5-20 Jahre.			20-30 Jahre.			30-40 Jahre.			40-60 Jahre.			Über 60 Jahre.			Unbekantes Alter.			Summe der über Einjährigen.			
		Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Total.
Organ. Herzfehler (Vitia Cordis)	Behandelte.	...	...	...	...	...	...	37	2	2	38	1	...	20	5	1	68	11	1	23	10	3	...	...	...	186	29	7	222
	Verstorbene.	...	...	...	...	...	...	3	...	...	5	...	...	1	3	...	21	2	...	8	2	3	...	...	...	38	7	3	48
Sonstige Krankheiten des Gefässsystems	Behandelte.	2	...	...	4	...	...	17	3	...	29	1	1	27	4	...	31	4	4	18	12	...	1	...	...	127	24	5	156
Summe der Krankheiten des Gefässsystems. Budapest	Behandelte.	2	...	...	4	...	...	26	1	...	19	...	1	15	1	...	38	4	2	18	11	1	...	...	...	120	17	4	141
	Verstorbene.	...	...	...	...	...	...	1	...	...	4	...	...	4	1	...	12	1	2	7	6	1	...	...	...	28	8	3	39
Provinz	Behandelte.	...	...	...	...	...	...	28	4	...	48	2	...	32	8	1	61	11	3	23	11	2	1	...	...	193	36	6	235
	Verstorbene.	...	...	...	...	...	...	2	...	...	4	...	...	5	3	...	17	4	...	8	2	2	...	...	...	36	9	2	47
Zusammen	Behandelte.	2	...	...	4	...	...	54	5	...	67	2	1	47	9	1	99	15	5	41	22	3	1	...	...	313	53	10	376
	Verstorbene.	...	...	...	...	...	...	3	...	...	8	...	...	9	4	...	29	5	2	15	8	3	...	...	...	64	17	5	86

III. KRANKHEITEN DER RESPIRATIONSORGANE.

Krankheiten.		0-1 Jahr.			1-5 Jahre.			5-20 Jahre.			20-30 Jahre.			30-40 Jahre.			40-60 Jahre.			Über 60 Jahre.			Unbekantes Alter.			Summe der über Einjährigen.			
		Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Total.
Croup	Behandelte.	...	1	...	8	6	...	13	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	21	7	...	28
	Verstorbene.	...	1	...	5	5	...	11	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	16	6	...	22
Dyphtheritis	Behandelte.	2	2	...	40	9	1	33	2	1	13	...	...	3	...	...	1	...	...	...	...	...	1	...	...	91	11	2	104
	Verstorbene.	1	2	...	22	6	1	6	...	...	2	..	...	...	...	...	...	...	...	...	...	...	...	...	...	30	6	1	37
Keuchhusten (Pertussis)	Behandelte.	...	...	...	2	1	...	4	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	6	1	...	7
	Verstorbene.	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
Lungen- und Brustfell-Entzündung (Pneumonia, Bronchitis and Pleuritis).	Behandelte.	1	6	1	18	10	...	99	9	2	158	26	3	117	14	7	126	35	6	32	10	6	1	...	...	551	104	24	679
	Verstorbene.	...	3	1	3	5	...	4	2	1	11	2	...	9	2	2	33	10	2	12	5	4	...	...	...	72	26	9	107
Lungentuberculose (Tuberculosis Pulmonum).	Behandelte.	...	...	1	6	1	...	94	12	2	314	29	9	233	35	13	267	46	14	42	18	5	1	...	...	957	141	43	1,141
	Verstorbene.	...	...	1	6	1	...	36	3	...	110	8	5	93	17	6	122	24	5	22	7	5	1	...	...	390	60	21	471
Sonstige Krankheiten der Respirationsorgane	Behandelte.	2	4	...	90	7	1	159	15	2	256	30	8	174	20	7	253	42	14	96	13	15	3	...	...	1,031	127	47	1,205
	Verstorbene.	...	3	...	1	5	...	1	...	...	29	2	1	8	1	1	27	6	2	18	3	1	1	...	..	85	17	5	107
Summe der Krankheiten der Respirationsorgane. Budapest	Behandelte.	3	8	2	152	23	...	168	16	4	255	20	9	184	17	4	263	17	9	69	10	5	2	...	...	1,093	103	31	1,227
	Verstorbene.	1	6	2	35	18	...	30	4	1	59	5	1	50	4	3	77	5	3	21	5	2	1	...	...	278	41	10	324
Provinz	Behandelte	2	5	...	12	11	2	234	23	3	486	65	11	343	52	23	384	106	25	101	31	21	4	...	...	1,564	288	85	1,937
	Verstorbene.	...	3	...	2	4	1	28	2	...	93	7	5	60	16	6	105	35	6	31	10	8	1	...	...	320	74	26	420
Zusammen	Behandelte.	5	13	2	164	34	2	402	39	7	741	85	20	527	69	27	647	123	34	170	41	26	6	...	...	2,657	391	116	3,164
	Verstorbene.	1	9	2	37	22	1	58	6	1	152	12	6	110	20	9	182	40	9	52	15	10	2	...	...	593	115	36	744

IV. KRANKHEITEN DER VERDAUUNGSORGANE.

Krankheiten.		0–1 Jahr.			1–5 Jahre.			5–20 Jahre.			20–30 Jahre.			30–40 Jahre.			40–60 Jahre.			Über 60 Jahre.			Unbekanntes Alter.			Summe der über Einjährigen.			
		Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Total.
Bauchfellentzündung (Peritonitis).	Behandelte.	...	...	...	...	...	...	14	2	...	14	4	...	8	2	...	4	1	...	3	...	...	1	...	...	44	9	...	53
	Verstorbene	...	...	...	...	...	...	8	1	...	2	3	...	4	1	...	1	1	...	1	...	...	1	...	...	17	6	...	23
Darmentzündung (Enteritis)	Behandelte.	...	...	...	3	...	...	...	...	...	2	...	...	2	...	1	5	1	1	2	1	...	...	...	...	14	2	2	18
	Verstorbene.	...	...	...	3	...	...	...	...	...	1	...	...	...	...	...	2	...	1	...	...	...	...	...	...	6	...	1	7
Durchfall (Diarrhœa)	Behandelte.	9	16	...	12	9	...	46	7	...	94	8	3	77	8	2	81	17	2	21	12	2	1	1	...	335	62	9	406
	Verstorbene.	2	6	...	3	2	...	1	...	...	...	...	...	2	1	...	3	1	...	...	2	...	...	...	...	9	6	...	15
Sonstige Krankheiten der Verdauungsorgane	Behandelte.	5	21	...	16	7	...	183	26	5	310	29	9	206	25	5	274	54	16	72	24	4	2	1	...	1,063	166	39	1,268
	Verstorbene.	...	...	...	1	...	...	4	1	...	12	...	...	10	1	...	19	5	2	11	4	...	...	...	...	57	11	2	70
Summe der Krankheiten der Verdauungsorgane. Budapest	Behandelte.	4	17	...	17	7	...	105	14	3	142	9	1	90	6	2	145	9	6	26	8	1	1	...	...	526	53	13	592
	Verstorbene.	1	3	...	6	...	...	9	1	...	4	1	...	5	...	...	8	...	...	5	...	...	1	...	...	38	2	...	40
Provinz	Behandelte.	10	20	...	14	9	...	138	21	2	278	32	11	203	29	6	222	64	13	72	29	5	3	2	...	930	186	37	1,153
	Verstorbene.	1	3	...	1	2	...	4	1	...	11	2	...	11	3	...	17	7	3	7	6	...	...	...	...	51	21	3	75
Zusammen	Behandelte.	14	37	...	31	16	...	243	35	5	420	41	12	293	35	8	367	73	19	98	37	6	4	2	...	1,456	239	50	1,745
	Verstorbene.	2	6	...	7	2	...	13	2	...	15	3	3	16	3	...	25	7	3	12	6	...	1	...	...	89	23	3	115

V. KRANKHEITEN DER GESCHLECHTS- UND HARNORGANE.

Krankheiten.		Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Total.
Bright'sche Krankheit	Behandelte.	...	...	...	...	...	...	5	...	...	11	...	...	18	2	...	12	...	...	4	2	...	...	...	...	50	4	...	54
	Verstorbene.	...	...	...	...	...	...	3	...	...	4	...	...	6	...	...	7	...	...	1	1	...	...	...	...	21	1	...	22
Kindbettfieber (Febris Puerperalis).	Behandelte.	...	...	...	...	...	...	2	...	1	11	...	1	3	...	...	...	...	...	...	...	...	...	...	...	16	...	2	18
	Verstorbene.	...	...	...	...	...	...	...	...	...	1	...	1	...	...	...	...	...	...	...	...	...	...	...	...	1	...	1	2
Andere Kindbett-Krankheiten.	Behandelte.	...	...	...	...	...	...	14	...	...	40	4	3	8	...	...	2	...	...	...	...	...	...	...	...	64	4	3	71
	Verstorbene.	...	...	...	...	...	...	...	...	...	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	1	...	...	1
Andere Krankheiten der Geschlechts- und Harnorgane	Behandelte.	...	...	...	25	3	...	354	37	5	702	84	19	260	22	5	219	32	14	29	9	8	6	...	...	1,595	187	51	1,833
	Verstorbene.	...	...	...	2	1	...	10	5	...	8	4	...	12	4	...	23	3	4	8	5	5	...	...	...	63	22	9	94
Summe der Krankheiten der Geschlechts- und Harnorgane. Budapest	Behandelte.	...	...	...	17	3	...	164	14	...	304	21	7	107	9	2	80	5	2	11	2	7	...	...	...	683	54	18	755
	Verstorbene.	...	...	...	1	1	...	8	5	...	7	2	...	6	2	...	7	...	1	3	1	4	...	...	...	32	11	5	48
Provinz	Behandelte.	...	...	...	8	...	...	211	23	6	460	67	16	182	15	3	153	27	12	22	9	1	6	...	...	1,042	141	38	1,221
	Verstorbene.	...	...	...	1	...	...	5	...	...	7	2	1	12	2	...	23	3	3	6	5	1	...	...	...	54	12	5	71
Zusammen	Behandelte.	...	...	...	25	3	...	375	37	6	764	88	23	289	24	5	233	32	14	33	11	8	6	...	...	1,725	195	56	1,976
	Verstorbene.	...	...	...	2	1	...	13	5	...	14	4	1	18	4	...	30	3	4	9	6	5	...	...	...	86	23	10	119

VI. KRANKHEITEN DER BEWEGUNGSORGANE.

Krankheiten.		0–1 Jahre.			1–5 Jahre.			5–20 Jahre.			20–30 Jahre.			30–40 Jahre.			40–60 Jahre.			Über 60 Jahre.			Unbekanntes Alter.			Summe der über Einjährigen.			
		Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Total.
Englische Krankheit (Rachitis)	Behandelte.	2	1	...	3	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	3	1	...	4
	Verstorbene.	...	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	1	...	1
Andere Krankheiten der Bewegungsorgane	Behandelte.	3	2	...	40	12	...	475	48	1	599	59	12	388	45	11	580	108	24	127	65	19	2	...	...	2,211	337	67	2,615
	Verstorbene.	...	...	...	4	2	...	8	2	...	5	...	...	8	...	...	9	4	...	2	3	2	...	...	...	36	11	2	49
Summe der Krankheiten der Bewegungsorgane. Budapest	Behandelte.	1	1	...	34	7	...	168	16	...	171	12	2	135	9	3	199	14	8	43	6	1	2	...	...	752	64	14	830
	Verstorbene.	...	...	...	3	1	...	6	1	...	4	...	...	3	...	...	1	...	...	1	...	...	...	...	...	18	2	...	20
Provinz	Behandelte.	4	2	...	9	6	...	307	32	1	428	47	10	253	36	8	381	94	16	84	59	18	...	...	...	1,462	274	53	1,789
	Verstorbene.	...	1	...	1	1	...	2	1	...	1	...	...	5	...	...	8	4	...	1	3	2	...	...	...	18	9	2	29
Zusammen	Behandelte.	5	3	...	43	13	...	475	48	1	599	59	12	388	45	11	580	108	24	127	65	19	2	...	...	2,214	338	67	2,619
	Verstorbene.	...	1	...	4	2	...	8	2	...	5	...	...	8	...	...	9	4	...	2	3	2	...	...	...	36	11	2	58

VII. HAUTKRANKHEITEN.

Summe der Hautkrankheiten. Budapest	Behandelte.	...	...	...	7	...	...	78	4	...	55	6	1	33	...	1	41	...	3	10	1	2	...	...	...	224	11	7	242
	Verstorbene.	...	...	...	1	...	...	...	...	...	...	...	...	1	...	...	...	...	...	...	...	...	...	...	...	2	...	...	2
Provinz	Behandelte.	2	...	...	11	2	...	193	23	6	216	30	4	88	22	8	102	31	10	30	20	7	3	3	...	643	131	35	809
	Verstorbene.	...	...	...	1	...	...	1	...	...	...	1	1	...	1	...	1	1	...	...	...	...	...	...	...	3	3	1	7
Zusammen	Behandelte.	2	...	...	18	2	...	271	27	6	271	36	5	121	22	9	143	31	13	40	21	9	3	3	...	867	142	42	1,051
	Verstorbene.	...	...	...	2	...	...	1	...	...	...	1	1	1	1	...	1	1	1	...	...	...	...	...	...	5	3	1	9

VIII. CONSTITUTIONELLE KRANKHEITEN.

Masern (Morbilli)	Behandelte.		3		5	5		52	4		4	...		...	...		...	...		...	...			...		61	9	5	75
	Verstorbene.		...		2	1		...	...		...	...		...	...		...	...		...	...			...		2	1	...	3
Scharlach (Scarlatina)	Behandelte.		...		42	13		118	10		11	1		3	...		...	...		...	...			...		174	24	1	199
	Verstorbene.		...		13	8		14	5		1	...		...	...		...	...		...	...			...		28	13	...	41
Blattern (Variola)	Behandelte.		51		12	165	2	271	219	11	249	66	3	72	8		24	6		3	1			...		631	465	17	1,113
	Verstorbene.		38		1	112		7	94	2	18	24	1	9	1		5	...		2	...			...		42	231	3	276
Typhus	Behandelte.		...		1	...		81	5	2	112	13	3	31	3		18	8		3	1			1		247	31	5	283
	Verstorbene.		...		...	...		8	1		15	2		6	2		6	3		2	...			...		38	8	1	47
Skropheln	Behandelte.		...		3	2		38	3		25	2		6	2		2	...		...	...			...		74	9	2	85
	Verstorbene.		...		3	...		...	...		...	1		...	...		...	...		...	...			...		3	1	...	4
Cholera	Behandelte.		...		...	...		7	...		13	2		7	2	2	4	7	4	2	3			...		33	14	7	54
	Verstorbene.		...		...	...		3	...		6	1		3	2	2	1	6	4	1	2	1		...		14	11	7	32

VIII. CONSTITUTIONELLE KRANKHEITEN. — Fortsetzung.

Krankheiten.		0–1 Jahr.			1–5 Jahre.			5–20 Jahre.			20–30 Jahre.			30–40 Jahre.			40–60 Jahre.			Über 60 Jahre.			Unbekanntes Alter.			Summe der über Einjährigen.			
		Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Total.
Angeborene Syphilis (Syphilis Congenita)	Behandelte.	1	6	...	7	2	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	7	2	...	9
	Verstorbene.	...	3	...	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	1	...	...	1
Syphilis	Behandelte.	5	4	...	8	9	...	607	76	20	1,246	147	20	313	47	9	151	24	14	13	5	4	3	...	...	2,341	308	67	2,716
	Verstorbene.	...	2	...	...	...	...	3	...	...	3	1	...	...	...	...	1	...	...	...	...	...	...	...	...	7	1	...	8
Rothlauf (Erysipelas)	Behandelte.	...	1	...	...	...	...	26	1	1	47	6	1	18	2	...	25	5	2	7	3	...	1	...	...	124	17	4	145
	Verstorbene.	...	1	...	...	...	...	1	...	...	1	...	...	...	...	...	2	...	...	2	...	...	...	...	...	6	...	...	6
Blutvergiftung (Pyæmia)	Behandelte.	...	...	...	...	...	...	3	...	...	2	...	...	1	...	...	7	1	...	...	...	...	...	...	...	13	1	...	14
	Verstorbene.	...	...	...	...	...	...	1	...	...	2	...	...	...	...	...	5	1	...	...	...	...	...	...	...	8	1	...	9
Andere constitutionelle Krankheiten	Behandelte.	1	...	...	9	1	...	64	12	2	115	7	1	62	10	2	84	10	3	24	4	3	...	...	...	358	44	11	413
	Verstorbene.	...	...	...	1	1	...	4	1	1	2	...	...	1	2	...	5	...	...	2	...	1	...	...	...	15	4	2	21
Summe der constitutionellen Krankheiten. Budapest	Behandelte.	...	52	...	65	174	2	504	194	8	540	72	3	160	18	1	85	11	3	10	3	2	2	...	...	1,366	472	19	1,857
	Verstorbene.	...	39	...	20	118	...	23	86	...	25	24	...	13	1	...	13	...	...	4	...	...	1	...	...	99	229	...	328
Provinz	Behandelte.	7	13	...	22	23	1	763	136	34	1,284	172	26	353	56	12	230	50	21	42	14	6	3	1	...	2,697	452	100	3,249
	Verstorbene.	...	5	...	1	4	...	18	15	3	23	5	2	6	6	2	12	10	4	5	2	2	...	...	...	65	42	13	120
Zusammen	Behandelte.	7	65	...	87	197	3	1,267	330	42	1,824	244	29	513	74	13	315	61	24	52	17	8	5	1	...	4,063	924	119	5,106
	Verstorbene.	...	44	...	21	122	...	41	101	3	48	29	2	19	7	2	25	10	4	9	2	2	1	...	...	164	271	13	448

IX. SONSTIGE KRANKHEITEN.

Krankheiten.		Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Total.
Angeborene Schwäche (Debilitas Congenita)	Behandelte.	1	51	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
	Verstorbene.	...	14	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
Altersschwäche (Marasmus Senilis)	Behandelte.	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	77	52	22	...	...	...	77	52	22	151
	Verstorbene.	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	38	30	10	...	...	...	38	30	10	78
Sonstige Krankheiten	Behandelte.	1	5	...	33	15	...	262	27	9	440	50	18	211	23	17	260	68	30	113	27	9	4	2	1	1,323	212	84	1,619
	Verstorbene.	...	...	...	3	4	...	5	...	1	9	1	5	11	1	6	29	9	8	12	3	5	1	1	1	70	19	26	115
Summe der verschiedenen Krankheiten. Budapest	Behandelte.	1	12	1	21	7	...	68	4	2	57	4	2	47	6	3	50	5	3	46	5	4	1	...	...	290	31	14	335
	Verstorbene.	...	8	1	2	2	...	...	...	...	1	...	...	2	...	...	9	2	...	18	1	1	...	...	...	32	5	1	38
Provinz	Behandelte.	1	44	...	12	8	...	194	23	7	383	46	16	164	17	14	210	63	27	144	74	27	3	2	1	1,110	233	92	1,435
	Verstorbene.	...	6	...	1	2	...	5	...	1	8	1	5	9	1	6	20	7	8	32	32	14	1	1	1	76	44	35	155
Zusammen	Behandelte.	2	56	1	33	15	...	262	27	9	440	50	18	211	23	17	260	68	30	190	79	31	4	2	1	1,400	264	106	1,770
	Verstorbene.	...	14	1	3	4	...	5	...	1	9	1	5	11	1	6	29	9	8	50	33	15	1	1	1	108	49	36	193

X. UNFÄLLE (GEWALTSAME TODESURSACHEN).

Krankheiten.			0-1 Jahr.			1-5 Jahre.			5-20 Jahre.			20-30 Jahre.			30-40 Jahre.			40-60 Jahre.			Über 60 Jahre.			Unbekanntes Alter.			Summe der über Einjährigen.			
			Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Total.
Summe der gewaltsamen Todesursachen.	Budapest	Behandelte.	...	2	2	13	6	1	69	1	1	91	7	2	83	7	5	91	9	4	14	3	2	2	...	...	363	33	15	411
		Verstorbene.	...	2	2	1	...	1	9	...	...	11	1	...	6	1	2	11	1	2	1	...	...	1	...	...	40	3	5	48
	Provinz	Behandelte.	...	...	...	7	2	...	194	20	7	285	40	10	222	32	12	233	79	16	49	29	5	1	...	...	991	202	50	1,243
		Verstorbene.	...	...	...	...	...	...	11	...	...	14	1	...	7	3	1	8	4	1	4	2	...	...	...	...	44	10	2	56
	Zusammen	Behandelte.	...	2	2	20	8	1	263	21	8	376	47	12	305	39	17	324	88	20	63	32	7	3	...	...	1,354	235	65	1,654
		Verstorbene.	...	2	2	1	...	1	20	...	...	25	2	...	13	4	3	19	5	3	5	2	...	1	...	...	84	13	7	104
Sämmtliche Krankheiten. Hauptsumme.																														
Sämmtliche Krankheiten. Hauptsumme.	Budapest	Behandelte.	12	95	5	339	233	3	1,402	270	19	1,672	157	32	902	78	24	1,038	75	41	268	51	26	10	...	...	5,631	864	148	6,643
		Verstorbene.	3	60	4	73	143	1	92	97	1	117	33	2	99	10	5	148	9	8	70	13	9	4	...	...	603	305	26	934
	Provinz	Behandelte.	26	85	...	99	62	3	2,342	316	72	3,988	520	109	1,960	288	96	2,112	553	157	609	291	96	25	8	2	11,135	2,038	535	13,708
		Verstorbene.	1	19	...	10	14	1	86	20	6	178	22	14	126	38	18	236	83	30	106	69	31	3	1	2	745	247	102	1,094
	Zusammen	Behandelte.	38	180	5	438	295	6	3,744	586	91	5,660	677	141	2,862	366	120	3,150	628	201	877	342	122	35	8	2	16,766	2,902	683	20,351
		Verstorbene.	4	79	4	83	157	2	178	117	7	295	55	16	225	48	23	384	92	38	176	82	40	7	1	2	1,348	552	128	2,028
Impfzustand der Kranken in Procenten			17%	80.7%	2.3%	59.3%	39.9%	0.8%	84.7%	13.2%	2.1%	87.4%	10.4%	2.2%	85.4%	10.9%	3.7%	79.1%	15.8%	5.1%	25.5%	65.4%	9.1%	...	...	...	82.3%	14.3%	3.4%	100%
Summe der "Nichtblattern"			38	129	5	426	130	4	3,473	367	80	5,411	611	138	2,790	358	120	3,126	622	200	874	341	122	35	8	2	16,135	2,437	666	19,238
In Procenten			22.1%	75.0%	2.9%	76.1%	23.2%	0.7%	88.6%	9.4%	2.0%	87.8%	9.9%	2.3%	85.4%	10.9%	3.7%	79.2%	15.7%	5.1%	65.4%	25.5%	9.1%	77.8%	17.8%	4.4%	83.9%	12.7%	3.4%	100%

Gehen wir nun auf den Inhalt dieser Tabelle I über, so zeigt uns dieselbe folgendes Hauptergebniss für die Uebereinjährigen: [1]

	Geimpfte.	Ungeimpfte.	Zweifelhafte Fälle.	Zusammen.
Unter den an Blattern Erkrankten.....	631=56.7%	465=41.8%	17=1.5%	1,113
Unter den an sonstigen Krankheiten Erkrankten.....	16,135=83.9%	2,437=12.7%	666=3.7%	19,238
Zusammen.....	16,766	2,902	683	20,351

Unter 19,238 an Nichtblattern Erkrankten befanden sich also 2437 Ungeimpfte, d. i. 12.7%. Da nun die Anzahl der an Blattern Erkrankten 1113 betrug, wären für den Fall, als die Impfung keinen Schutz gegen die Erkrankung gewährte, auch bei den Blatternkranken 12.7% Ungeimpfte, d. i. 141 zu erwarten gewesen. Wären die Nichtvaccinirten, wie dies die Gegner behaupten, besser geschützt, als die Vaccinirten, so müsste die Anzahl der unter denselben Erkrankten hinter diesem Maasse zurückbleiben, während, falls diese weniger geschützt wären, deren Anzahl eine grössere sein müsste.

In Wirklichkeit ergibt sich nun, dass sich unter den an Blattern Erkrankten nicht 141, sondern 465 Ungeimpfte (=41.8) befanden! Es wird also bei Blattern die bei Ungeimpften bestehende (wahrscheinlich schon von Haus aus grössere) Erkrankungsdisposition *bloss in Folge der unterlassenen Impfung um das* $3\frac{1}{3}$*-fache gesteigert.*

Die Einwendung, dass bei den Ungeimpften das Kindesalter stärker besetzt sei, ist dieser Berechnung gegenüber nur theilweise berechtigt; die durch diesen Umstand hervorgerufene grössere Morbidität der Ungeimpften hat nämlich schon in dem Procentsatze von 12.7%, wenn auch nur theilweise, Ausdruck gefunden; überdies wurden doch die Untereinjährigen ganz ausser Rechnung gelassen. Um aber diesem berechtigten Einwande der Impfgegner Rechnung zu tragen, lassen wir hier die obige Intensitätsberechnung für jede Altersklasse folgen:

	NICHTBLATTERN.				BLATTERN.			
	Geimpft.	Nichtgeimpft.	Zweifelhaft.	Zus.	Geimpft.	Nichtgeimpft.	Zweifelhaft.	Zus.
0— 1 Jahr	38	129	5	172	—	51	—	51
1— 5 Jahre	426	130	4	560	12	165	2	179
5—20 "	3473	367	80	3920	271	219	11	501
20—30 "	5411	611	138	6160	249	66	3	318
30—40 "	2790	358	120	3268	72	8	—	80
40—60 "	3126	622	200	3948	24	6	1	31
Ueber 60 Jahre	874	341	122	1337	3	1	—	4
Alter unbekannt	35	8	2	45	—	—	—	—
Uebereinjährige	16,135 =83.9%	2,437 =12.7%	666 =3.7%	19,238	631 =56.7%	465 =41.8%	17 =1.5%	1113

Von je 100 Behandelten waren also nicht geimpft (Altersklassen mit unter 100 Fällen bleiben unberechnet und sind mit einem * bezeichnet):

	Bei Nichtblattern.	Bei Blattern.
1— 5 Jahre	23.2	92.2
5—20 "	9.4	43.7
20—30 "	9.9	20.8
30—40 "	11.0	*
40—60 "	15.8	*
Ueber 60 "	25.5	*

Also wenn Nichtblattern = 100 gesetzt werden, beträgt die relative Intensität (oder, wenn man die andere Bezeichnung vorzieht, die relative Häufigkeit):

Für die Altersklasse von 1— 5 Jahren 397, also eine 4-fache Steigerung der Erkrankungsgefahr.
" " " " 5—20 " 465, " " $4\frac{2}{3}$-fache " " "
" " " " 20—30 " 210, " " 2-fache " " "

[1] Um dem Einwand gerecht zu werden, wonach die grössere Blatternsterblichkeit der Ungeimpften sich aus der grösseren Sterblichkeit der (zumeist ungeimpften) Säuglinge überhaupt erkläre, lassen wir sämmtliche im ersten Lebensjahre Verstorbene ganz ausser Rechnung.

Wir ersehen nun aus dieser Tabelle, dass die Schutzkraft der Impfung für alle Altersklassen feststeht. Die präcisen Ergebnisse sind die folgenden:

1.—5. Lebensjahr; bei Nichtblattern bilden die Ungeimpften im Durchschnitte 23% der Erkrankten, bei Blattern hingegen 92%; in dieser Altersklasse sind also die Ungeimpften einer vierfach grösseren Gefährdung, an Blattern zu erkranken, ausgesetzt, als die Geimpften;

im Alter von 5—20 Jahren beträgt der Durchschnitt bei Nichtblattern 9.4%, bei Blattern aber 44%; der Schutz der Geimpften ist hier also ein 4½-facher;

im Alter von 20—30 Jahren beträgt der Durchschnitt 10%, bei Blattern hingegen 21%; der Schutz ist demnach zweimal stärker;

in den nachfolgenden Altersklassen bleibt die Anzahl der Blatternkranken unter 100 zurück, die Berechnung von Verhältnisswerthen wird in Folge dessen bedenklich. Soweit die vorhandenen Beobachtungen einen Schluss auf jene Richtung ziehen lassen, in welcher sich die Schutzkraft der Impfung verändert, sprechen dieselben ebenfalls entschieden für den grossen Schutz der Impfung.[1]

2. EINFLUSS DER IMPFUNG AUF DIE MORTALITÄT.

Das in Tabelle II enthaltene Material besteht aus den Aufzeichnungen der Sanitätsbeamten der Hauptstadt und neun ungarischer Provinzstädte über den Impfzustand

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[1] Das Maass der Vergleichung bildete in der obigen Berechnung die Summe der Nichtblatternfälle. Da man die Benützung dieses Durchschnittes als Einheitsmaass eventuell anfechten könnte, wollen wir im Nachfolgenden eine zweite Berechnung mit Hülfe eines anderen Maassstabes versuchen. Wir wollen nämlich jene Krankheiten, bez. Todesursachen auswählen, für die es im Vorherein absolut ausgeschlossen ist, dass die Impfung auf deren Auftreten von Einfluss sein könnte. Hieher zählen vor Allem Verunglückungen, Selbstmorde und alle gewaltsamen Todesursachen. Ferner wird man zugeben, dass auch die nachfolgenden Krankheiten, und zwar

Krankheiten des Nervensystems,
" " Gefässsystems,
Lungenentzündung,
Krankheiten der Verdauungsorgane,
" " Geschlechts- und Harnorgane,
Altersschwäche,

davon unabhängig sind, ob die Betreffenden geimpft gewesen oder nicht. In der Summe dieser gegen die Impfung indifferenten Krankheiten gewinnen wir zugleich einen Anhaltspunkt dafür, welcher Procentsatz den Ungeimpften in der Gesammtheit der Bevölkerung zukommt. Verhalten sich nun auch die Blattern der Impfung gegenüber indifferent, so wird auch hier sich ein gleich grosser Procentsatz an Ungeimpften finden müssen. Nun zeigt sich aber, dass die Ungeimpften unter den Blatternkranken verhältnissmässig häufiger sind, woraus also ersichtlich, dass die Impfung der Variola gegenüber nichts weniger als indifferent ist.

Die genannten indifferenten Krankheiten ergeben folgende Zahlen (siehe am Schlusse dieses Abschnittes):

	Geimpft.	Ungeimpft.	Zweifelhaft.	Zusammen.
0—1 Jahr	18	49	3	70
1—5 Jahre	111	44	1	156
5—20 "	1166	124	28	1318
20—30 "	1943	229	60	2232
30—40 "	1219	147	50	1416
40—60 "	1331	272	82	1685
Ueber 60 Jahre	407	181	57	645
Alter unbekannt	16	2	1	19
Summe der Uebereinjährigen	6193	999	279	7471
	= 82.9%	= 13.4%	= 3.7%	

Unter je hundert (übereinjährigen) Personen waren demnach 13.4 ungeimpft.

Hält man den aus diesen Zahlen abgeleiteten Verhältnissen diejenigen entgegen, welche sich

jeder auf ihrem Gebiete verstorbenen Person. Die Budapester Aufzeichnungen erstrecken sich auf nicht weniger als 422,557 Seelen und werden seit dem 1. April 1886 geführt. Ueberdies waren die Physikate von neun ungarischen Städten so freundlich, ähnliche Beobachtungen für dieselbe Zeit einzuführen, nämlich die Herren Stadtphysici

Dr. Julius Sárváry in Debreczen	für 55,256	Einwohner.
" Béla Tauscher in Pozsony (Pressburg)	48,737	"
" Johann Darányi in Arad	36,969	"
" Josef Novák in Makó	31,212	"
" Josef Fabritius in Brassó (Kronstadt)	30,491	"
" Franz Major in Székes-Fehérvár (Stuhlweissenburg)	27,990	"
" Wilhelm Markgraf in Jászberény	22,145	"
" Ludwig Petz in Györ (Raab)	21,454	"
" Koloman Jéger in Szatmár-Németi	20,384	"

Ich benutze diesen Anlass, um den genannten Herren meinen tiefsten Dank für ihre bereitwillige Unterstützung und für die konsequente Durchführung dieser Beobachtungen auszudrücken.

Alle genannten Beobachtungen erstrecken sich auf die Zeit vom 1. April bis 31. December 1886, die Beobachtungen in Jászberény auf das ganze Jahr 1886, da Herr Stadtphysikus Markgraf sich die Mühe genommen, den Impfzustand der Verstorbenen auch für die ersten drei Monate des Jahres durch Nachfragen bei den Angehörigen nachträglich festzustellen. In diesen drei Monaten kam in Jászberény kein Blatterntodesfall vor.

Insgesammt repräsentiren diese neun Provinzstädte eine nicht unbedeutend zu nennende Bevölkerung von 294,638 Seelen; mit Hinzurechnung der hauptstädtischen Bevölkerung erstrecken sich demnach meine Mortalitäts-Beobachtungen insgesammt auf 717,195 Personen.

In der Tabelle II folgt nun, für jede dieser Städte abgesondert, der Nachweis aller daselbst vorgekommenen Todesarten und zwar für jede der specificirten 45 Todesursachen mit Unterscheidung des Impfzustandes (geimpft, nicht geimpft, zweifelhaft) und zugleich des Alters (0–1, 1—5, 5—20, 20—30, 30—40, 40—60, über 60 Jahre, Alter unbekannt).[1]

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oben für die Blattern ergaben, das ist 41.8 %, so bemerkt man, dass auch nach dieser Berechnung sich für die Ungeimpften zusammen eine 3½-fache Steigerung der Gefahr, an Blattern zu erkranken, ergibt. Uebergeht man auf die einzelnen Altersklassen, so sieht man, dass von je 100 Uebereinjährigen ungeimpft waren

	bei den indifferenten Krankheiten	bei Blattern
im Alter von 1— 5 Jahren	28.2	92.2
" 5—20 "	9.4	43.7
" 20—30 "	10.3	20.8
" 30—40 "	10.4	*
" 40—60 "	16.1	*
" über 60 "	28.1	*

Wenn wir nun die Verhältnisszahlen der indifferenten Krankheiten = 100 setzen, so ergeben die Blatternfälle folgende Steigerung (= relative Intensität):

im Alter von 1— 5 Jahren	327,	also eine 3¼-fache Steigerung der Ekrankungsgefahr.
" 5—20 "	465,	" 4⅔ " " "
" 20—30 "	202,	" 2 " " "

Die oben gewonnenen Resultate gewinnen also auf diesem Wege ebenfalls ihre volle Bestätigung.

[1] Es mag hierbei bemerkt werden, dass eine derartige Specificirung der Todesursachen in der Mortalitätsstatistik Ungarns derzeit noch von besonderem Interesse ist. Hinsichtlich der Altersverhältnisse der Verstorbenen war nämlich die amtliche Landesstatistik bis heute nicht in der

Das Hauptergebniss der Tabelle No. II ergibt folgende Verhältnisse :

Es befanden sich unter den Uebereinjährigen

	Geimpfte,	Ungeimpfte,	Zweifelhafte Fälle,	Insgesammt, (= 100 pCt.)
unter den an Blattern Verstorbenen	239 = 18.3 %	1054 = 80.8 %	12 = 0.9 %	1305
unter den an sonstigen Krankh. Verstorbenen...	10,003 = 74.8 %	1839 = 13.8 %	1531 = 11.4 %	13,373
insgesammt.....	10,242	2893	1543	14,678

Während also bei allen anderen Todesursachen die grosse Mehrzahl der Verstorbenen — entsprechend der grossen Anzahl von Geimpften unter den Lebenden — aus Geimpften besteht, finden sich bei den Blattern-Verstorbenen fast nur Ungeimpfte! Von den 2900 ungeimpft Verstorbenen entfallen auf die vielen Hunderte der zum Hades führende Wege (auch Selbstmorde und Unglücksfälle mitgerechnet) nur an 1800 ; auf dem Wege der Blatternkrankheit aber, die kaum $\frac{1}{10}$ der totalen Todesfälle repräsentirt, erlagen nicht weniger als 1050 Ungeimpfte. Schon dieser Umstand allein lässt uns auf den grossen Schutz schliessen, dessen die Geimpften geniessen. Dies erhellt auch aus folgender Berechnung :

Unter 13,373 an Nichtblattern (d. h. an allen Todesursachen mit Ausnahme der Blattern) Verstorbenen gab es 1839 Ungeimpfte, also 13.8 %. In diesem Verhältnisse findet die von Impfgegnern behauptete (übrigens auch plausible) geringere Widerstandskraft der Ungeimpften und deren grössere Lebensgefährdung bereits ihren Ausdruck. Ist nun die Impfung ohne Einfluss auf die Gefahr, den Blattern zu erliegen, so müssten auch bei den Blatternfällen die Ungeimpften 13.8, das ist 80 Fälle, betragen. Wir sehen aber, dass deren faktisch nicht weniger als 81 %, das ist 1054, gezählt

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Lage, mehr als die Anzahl der Unterfünfjährigen nachzuweisen. Was aber die Todesursachen anbetrifft, so ist deren statistische Beobachtung bis zum heutigen Tage noch gar nicht möglich gewesen. (Das Ministerium des Innern veröffentlicht zwar eine Statistik der Todesursachen auf Grund der Todtenschauerberichte, doch umfasst diese Veröffentlichung nur 60—70 pCt. der Sterbefälle.) Für die in Ungarn herrschenden Todesursachen bieten demnach, ausser meinen ausführlichen Arbeiten über die Mortalität der Hauptstadt, die in dem von mir seit neun Jahren redigirten "Bulletin hebdomadaire de statistique internationale" enthaltenen Mittheilungen über die demographischen Verhältnisse in mehreren Städten Ungarns, leider noch die einzige Quelle. In Folge meines vor einem Jahrzehnt an verschiedene Stadtverwaltungen Ungarns ergangenen Ansuchens, hat nämlich eine Reihe derselben beschlossen, Aufzeichnungen über die Geburten und Todesfälle ihrer Stadt nach dem im Budapester internationalen statistischen Congress angenommenen Schema zu verfassen und mir regelmässig zur Publikation einzusenden. Diese Städte sind: Szeged, Szabadka (Maria-Theresiopel), Debreczen, Pozsony (Pressburg), Kecskemét, Arad, Temesvár, B.-Csaba, Nagyvárad (Grosswardein), Kassa (Kaschau), Makó, Pécs (Fünfkirchen), Brassó (Kronstadt), Kolozsvár (Klausenburg), Székes-Fehérvár (Stuhlweissenburg), Czegléd, Sopron (Oedenburg), Miskolcz, Nyiregyháza, Kunfélegyháza, Nagy-Körös, Versecz, Békés, Jászberény, Györ (Raab), Szatmár-Németi und Baja.

Diese im Budapester statistischen Bureau aufbewahrten handschriftlichen Mittheilungen über die demographischen Verhältnisse ungarischer Städte bilden also unter den gegenwärtigen Verhältnissen einen werthvollen Beitrag zur Kenntniss der Mortalitätsverhältnisse unseres Landes. (Ueberdies veröffentlichte dieses Bulletin die erwähnten demographischen Daten noch für die folgenden grösseren, zumeist südost-europäischen, Städte: Wien, Prag. Triest, Lemberg, Gratz, Brünn, Krakau, Czernowitz, Olmütz, Zara, Serajewo, Venedig, Odessa, Cherson, Bukarest, Jassy, Galatz, schliesslich auch für Cairo und Alexandrien.) Diese Mittheilungen erstrecken sich aber bloss auf 14 Todesursachen und zwar ohne Unterscheidung der Altersklassen. Indem also gegenwärtig für neun ungarische Provinzstädte eine nach 45 Todesursachen, und zwar, in Combination mit den Altersverhältnissen, aufgearbeitete Mortalitätsstatistik geboten wird, dürfte man den diesbezüglichen Nachweisungen — abgesehen von ihrem speciellen vaccinationsstatistischen Inhalte — auch ein allgemeines Interesse zuerkennen.

wurden! Die relative Intensität beträgt demnach für Ungeimpfte an 586, d. h. die Ungeimpften laufen, nur in Folge der unterlassenen Impfung, *nahezu sechsfach grössere Gefahr, den Blattern zu erliegen*, als die Geimpften.[1]

Wenn wir nun wieder auf die Specificirung der Altersklassen eingehen, gelangen wir zu folgenden Ergebnissen:

	NICHTBLATTERN.				BLATTERN.			
	Geimpfte.	Nichtgeimpfte.	Zweifelhaft.	Zus.	Geimpfte.	Nichtgeimpfte.	Zweifelhaft.	Zus.
0— 1 Jahr	499	4183	631	5313	52	302	2	316
1— 5 Jahre	2002	1150	67	3219	57	577	7	641
5—20 "	1425	150	76	1651	74	372	2	448
20—30 "	1245	55	167	1467	49	82	1	132
30—40 "	1262	57	220	1539	36	13	2	51
40—60 "	2210	146	392	2748	18	8	—	26
über 60 "	1841	271	591	2703	5	2	—	7
Alter unbekannt....	18	10	18	46	—	—	—	—
Zusammen	10,003	1839	1531	13373	239	1054	12	1305
Uebereinjährige..	— 74.8 %	13.8 %	11.4 %		— 18.3 %	80.8 %	0.9 %	

Von je 100 Verstorbenen waren also nicht geimpft

	bei Nichtblattern,	bei Blattern,	also, falls Nichtblattern — 100 (relative Intensität),
1— 5 Jahre.....	35.7	90.0	252
5—20 "	9.1	83.0	912
20—30 "	3.7	62.1	1678
30—40 "	3.7	*	*
40—60 "	5.3	*	*
über 60 "	10.0	*	*

Die Nichtimpfung erhöht also die Gefahr, an Blattern zu sterben, bei den

1— 5-jährigen um das 2½fache,
5—20 " " " 9fache,
20—30 " " " .....17fache.[2]

---

[1] Die in vier Budapester und in 15 Provinzspitälern aufgenommenen Erkrankten ergaben 12.7 % Ungeimpfter bei Nichtblattern. In obigem Falle, der sich auf beinahe total verschiedene Bevölkerungsgruppen bezieht, die überwiegend hauptstädtischen Beobachtungen entnommen sind, während die Morbiditätsbeobachtungen überwiegend der Provinzbevölkerung entstammen, ergab sich ein Verhältniss von 13.8 %. Diese frappante Uebereinstimmung kann füglich als Beweis der Verlässlichkeit der Aufzeichnungen betrachtet werden.

[2] Wie für die Morbidität, wollen wir auch für die Mortalität die zweite Berechnungsweise, wo die Impfverhältnisse der "indifferenten" Krankheiten den Maassstab abgeben, parallel durchführen.

Die indifferenten Krankheiten ergeben folgende Zahlen (siehe am Schlusse dieses Abschnittes):

	Geimpfte.	Ungeimpfte.	Zweifelhaft.	Zusammen.
0— 1 Jahr.....	326	2464	523	3313
1— 5 Jahre.....	880	589	35	1504
5—20 "	479	44	31	554
20—30 "	367	14	89	470
30—40 "	428	21	99	548
40—60 "	1096	70	227	1393
über 60 "	1303	219	431	1953
Alter unbekannt.....	13	7	13	33
Summe der Uebereinjährigen	4566 — 70.8 %	964 — 14.9 %	925 — 14.3 %	6455

Hält man die aus dieser Zusammenstellung sich ergebenden Verhältnisszahlen denjenigen

Wir können diesen Daten noch jene beifügen, welche sich unserer I. Tabelle entnehmen lassen, die von in verschiedenen hauptstädtischen und Provinz-Spitälern entnommenen Mortalitäts-Aufzeichnungen herrührt. Da aber diese Spitalsbeobachtungen sich besser zur Einsicht in die Lethalitätsverhältnisse eignen, wollen wir hier nur des Hauptresultates erwähnen, wonach nämlich unter den daselbst verstorbenen Uebereinjährigen sich befinden:

	Geimpfte,	Ungeimpfte,	Zweifelhafte Fälle,	Insgesammt,
bei den an Blattern Verstorbenen	42	231	3	276
bei den an sonstigen Krankheiten Verstorbenen.....	1306	321	125	1752

Im Durchschnitte (der Nichtblatternfälle) betragen also, selbst bei der ärmeren und ungebildeteren Spitalsbevölkerung, die Ungeimpften nur mehr 18½ der Uebereinjährigen, bei den Blattern aber 83.7 % ! Die durch die Nichtimpfung hervorgerufene Gefährdung, den Blattern zu erliegen, beträgt also auch bei diesem Bevölkerungsgemengsel mehr als das 4½-fache !

3. EINFLUSS DER IMPFUNG AUF DIE LETHALITÄT.

Wie aus den reichen Aufzeichnungen anderer Spitäler, so ergibt sich auch aus unseren 1113 Blatternerkrankungen umfassenden Beobachtungen, eine erheblich, nämlich *achtfach* grössere Lethalität der Ungeimpften gegenüber jener der Geimpften.

Es betrug nämlich unter den Uebereinjährigen die Anzahl der an Blattern

	Erkrankten	Verstorbenen	Also Lethalität
bei den Geimpften	631	42	6,66%
" " Ungeimpften	465	231	49,68% (!)
" " zweifelhaften Fällen	17	3	

Die Blatternlethalität der Ungeimpften übersteigt also jene der Geimpften nahezu um das *Achtfache!*[1]

Es ist vielleicht überflüssig, hier noch einmal auf die bekannten zwei Einwendungen der Impfgegner zurückzukommen, als ob nämlich diese grössere Lethalität nicht eine Folge der unterlassenen Impfung, sondern einerseits der stärkeren Besetzung des Kindesalters unter den Nichtgeimpften, andererseits ihrer von Haus aus schwächeren Widerstandskraft wäre, — Einwände, deren wir im Vorhergegangenen stets Rechnung getragen und die wir auf ihren wahren Werth reducirt haben. Trotzdem wollen wir, um dieses gewichtige Gegenargument des Impfangriffes nach Gebühr gewürdigt zu haben, demselben hier noch einmal begegnen.

---

gegenüber, welche für die Blatternmortalität gewonnen wurden, so sehen wir, dass von je 100 Personen einer Altersklasse ungeimpft waren

	bei den indifferenten Krankheiten.	bei Blattern,
im Alter von 1— 5 Jahren.....	39.2	90.0
" 5—20 "	7.9	83.0
" 20—30 "	3.0	62.1
" 30—40 "	3.8	*
" 40—60 "	5.0	*
" über 60 "	11.2	*

Wenn wir nun die Verhältnisszahl der indifferenten Krankheiten = 100 setzen, so ergeben die Blatternfälle folgende Steigerung (relative Intensität):

im Alter von 1— 5 Jahren.....	230,	d. i. um mehr als das 2 fache,
" 5—20 " 1050,		" " " " 10 fache,
" 20—30 " 2070,		" " " " 20 fache.

Auch nach dieser Berechnungsweise zeigt sich also, dass der Schutz der Impfung sich auf alle Altersklassen — soweit nämlich genügendes Material vorlag — erstreckt.

[1] Genauer gesprochen, um 752 pCt.

Mit Rücksicht auf die Einwendung der verschiedenen Altersbesetzung, lassen wir hier wieder die Lethalität für jede der angeführten Altersklassen folgen.

Es ergibt sich hierbei aus der Tabelle No. II folgendes Resultat:

	Anzahl der an Blattern erkrankten — verstorbenen GEIMPFTEN		erkrankten — verstorbenen UNGEIMPFTEN	
im Alter von 0— 1 Jahr	—	—	51	38
" " " 1— 5 "	12	1	165	112
" " " 5—20 "	271	7	219	94
" " " 20—30 "	249	18	66	24
" " " 30—40 "	72	9	8	1
" " " 40—60 "	24	5	6	—
" " über 60 "	3	2	1	—
Zusammen über ein Jahr	631	42	465	231

Soweit nun eine Percentualberechnung erlaubt ist, nämlich für jene Altersklassen, in denen wenigstens hundert Erkrankungen vorkamen, zeigt sich, dass verstorben sind:

	von Geimpften,	von Ungeimpften,	also für Ungeimpfte, falls Geimpfte = 100,
im Alter von 5—20 Jahren	2.6%	42.0%	1650
" " " 20—30 "	7.2%	36.4%	506

Die Lethalität der Ungeimpften ist also im Kindesalter eine sechzehnfach, im Mannesalter eine fünffach grössere!

Was den zweiten Einwand betreffs der geringeren Widerstandskraft der Ungeimpften betrifft, so ist derselbe theoretisch gewiss berechtigt: es fragt sich nur stets, *wie viel* auf Rechnung dieser geringeren Widerstandsfähigkeit zu schreiben sei. Hierauf gibt nun unsere Methode, welche das Maass dieses Widerstandes erkennen lässt, vollkommene Antwort.

Es wurden nämlich behandelt im Alter von über einem Jahre an allen Krankheiten, mit Ausnahme der Blattern:

	Geimpfte	Ungeimpfte
	16,135	2,437
hievon starben	1,306	321
demnach Lethalität, ...	8.1%	13.2%

Es war also, wenn man von den Blattern absieht, die Lethalität der Ungeimpften in allen übrigen Krankheiten eine um die Hälfte grössere, als die der Geimpften. Wir haben aber soeben ersehen, dass in Blatternfällen die Lethalität der Ungeimpften eine nahezu *achtfach* grössere ist. Wenn es nun auch unrichtig wäre, diese ganze Steigerung von 800% auf Rechnung der Nichtimpfung zu stellen, so kann doch nicht geläugnet werden, dass eine nahezu 5½-fache Steigerung zu Lasten der Nichtimpfung zu schreiben[1], d. h. als die Wirkung dieses Umstandes zu betrachten ist; mit andern Worten dass die Ungeimpften — unter sonst ganz gleichen Umständen — darauf rechnen müssen, im Falle der Erkrankung, *bloss in Folge des Ungeimpftseins, beinahe fünffach häufiger den Blattern zu erliegen!*

Wie eben gezeigt, wird der Einwand der Impfgegner, dass die Ungeimpften schon von Haus aus eine geringere Widerstandskraft besässen, von unseren Beobachtungen vollkommen bestätigt, und zwar werden von ungeimpften Kranken stets um die Hälfte mehr erliegen, als von geimpften. Da wir in diese Frage schon eingegangen sind, wollen wir es nicht unterlassen, hier auch

[1] Die geringere Widerstandskraft dieser Gruppe steigerte deren Lethalität von 8.1 auf 13.2, d. h. von 100 auf 163, deren Ungeimpftheit aber von 163 auf 752 (s. vorige Seite) = 461 pCt.

noch auf jene interessante Untersuchung einzugehen, ob und in welcher Weise sich diese gesteigerte Lethalität in einzelnen Altersklassen verändere. Eine derartige Vergleichung der Lethalitätsziffern für die einzelnen Altersklassen führt zu folgendem Ergebniss:

	a) FÜR GEIMPFTE.			b) FÜR NICHTGEIMPFTE.		
	Erkrankt.	Gestorben.	Lethalität.	Erkrankt.	Gestorben.	Lethalität.
0— 1 Jahr	38	4	*	129	41	31.8 %
1— 5 Jahre	426	82	19.2 %	130	45	34.6
5—20 "	3,473	171	4.9	367	23	6.3
20—30 "	5,411	277	5.3	611	31	5.1
30—40 "	2,790	216	7.7	358	47	13.1
40—60 "	3,126	379	12.1	622	92	14.7
Ueber 60 Jahre	874	174	19.9	341	82	24.0
Alter unbekannt	35	7	*	8	1	*
Summe d. Uebereinjährigen	16,135	1,306	8.1	2,437	321	13.2

Vergleicht man die für jede Altersklasse gewonnene Lethalitätsziffer der Geimpften und Ungeimpften, so findet man, dass bei den Ungeimpften die ungünstigste Erhöhung, nämlich nahezu auf das Doppelte der Lethalität im Kindesalter (1—5 Jahre) und im Alter von 30—40 Jahren eintrat, während im Alter von 20—30 Jahren die Lethalität der Ungeimpften genau so viel wie jene der Geimpften betrug. Nimmt man nämlich die Lethalität der Letzteren gleich 100, so betrug die der Ungeimpften im Alter von

1— 5 Jahren,	180	30—40 Jahren,	170
5—20 "	129	40—60 "	121
20—30 "	100	über 60 "	121

In diesen Zahlen hätten wir demnach den allgemeinen Ausdruck für die in verschiedenen Lebensaltern wechselnde geringere Widerstandskraft der Ungeimpften gefunden. Will man diese Ziffern nun mit dem Ausschlage vergleichen, der sich speciell für Blattern ergibt, so steht uns genügendes Material leider nur für zwei Altersklassen zur Verfügung, und zwar für die Altersgruppen von 5—20 und 20—30 Jahre. Hier beträgt die Blatternlethalität der Ungeimpften, wenn jene der Geimpften gleich 100 gesetzt wird, im Alter von

5—20 Jahren, 1650 20—30 Jahren, 506.

Zieht man nun die normale Ueberlethalität der Ungeimpften in Rechnung, so ergibt sich, dass für die Altersklasse von 5—20 Jahren, wo die Lethalität der Ungeimpften bei Blattern im Ganzen 16½-mal ungünstiger ist, hiervon auf Rechnung der Nichtimpfung allein, eine 13-fache Verschlimmerung zu setzen ist. In der nächsten Altersklasse, wo die Lethalität der Ungeimpften sich von jener der Geimpften im Allgemeinen nicht unterscheidet, bei Blattern aber eine fünffache Verschlimmerung zu constatiren ist, kommt diese ganze Steigerung zu Lasten der Nichtimpfung zu stehen.

Hiermit wäre nun die Frage des durch die Vaccination gebotenen Schutzes nach allen Seiten, und zwar in einer *für die Impftheorie überaus günstigen Weise erledigt.* Es erübrigt demnach nur noch die wichtige Frage von der Ueberimpfung der Krankheiten.

TODESURSACHEN IN BUDAPEST UND IN NEUN UNGARISCHEN PROVINZ-STÄDTEN, NACH IMPFZUSTAND UND ALTER (1886).

1. HIRNHAUTENTZÜNDUNG (MENINGITIS).

Städte.	0–1 Jahr.			1–5 Jahre.			5–20 Jahre.			20–30 Jahre.			30–40 Jahre.			40–60 Jahre.			Über 60 Jahre.			Alter unbekannt.			Summe der Über-einjährigen.			
	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Zusammen.
Budapest	8	41	4	77	31	2	57	7	2	12	...	2	14	...	...	10	...	1	5	...	2	...	...	...	175	38	9	222
Debreczen	...	...	...	...	...	...	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	1	...	...	1
Pressburg	3	4	...	5	1	...	5	...	...	4	...	...	...	...	...	1	...	...	...	...	...	...	...	...	15	1	...	16
Arad	1	...	...	6	...	...	4	...	...	2	...	...	...	...	...	1	...	1	...	...	...	...	...	...	13	...	1	14
Makó	...	...	...	...	...	...	...	...	...	1	...	...	...	...	...	1	...	...	...	...	...	...	...	...	2	...	...	2
Cronstadt	...	4	2	5	5	1	6	1	...	1	...	...	1	...	...	...	...	...	...	...	...	4	3	...	17	9	1	27
Stuhlweissenburg	...	4	...	10	1	...	5	...	...	...	...	1	...	...	1	1	...	2	...	...	...	...	...	...	16	1	4	21
Jászberény	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
Raab	...	1	...	5	...	...	2	...	...	...	...	...	1	...	...	...	...	...	...	...	...	...	...	...	8	...	...	8
Szatmár-Németi	1	10	...	4	1	...	3	...	1	1	1	...	2	...	1	...	...	...	...	...	...	...	...	...	10	2	2	14
Zusammen	13	64	6	112	39	3	83	8	3	21	1	3	18	...	2	14	...	4	5	...	2	4	3	...	257	51	17	325

2. GEHIRNENTZÜNDUNG (ENCEPHALITIS).

Budapest	3	5	1	17			8	...	...	1	...	...	...	...	...	2	...	...	...	...	1	...	...	...	28	7	2	37
Debreczen	1	1	1	6	2		9	2	...	2	...	...	...	...	...	2	...	...	1	...	...	...	...	...	20	4	...	24
Pressburg	...	...	...	...			1	..	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	1	...	...	1
Arad	2	2	...	15	1		9	...	...	2	...	...	1	...	...	...	...	...	...	...	...	...	...	...	27	1	...	28
Makó	...	...	...	...			...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
Cronstadt	...	...	...	...			...	...	1	1	...	...	...	...	...	...	...	...	1	...	...	...	...	...	2	...	1	3
Stuhlweissenburg	...	...	...	1	1		1	...	...	...	...	...	1	...	...	...	...	...	...	...	...	...	...	...	3	1	...	4
Jászberény	...	...	...	1	1		5	...	...	1	...	...	...	...	...	4	...	...	1	...	...	...	...	...	12	1	...	13
Raab	1	...	...	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	1	...	...	1
Szatmár-Németi	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
Zusammen	7	8	2	41	12	1	33	2	1	7	...	...	2	...	...	8	...	...	3	...	1	...	...	...	94	14	3	111

3. WASSERKOPF (HYDROCEPHALUS).

Städte.	0–1 Jahr.			1–5 Jahre.			5–20 Jahre.			20–30 Jahre.			30–40 Jahre.			40–60 Jahre.			Über 60 Jahre.			Alter unbekannt.			Summe der Über-einjährigen.			
	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Zusammen.
Budapest	1	17	2	13	10	...	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	14	10	...	24
Debreczen	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
Pressburg	2	9	...	3	2	...	3	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	6	2	...	8
Arad	2	2	...	4	...	...	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	5	...	...	5
Makó	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
Cronstadt	...	2	...	3	2	...	...	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	3	3	...	6
Stuhlweissenburg	...	...	...	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	1	...	...	1
Jászberény	...	...	...	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	1	...	...	1
Raab	...	1	...	...	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	1	...	1
Szatmár-Németi	...	...	...	1	...	...	...	...	...	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	2	...	...	2
Zusammen	5	31	2	26	15	...	5	1	...	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	32	16	...	48

4. GEHIRNSCHLAG UND LÄHMUNG (APOPLEXIA ET PARALYSIS).

Budapest	...	...	...	...		...	4		...	2	...	3	18	2	6	60		14	71	2	35	2	...	...	157	6	58	221
Debreczen	...	1	...	...		...	...		...	...	...	...	3	...	...	3		...	11	3	2	...	...	...	17	4	2	23
Pressburg	...	...	...	...		...	...		...	1	...	...	1	...	...	5		...	18	1	...	...	...	...	25	3	...	28
Arad	...	...	...	...		...	...		...	...	1	...	3	...	1	11		2	9	1	5	...	...	...	23	2	8	33
Makó	...	...	...	...		...	...		...	2	...	...	1	...	...	4		...	7	4	...	1	...	...	15	4	...	19
Cronstadt	...	...	...	...		...	...		...	...	...	...	1	...	...	6		...	5	...	2	...	...	...	12	...	2	14
Stuhlweissenburg	...	...	...	...		...	...		...	...	...	...	...	...	1	1		8	...	1	14	...	...	2	1	1	25	27
Jászberény	...	...	...	1		...	2		...	...	...	...	2	...	...	3		...	5	...	...	...	...	...	13	...	...	13
Raab	...	...	...	1		...	...		...	1	...	...	...	...	...	3		...	2	...	...	...	...	...	7	...	...	7
Szatmár-Németi	...	...	...	...		...	...		...	...	...	...	...	...	...	4		...	5	6	1	...	...	...	9	6	1	16
Zusammen	...	1	...	2		...	6		...	6	1	3	29	2	8	100	4	24	133	18	59	3	...	2	279	26	96	401

5. KRÄMPFE (CONVULSIONES).

Budapest	9	214	102	40	37	3	7	1	...	2	...	...	...	...	...	2	...	...	...	...	...	...	...	...	51	38	3	92
Debreczen	8	87	...	8	2	...	...	...	...	...	...	...	2	...	...	1	...	...	...	...	...	...	...	...	11	2	...	13
Pressburg	1	20	...	3	3	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	3	3	...	6
Arad	4	55	...	6	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	6	1	...	7
Makó	2	78	7	11	2	1	3	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	14	2	1	17
Cronstadt	...	8	1	1	4	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	1	4	1	6
Stuhlweissenburg	...	16	...	1	...	...	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	2	...	...	2
Jászberény	21	34	...	9	1	...	...	...	...	...	...	...	1	...	...	2	...	...	2	...	...	...	3	...	14	4	...	18
Raab	7	48	...	7	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	7	1	...	8
Szatmár-Németi	...	54	...	...	3	...	1	...	...	1	...	...	...	...	...	1	1	...	...	...	...	...	...	...	3	4	...	7
Zusammen	52	614	110	86	54	5	12	1	...	3	...	...	3	...	...	6	1	...	2	...	...	...	3	...	112	59	5	176

6. SONSTIGE NERVENKRANKHEITEN.

Städte.	0-1 Jahr.			1-5 Jahre.			5-20 Jahre.			20-30 Jahre.			30-40 Jahre.			40-60 Jahre.			Über 60 Jahre.			Alter unbekannt.			Summe der Übereinjährigen.			
	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Zusammen.
Budapest	3	9	3	15	8	...	16	2	3	20	1	13	38	6	10	90	10	34	27	3	7	...	...	...	206	30	67	303
Debreczen	...	...	...	...	...	...	...	...	...	...	...	...	1	...	1	1	...	1	...	...	...	...	...	...	2	...	2	4
Pressburg	...	12	...	2	...	...	4	...	...	1	...	...	4	...	1	7	3	1	4	2	1	...	...	...	22	5	3	30
Arad	...	2	...	1	1	...	3	...	...	2	...	...	3	...	...	4	...	...	2	...	...	...	...	...	15	1	...	16
Makó	...	...	...	...	...	1	...	...	...	1	...	...	...	...	...	3	...	...	...	...	...	...	...	...	4	...	1	5
Cronstadt	...	1	...	...	2	...	1	...	...	...	...	...	...	...	...	2	...	...	2	...	...	...	...	...	5	2	...	7
Stuhlweissenburg	1	...	...	...	...	...	...	...	...	1	...	2	...	...	...	...	...	1	2	...	2	...	...	...	3	...	5	8
Jászberény	...	...	...	1	...	...	1	...	...	...	...	...	...	...	...	1	...	...	...	...	...	...	...	...	3	...	...	3
Raab	...	...	...	...	...	...	...	...	...	...	...	...	4	...	...	1	...	...	2	...	...	...	...	...	7	...	...	7
Szatmár-Németi	...	...	...	...	...	...	2	...	...	...	...	...	1	...	...	2	1	...	3	2	...	...	...	...	8	3	...	11
Zusammen	4	24	3	19	11	1	27	2	3	25	1	15	51	6	12	111	14	37	42	7	10	...	...	...	275	41	78	394

1-6. SUMME DER NERVENKRANKHEITEN.

Städte.	0-1 Jahr.			1-5 Jahre.			5-20 Jahre.			20-30 Jahre.			30-40 Jahre.			40-60 Jahre.			Über 60 Jahre.			Alter unbekannt.			Summe der Übereinjährigen.			
	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Zusammen.
Budapest	24	286	112	162	94	6	93	10	5	37	1	18	70	8	16	164	11	49	103	5	45	2	...	...	631	129	139	899
Debreczen	9	89	1	14	4	...	10	2	...	2	...	...	6	...	1	7	1	1	12	3	2	...	...	...	51	10	4	65
Pressburg	6	45	...	13	6	...	13	...	...	6	...	...	5	...	1	13	5	1	22	3	1	...	...	...	72	14	3	89
Arad	9	61	...	32	3	...	17	...	...	6	1	...	7	...	1	16	...	3	11	1	5	...	...	...	89	5	9	103
Makó	2	78	7	11	2	2	3	...	...	4	...	...	1	...	...	8	...	...	7	4	...	1	...	...	35	6	2	43
Cronstadt	...	15	3	9	13	2	7	2	1	2	...	...	2	...	...	8	...	...	8	...	2	4	3	...	40	18	5	63
Stuhlweissenburg	1	20	...	13	2	...	7	...	...	1	...	3	1	...	2	2	...	11	2	1	16	...	...	2	26	3	34	63
Jászberény	21	34	...	13	2	...	8	...	...	1	...	...	3	...	...	10	...	...	8	...	...	...	3	...	43	5	...	48
Raab	8	50	...	14	2	...	2	...	...	1	...	...	5	...	...	4	...	...	4	...	...	...	...	...	30	2	...	32
Szatmár-Németi	1	64	...	5	4	...	6	...	1	3	1	...	3	...	1	7	2	...	8	8	1	...	...	...	32	15	3	50
Zusammen	81	742	123	286	132	10	166	14	7	63	3	21	103	8	22	239	19	65	185	25	72	7	6	2	1,049	207	199	1,455

7. ORGANISCHE HERZFEHLER (VITIA CORDIS ORGANICA).

Städte.	0-1 Jahr.			1-5 Jahre.			5-20 Jahre.			20-30 Jahre.			30-40 Jahre.			40-60 Jahre.			Über 60 Jahre.			Alter unbekannt.			Summe der Übereinjährigen.			
	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Zusammen.
Budapest	...	1	...	2	1	...	15	1	...	24	...	2	31	1	7	113	3	12	73	3	20	...	...	...	258	9	41	308
Debreczen	...	1	...	...	...	...	8	1	...	3	...	...	...	...	...	8	...	5	10	...	2	...	...	...	24	1	7	32
Pressburg	...	...	...	...	...	...	3	...	...	3	...	...	3	1	...	19	3	...	19	4	1	...	...	...	47	8	1	53
Arad	1	...	...	...	...	...	2	...	...	2	...	...	2	...	...	8	1	1	6	...	1	...	...	...	20	1	2	23
Makó	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	1	...	...	1	...	...	...	...	...	2	...	...	2
Cronstadt	...	...	...	...	...	...	1	...	...	...	...	1	...	...	...	4	...	...	1	...	...	...	...	...	6	...	1	7
Stuhlweissenburg	...	...	...	...	...	...	1	...	...	1	2	...	1	1	1	1	...	1	1	...	3	1	...	...	6	3	5	14
Jászberény	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
Raab	...	...	...	...	...	...	...	...	...	1	...	...	...	...	...	...	...	...	2	...	...	...	...	...	3	...	...	3
Szatmár-Németi	...	...	...	...	...	...	...	...	...	1	...	...	3	...	1	3	3	...	4	1	1	...	...	...	11	4	2	17
Zusammen	1	2	...	2	1	...	25	2	...	35	2	3	40	3	9	157	10	19	117	8	28	1	...	...	377	26	59	462

Städte.	0-1 Jahr.			1-5 Jahre.			5-20 Jahre.			20-30 Jahre.			30-40 Jahre.			40-60 Jahre.			Über 60 Jahre.			Alter unbekannt.			Summe der Überjährigen.			
	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Zusammen.
Budapest	...	...	1	2	1	...	10	...	...	9	...	1	19	...	2	41	1	12	27	2	8	1	...	1	109	4	24	137
Debreczen	...	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	2	1	...	...	...	...	2	1	...	3
Pressburg	...	1	...	...	...	...	...	...	...	1	...	...	2	...	...	4	...	...	7	...	...	...	...	...	14	...	...	14
Arad	...	...	...	...	...	...	1	...	...	1	...	...	2	...	...	2	1	2	7	2	2	...	...	...	13	3	2	18
Makó	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	1	...	...	...	...	...	...	...	...	1	1
Cronstadt	...	...	...	...	...	...	1	...	...	...	...	...	...	...	...	4	...	...	1	...	...	...	...	...	6	...	...	6
Stuhlweissenburg	...	...	...	...	...	...	1	...	...	...	...	...	...	...	...	...	...	1	1	...	...	...	...	...	2	...	1	3
Jászberény	...	1	...	...	...	...	1	...	...	...	...	...	...	...	...	2	1	...	2	...	...	...	...	...	5	1	...	6
Raab	...	1	...	...	...	...	...	...	...	...	...	...	...	...	...	1	1	...	5	...	...	...	...	...	6	1	...	7
Szatmár-Németi	...	...	...	...	...	...	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	1	...	...	1
Zusammen	...	4	1	2	1	...	15	...	...	11	...	1	23	...	2	54	4	16	52	5	10	1	...	1	158	10	28	196
7-8. SUMME DER KRANKHEITEN DES GEFÄSSSYSTEMS.																												
Budapest	...	1	1	4	2	...	25	1	...	33	...	3	50	1	9	154	4	24	100	5	28	1	...	1	367	13	65	445
Debreczen	...	2	...	...	...	...	3	1	...	3	...	...	...	...	...	8	...	5	12	1	2	...	...	...	26	2	7	35
Pressburg	...	1	...	...	...	...	3	...	...	4	...	...	5	1	...	23	3	...	26	4	1	...	...	...	61	8	1	70
Arad	1	...	...	...	...	...	3	...	...	3	...	...	4	...	...	10	2	1	13	2	3	...	...	...	33	4	4	41
Makó	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	1	...	1	1	...	...	...	...	...	2	...	1	3
Cronstadt	...	...	...	...	...	...	2	...	...	...	...	1	...	...	...	8	...	...	2	...	...	...	...	...	12	...	1	13
Stuhlweissenburg	...	...	...	...	...	...	2	...	...	1	2	...	1	1	1	1	...	2	2	...	3	1	...	...	8	3	6	17
Jászberény	...	1	...	...	...	...	1	...	...	...	...	...	...	...	...	2	1	...	2	...	...	...	...	...	5	1	...	6
Raab	...	1	...	...	...	...	...	...	...	1	...	...	...	...	...	1	1	...	7	...	...	...	...	...	9	1	...	10
Szatmár-Németi	...	...	...	...	...	...	1	...	...	1	...	...	3	...	1	3	3	...	4	1	1	...	...	...	12	4	2	18
Zusammen	1	6	1	4	2	...	40	2	...	46	2	4	63	3	11	211	14	33	169	13	38	2	...	1	535	36	87	658
9. CROUP.																												
Budapest	2	5	1	83	31	1	27	5	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	110	36	1	147
Debreczen	...	1	...	13	4	1	2	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	15	4	1	20
Pressburg	...	1	...	6	2	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	6	2	1	9
Arad	1	...	...	5	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	5	1	...	6
Makó	...	...	...	5	1	...	2	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	1	...	7	2	...	9
Cronstadt	...	1	...	...	...	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	1	1
Stuhlweissenburg	...	4	...	15	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	15	...	...	15
Jászberény	6	1	...	11	2	...	4	2	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	15	4	...	19
Raab	...	...	...	5	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	5	...	...	5
Szatmár-Németi	...	1	...	2	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	2	1	...	3
Zusammen	9	14	1	145	42	4	35	7	...	...	...	...	...	...	...	...	...	...	...	...	...	...	1	...	180	50	4	234

10. DYPHTHERITIS.

Städte.	0-1 Jahr.			1-5 Jahre.			5-20 Jahre.			20-30 Jahre.			30-40 Jahre.			40-60 Jahre.			Über 60 Jahre.			Alter unbekannt.			Summe der Überjährigen.			
	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Zusammen.
Budapest	13	22	...	152	65	2	64	2	1	2	...	...	...	...	...	...	...	...	...	...	...	...	...	...	218	67	3	288
Debreczen	...	...	...	...	1	...	2	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	2	1	...	3
Pressburg	...	1	...	3	4	...	...	...	...	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	4	4	...	8
Arad	...	...	...	9	...	...	4	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	13	...	...	13
Makó	...	...	...	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	1	...	...	1
Cronstadt	...	...	...	3	5	1	8	...	1	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	12	5	2	19
Stuhlweissenburg	...	...	...	5	...	...	2	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	7	...	...	7
Jászberény	5	1	...	7	2	1	7	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	14	2	1	17
Raab	...	...	...	4	...	...	2	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	6	...	...	6
Szatmár-Németi	...	...	...	1	...	1	1	...	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	2	...	2	4
Zusammen	17	24	...	185	77	5	90	2	3	4	...	...	...	...	...	...	...	...	...	...	...	...	...	...	279	79	8	366

11. KEUCHHUSTEN (PERTUSSIS).

Budapest	1	10	4	6	6	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	6	6	...	12
Debreczen	...	...	...	3	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	3	1	...	4
Pressburg	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
Arad	3	5	...	7	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	7	1	...	8
Makó	...	2	...	4	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	4	...	...	4
Cronstadt	...	2	...	1	3	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	1	3	...	4
Stuhlweissenburg	...	4	...	3	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	3	...	...	3
Jászberény	3	3	...	6	3	...	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	7	3	...	10
Raab	1	2	...	2	...	...	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	3	...	...	3
Szatmár-Németi	...	3	...	...	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	1	...	1
Zusammen	8	31	4	32	15	...	2	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	34	15	...	49

12. LUNGEN- UND BRUSTFELL-ENTZÜNDUNG (PNEUMONIA, BRONCHITIS ET PLEURITIS).

Budapest	38	273	90	164	164	9	35	8	1	36	3	2	61	1	7	108	2	19	75	6	19	...		...	479	184	57	720
Debreczen	...	4	...	3			2	...	...	4	...	...	2	...	...	13	3	1	6		...	...		...	30	5	1	36
Pressburg	5	49	...	15	16		3	...	...	1	...	...	3	2	...	13	3	...	22	6	3	...		1	57	27	4	88
Arad	3	10	...	17			6	1	...	4	1	...	3	...	1	9	4	...	7		3	...		...	46	7	4	57
Makó	1	8	...	11			2	...	...	...	...	...	3	...	...	5	...	1	3		1	...		...	24	...	2	26
Cronstadt	...	7	...	12	12		2	1	...	1	...	...	3	...	...	6	1	...	2		...	...		...	26	14	...	40
Stuhlweissenburg	4	28	...	9	3	3	3	...	...	...	...	...	1	...	...	...	...	3	2		2	...		...	15	3	8	26
Jászberény	5	18	1	20			17	...	1	5	...	...	3	...	...	8	...	...	3	2	...	...		...	56	8	1	65
Raab	...	7	...	7			3	...	...	...	...	...	...	...	...	3	...	...	2	1	...	...		...	15	2	...	17
Szatmár-Németi	1	5	1	6	4		1	...	...	3	...	...	1	...	...	5	...	...	4	1	1	...		...	20	5	2	27
Zusammen	57	409	92	264	209		74	10	2	54	4	2	80	3	8	170	13	24	126	6	29	...	...	1	768	255	79	1,102

13. LUNGENTUBERCULOSE (TUBERCULOSIS PULMONUM).

Städte.	0–1 Jahr.			1–5 Jahre.			5–20 Jahre.			20–30 Jahre.			30–40 Jahre.			40–60 Jahre.			Über 60 Jahre.			Alter unbekannt.			Summe der Überjährigen.			
	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Zusammen.
Budapest.*	18	102	38	101	103	4	181	12	20	398	20	41	416	9	60	457	16	68	137	3	39	1	...	1	1,694	163	233	2,090
Debreczen	...	1	...	6	2	1	17	1	2	21	1	2	19	1	4	28	5	3	9	3	1	...	...	...	100	13	13	126
Pressburg	2	4	...	4	6	...	31	...	...	44	...	1	21	2	2	23	4	...	15	3	...	...	...	...	138	15	3	156
Arad	6	6	...	10	...	...	28	1	1	36	...	1	40	1	4	39	1	11	10	2	4	...	...	...	163	5	21	189
Makó	...	...	...	...	...	...	18	1	1	20	...	...	15	...	2	29	...	1	15	...	4	3	...	...	100	1	8	109
Cronstadt	...	...	...	2	2	...	6	4	1	15	1	1	11	3	1	13	2	2	3	...	1	...	...	...	50	12	6	68
Stuhlweissenburg	...	...	...	7	1	...	17	...	...	10	1	5	5	...	9	5	2	9	1	...	9	...	...	...	45	4	32	81
Jászberény	12	37	...	25	11	...	47	11	4	27	3	1	8	2	3	20	...	2	11	1	2	..	...	...	138	28	12	178
Raab	...	3	...	7	...	...	7	...	...	16	...	...	15	...	...	15	...	...	10	...	...	..	...	...	70	...	...	70
Szatmár-Németi	...	1	...	1	...	1	13	1	1	25	1	...	12	...	...	16	4	...	4	1	1	...	...	...	71	7	3	81
Zusammen	38	154	38	166	125	6	365	31	30	612	27	52	562	18	85	645	34	96	215	13	61	4	...	1	2,569	248	331	3,148
14. SONSTIGE KRANKHEITEN DER RESPIRATIONS-ORGANE.																												
Budapest	9	48	22	11	23	2	6	..	...	13	2	3	16	...	2	73	7	14	65	10	40	...	...	...	184	42	61	287
Debreczen	4	21	...	19	6	...	4	1	...	3		...	...	...	...	3	...	2	7	1	...	...	...	...	36	8	2	46
Pressburg	...	5	...	3	3	...	3	..	...	3	1	...	4	1	...	17	8	...	23	4	2	...	...	...	53	17	2	72
Arad	1	2	...	8	3	...	...	..	...	...		...	...	...	...	6	1	...	7	1	1	...	...	...	21	5	1	27
Makó	...	1	...	1	...	...	...	..	...	2		...	1	...	...	1	...	...	1	...	...	...	...	...	6	...	...	6
Cronstadt	1	23	...	2	21	...	1	2	1	...		...	1	1	...	13	...	...	10	2	...	...	...	...	27	26	1	54
Stuhlweissenburg	...	...	...	4	...	...	3	1	2	...		...	...	...	1	2	...	5	...	...	1	...	...	...	9	1	9	19
Jászberény	2	...	...	2	1	...	...	..	...	...		...	...	...	...	...	...	...	3	...	1	...	...	...	5	1	1	7
Raab	6	6	...	5	1	...	2	..	...	1	...	...	...	...	...	5	...	...	6	...	...	...	...	...	19	1	...	20
Szatmár-Németi	...	3	...	3	1	...	2	1	...	...		...	...	...	...	...	1	...	2	1	...	...	...	...	7	4	...	11
Zusammen	23	109	22	58	59	2	21	5	3	22	3	3	22	2	3	120	17	21	124	19	45	...	...	...	367	105	77	549
9–14. SUMME DER KRANKHEITEN DER RESPIRATIONS-ORGANE.																												
Budapest	80	460	155	520	392	18	313	27	22	449	25	46	493	10	69	638	25	101	277	19	98	1	...	1	2,691	498	355	3,544
Debreczen	4	27	...	44	16	2	27	2	2	28	1	2	21	1	4	44	8	6	22	4	1	...	...	...	186	32	17	235
Pressburg	7	60	...	31	31	1	37	...	...	49	1	1	28	5	2	53	15	...	60	13	5	...	...	1	258	65	10	333
Arad	14	23	...	56	6	...	38	2	1	40	1	1	43	1	5	51	6	11	24	3	8	...	...	...	255	19	26	300
Makó	1	11	...	22	1	...	22	1	1	22	...	...	19	...	2	35	...	2	19	...	5	3	1	...	142	3	10	155
Cronstadt	1	33	...	20	43	2	17	7	3	17	1	1	15	4	1	32	3	2	15	2	1	...	...	...	116	60	10	186
Stuhlweissenburg	4	36	...	43	4	3	25	1	2	10	1	5	6	...	10	7	2	17	3	...	12	...	...	...	94	8	49	151
Jászberény	33	60	1	71	25	1	76	13	5	32	3	1	11	2	3	28	...	2	17	3	3	...	...	...	235	46	15	296
Raab	7	18	...	30	2	...	15	...	...	17	...	...	15	...	...	23	...	...	18	1	...	...	...	...	118	3	...	121
Szatmár-Németi	1	13	1	13	7	3	17	2	2	28	1	...	13	...	...	21	5	...	10	3	2	...	...	...	102	18	7	127
Zusammen	152	741	157	850	527	37	587	55	38	692	34	57	664	23	96	935	64	141	465	48	135	4	1	2	4,197	752	499	5,448

15. BAUCHFELLENTZÜNDUNG (PERITONITIS).

Städte.	0–1 Jahr.			1–5 Jahre.			5–20 Jahre.			20–30 Jahre.			30–40 Jahre.			40–60 Jahre.			Über 60 Jahre.			Alter unbekannt.			Summe der Über-einjährigen.			
	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Zusammen.
Budapest	...	2	...	2	2	...	14	2	1	18	1	2	14	...	2	12	...	3	5	1	5	...	...	...	65	6	13	84
Debreczen	...	...	...	1	...	...	2	...	...	1	...	...	2	...	...	4	...	...	...	1	...	...	...	...	10	1	...	11
Pressburg	...	1	...	...	...	...	3	...	...	...	...	...	...	...	...	5	2	...	...	...	...	...	...	...	8	2	...	10
Arad	...	...	...	1	...	...	...	...	...	2	...	...	3	1	1	3	...	...	1	...	...	...	...	...	10	1	1	12
Makó	...	...	...	...	...	...	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	1	...	...	1
Cronstadt	...	...	...	1	2	...	5	...	...	1	...	...	1	...	...	1	...	1	1	...	...	...	...	...	10	2	1	13
Stuhlweissenburg	...	...	...	...	...	...	...	...	1	...	...	2	2	...	2	...	...	1	1	...	2	...	...	...	3	...	8	11
Jászberény	...	...	...	...	...	...	...	...	...	...	...	..	...	...	...	1	...	...	...	...	...	...	...	...	1	...	...	1
Raab	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	3	...	...	...	...	...	...	...	...	3	...	...	3
Szatmár-Németi	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
Zusammen	...	3	...	5	4	...	25	2	2	22	1	4	22	1	5	29	2	5	8	2	7	...	...	...	111	12	23	146

16. DARMENTZÜNDUNG (ENTERITIS).

Städte.	0–1 Jahr. Geimpft.	Ungeimpft.	Zweifelhaft.	1–5 Jahre. Geimpft.	Ungeimpft.	Zweifelhaft.	5–20 Jahre. Geimpft.	Ungeimpft.	Zweifelhaft.	20–30 Jahre. Geimpft.	Ungeimpft.	Zweifelhaft.	30–40 Jahre. Geimpft	Ungeimpft.	Zweifelhaft.	40–60 Jahre. Geimpft.	Ungeimpft.	Zweifelhaft.	Über 60 Jahre. Geimpft.	Ungeimpft.	Zweifelhaft.	Alter unbekannt. Geimpft.	Ungeimpft.	Zweifelhaft.	Summe der Über-einjährigen. Geimpft.	Ungeimpft.	Zweifelhaft.	Zusammen.
Budapest	6	56	34	13	6	...	1	...	...	2	...	...	...	...	...	3	...	1	3	...	...	...	...	...	22	6	1	29
Debreczen	...	...	...	...	...	...	...	...	...	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	1	...	...	1
Pressburg	1	33	...	...	1	...	...	...	...	...	...	...	...	...	...	3	...	...	...	...	...	...	...	...	3	1	...	4
Arad	14	25	...	9	...	...	1	...	...	...	...	...	...	...	...	...	...	...	2	...	...	...	...	...	12	...	...	12
Makó	1	1	...	1	...	...	...	...	...	...	...	...	...	...	...	1	...	...	...	...	...	...	...	...	2	...	...	2
Cronstadt	...	...	...	...	...	...	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	1	...	...	1
Stuhlweissenburg	...	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
Jászberény	...	2	...	1	1	...	...	...	1	...	...	...	...	...	...	1	...	...	1	...	...	...	...	...	3	1	1	5
Raab	...	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
Szatmár-Németi	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
Zusammen	22	119	34	24	8	...	3	...	1	3	...	...	...	...	...	8	...	1	6	...	...	...	...	...	44	8	2	54

17. DURCHFALL (DIARRHŒA).

Städte.	0–1 Jahr. Geimpft.	Ungeimpft.	Zweifelhaft.	1–5 Jahre. Geimpft.	Ungeimpft.	Zweifelhaft.	5–20 Jahre. Geimpft.	Ungeimpft.	Zweifelhaft.	20–30 Jahre. Geimpft.	Ungeimpft.	Zweifelhaft.	30–40 Jahre. Geimpft	Ungeimpft.	Zweifelhaft.	40–60 Jahre. Geimpft.	Ungeimpft.	Zweifelhaft.	Über 60 Jahre. Geimpft.	Ungeimpft.	Zweifelhaft.	Alter unbekannt. Geimpft.	Ungeimpft.	Zweifelhaft.	Summe der Über-einjährigen. Geimpft.	Ungeimpft.	Zweifelhaft.	Zusammen.
Budapest	60	602	242	101	115	4	8	3	1	2	...	...	2	...	...	9	...	4	12	...	16	...	...	...	134	118	25	277
Debreczen	23	102	1	46	35	...	10	1	1	1	...	...	...	...	1	3	...	3	4	...	1	...	...	...	64	36	6	106
Pressburg	10	133	...	5	11	...	...	1	...	...	...	...	1	...	...	1	1	...	2	4	...	...	...	...	9	17	...	26
Arad	29	70	1	25	...	...	3	...	...	...	...	...	...	...	...	1	...	...	...	...	...	...	...	...	29	...	...	29
Makó	10	21	1	18	10	...	...	...	...	...	...	...	...	...	...	1	...	...	...	...	...	...	...	...	19	10	...	29
Cronstadt	1	39	1	4	24	...	2	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	6	24	...	30
Stuhlweissenburg	6	63	...	8	3	...	...	...	...	...	...	...	...	...	...	...	...	1	...	...	1	...	...	...	8	3	2	13
Jászberény	17	29	...	20	10	...	2	...	1	1	...	...	...	...	...	2	...	...	1	...	...	...	...	...	26	10	1	37
Raab	5	43	...	7	...	...	...	...	...	...	...	...	...	...	...	...	...	...	1	...	...	...	1	...	8	1	...	9
Szatmár-Németi	...	26	1	2	3	...	...	...	...	...	...	...	...	...	...	1	...	...	...	...	...	...	...	...	3	3	...	6
Zusammen	161	1,128	247	236	211	4	25	5	3	4	...	...	3	...	1	18	1	8	20	4	18	...	1	...	306	222	34	562

18. SONSTIGE KRANKHEITEN DER VERDAUUNGSORGANE.

Städte.	0-1 Jahr.			1-5 Jahre.			5-20 Jahre.			20-30 Jahre.			30-40 Jahre.			40-60 Jahre.			Über 60 Jahre.			Alter unbekannt.			Summe der Überjährigen.			
	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Zusammen.
Budapest	...	20	17	4	6	1	12	...	3	15	...	1	31	...	...	137	1	20	75	9	18	...	...	...	274	16	43	333
Debreczen	...	1	...	1	...	...	...	...	...	1	...	...	...	...	...	1	...	...	3	2	...	...	...	...	6	2	...	8
Pressburg	1	14	...	...	1	...	...	...	...	1	...	...	1	...	...	14	2	1	18	3	...	...	...	...	34	6	1	41
Arad	1	2	...	...	...	...	...	...	...	2	...	...	2	...	...	9	1	2	4	2	...	...	...	...	17	3	2	22
Makó	...	...	...	...	...	...	1	...	...	...	...	...	2	...	...	5	...	...	3	...	1	...	...	...	11	...	1	12
Cronstadt	...	1	...	...	...	...	1	...	...	2	...	...	3	...	...	9	...	...	2	1	...	...	...	...	17	1	...	18
Stuhlweissenburg	...	2	...	1	1	...	...	...	...	...	...	...	...	...	1	...	...	4	...	...	4	...	...	...	1	1	9	11
Jászberény	...	1	...	2	...	...	1	...	...	...	...	...	2	...	...	3	2	...	...	1	...	...	...	...	8	3	...	11
Raab	1	4	...	3	...	...	...	...	...	1	...	...	...	...	...	5	...	...	3	...	...	...	...	...	12	...	...	12
Szatmár-Németi	...	1	...	...	...	...	1	...	...	...	...	...	...	...	...	7	1	1	3	...	1	...	...	...	11	1	2	14
Zusammen	3	46	17	11	8	1	16	...	3	22	...	1	41	...	1	190	7	28	111	18	24	...	...	...	391	33	58	482

15-18. SUMME DER KRANKHEITEN DER VERDAUUNGSORGANE.

Budapest	66	680	293	120	129	5	35	5	5	37	1	3	47		2	161	1	28	95	10	39	...	...	...	495	146	82	723
Debreczen	23	103	1	48	35	...	12	1	1	4	...	...	2		1	8	...	3	7	3	1	...	...	...	81	39	6	126
Pressburg	12	181	...	5	13	...	3	1	...	1	...	...	2		...	23	5	1	20	7	...	...	...	...	54	26	1	81
Arad	44	97	1	35	...	...	4	...	...	4	...	...	5	1	1	13	1	2	7	2	...	...	...	...	68	4	3	75
Makó	11	22	1	19	10	...	2	...	...	...	...	...	2		...	7	...	...	3	...	1	...	...	...	33	10	1	44
Cronstadt	1	40	1	5	26	...	9	...	...	3	...	...	4		...	10	...	1	3	1	...	...	...	...	34	27	1	62
Stuhlweissenburg	6	66	...	9	4	...	...	...	1	...	...	2	2		3	...	...	6	1	...	7	...	...	...	12	4	19	35
Jászberény	17	32	...	23	11	...	3	...	2	1	...	...	2		...	7	2	...	2	1	...	...	...	...	38	14	2	54
Raab	6	48	...	10	...	...	...	...	...	1	...	...	...		...	8	...	...	4	...	...	...	1	...	23	1	...	24
Szatmár-Németi	...	27	1	2	3	...	1	...	...	...	...	...	...		...	8	1	1	3	...	1	...	...	...	14	4	2	20
Zusammen	186	1,296	298	276	231	5	69	7	9	51	1	5	66	1	7	245	10	42	145	24	49	...	1	...	852	275	117	1,244

19. BRIGHT'SCHE KRANKHEIT (MORBUS BRIGHTII).

Budapest	...	1	...	8	2	...	17	1	1	8	...	...	1	...	1	11	...	4	8	2	3	...	...	...	53	5	9	67
Debreczen	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
Pressburg	...	...	...	...	...	...	1	...	...	...	...	...	...	...	...	1	...	...	2	...	...	...	...	...	4	...	...	4
Arad	...	...	...	...	...	...	...	...	...	...	...	...	4	...	...	2	...	...	2	...	...	...	...	...	8	...	...	8
Makó	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
Cronstadt	...	...	...	...	...	...	1	1	...	...	...	...	...	...	1	...	...	...	...	...	...	...	...	...	1	1	1	3
Stuhlweissenburg	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	1	...	...	...	...	...	...	...	...	1	1
Jászberény	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
Raab	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	1	...	...	...	...	...	1	...	...	1
Szatmár-Németi	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
Zusammen	...	1	...	8	2	...	19	2	1	8	...	...	5	...	2	14	...	5	13	2	3	...	...	...	67	6	11	84

20. KINDBETTFIEBER (FEBRIS PUERPERALIS).

Städte.	0–1 Jahr.			1–5 Jahre.			5–20 Jahre.			20–30 Jahre.			30–40 Jahre.			40–60 Jahre.			Über 60 Jahre.			Alter unbekannt.			Summe der Überjährigen.			
	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Zusammen.
Budapest	...	...	...	...	...	...	2	...	...	16	...	2	5	...	...	3	...	...	...	...	...	...	...	...	26	...	2	28
Debreczen	...	...	...	...	...	...	...	...	...	2	...	...	1	...	1	1	...	...	...	...	...	...	...	...	4	...	1	5
Pressburg	...	...	...	...	...	...	...	...	...	...	...	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	1	1
Arad	...	...	...	...	...	...	...	...	...	1	...	...	...	...	1	...	...	...	...	...	...	...	...	...	1	...	1	2
Makó	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
Cronstadt	...	...	...	...	...	...	...	...	...	1	...	1	...	...	...	...	...	...	...	...	...	...	...	...	1	...	1	2
Stuhlweissenburg	...	...	...	...	...	...	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	1	...	...	1
Jászberény	...	...	...	...	...	...	...	...	...	1	...	...	1	...	...	...	...	...	...	...	...	...	...	...	2	...	...	2
Raab	...	...	...	...	...	...	...	...	...	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	1	...	...	1
Szatmár-Németi	...	...	...	...	...	...	...	...	...	2	...	...	...	...	1	...	...	...	...	...	...	...	...	...	2	...	1	3
Zusammen	...	...	...	...	...	...	3	...	...	24	...	4	7	...	3	4	...	...	...	...	...	...	...	...	38	...	7	45

21. ANDERE KINDBETT-KRANKHEITEN.

Städte.	0–1 Jahr. Geimpft.	Ungeimpft.	Zweifelhaft.	1–5 Jahre. Geimpft.	Ungeimpft.	Zweifelhaft.	5–20 Jahre. Geimpft.	Ungeimpft.	Zweifelhaft.	20–30 Jahre. Geimpft.	Ungeimpft.	Zweifelhaft.	30–40 Jahre. Geimpft.	Ungeimpft.	Zweifelhaft.	40–60 Jahre. Geimpft.	Ungeimpft.	Zweifelhaft.	Über 60 Jahre. Geimpft.	Ungeimpft.	Zweifelhaft.	Alter unbekannt. Geimpft.	Ungeimpft.	Zweifelhaft.	Summe der Überjährigen. Geimpft.	Ungeimpft.	Zweifelhaft.	Zusammen.
Budapest	...	...	...	...	...	...	1	...	...	5	...	...	1	...	...	4	...	...	...	...	...	...	...	...	11	...	...	11
Debreczen	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
Pressburg	...	...	...	...	...	...	...	...	...	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	1	...	...	1
Arad	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
Makó	...	...	...	...	...	...	...	...	...	1	...	...	...	...	...	1	...	...	...	...	...	...	...	...	2	...	...	2
Cronstadt	...	...	...	...	...	...	...	...	...	...	...	...	1	...	...	...	...	...	...	...	...	...	...	...	1	...	...	1
Stuhlweissenburg	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
Jászberény	...	...	...	...	...	...	...	...	...	...	...	...	1	...	...	...	...	...	...	...	...	...	...	...	1	...	...	1
Raab	...	...	...	...	...	...	...	...	...	...	...	...	1	...	...	...	...	...	...	...	...	...	...	...	1	...	...	1
Szatmár-Németi	...	...	...	...	...	...	...	...	...	1	...	...	...	...	...	2	...	...	...	1	...	...	...	...	3	1	...	4
Zusammen	...	...	...	...	...	...	1	...	...	8	...	...	4	...	...	7	...	...	...	1	...	...	...	...	20	1	...	21

22. ANDERE KRANKHEITEN DER GESCHLECHTS- UND HARN-ORGANE.

Städte.	0–1 Jahr. Geimpft.	Ungeimpft.	Zweifelhaft.	1–5 Jahre. Geimpft.	Ungeimpft.	Zweifelhaft.	5–20 Jahre. Geimpft.	Ungeimpft.	Zweifelhaft.	20–30 Jahre. Geimpft.	Ungeimpft.	Zweifelhaft.	30–40 Jahre. Geimpft.	Ungeimpft.	Zweifelhaft.	40–60 Jahre. Geimpft.	Ungeimpft.	Zweifelhaft.	Über 60 Jahre. Geimpft.	Ungeimpft.	Zweifelhaft.	Alter unbekannt. Geimpft.	Ungeimpft.	Zweifelhaft.	Summe der Überjährigen. Geimpft.	Ungeimpft.	Zweifelhaft.	Zusammen.
Budapest	...	...	...	22	6	1	31	6	...	20	...	1	37	2	3	86	3	12	41	3	16	...	...	...	237	20	33	290
Debreczen	...	...	...	3	...	1	4	2	...	1	...	...	2	...	...	6	3	...	1	...	...	...	...	...	17	5	1	23
Pressburg	...	...	1	2	...	...	2	...	...	...	...	...	4	1	...	7	...	1	4	3	1	...	...	...	19	4	2	25
Arad	...	...	...	1	...	...	...	...	...	2	...	...	3	...	1	10	1	...	2	...	...	...	...	...	18	1	1	20
Makó	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	4	...	...	...	...	...	...	...	...	4	...	...	4
Cronstadt	...	...	...	1	3	...	3	...	...	1	...	...	2	...	...	3	...	...	2	...	...	...	...	...	12	3	...	15
Stuhlweissenburg	...	...	...	1	1	1	...	...	...	...	...	...	...	...	1	...	...	3	...	...	4	...	...	...	1	1	9	11
Jászberény	...	...	...	...	...	...	...	...	...	1	...	...	...	...	...	1	...	...	...	...	...	...	...	...	2	...	...	2
Raab	...	...	...	...	...	...	...	...	...	1	...	...	...	...	...	4	...	...	...	...	...	...	...	...	5	...	...	5
Szatmár-Németi	...	1	...	1	...	...	...	...	...	...	...	...	...	...	...	3	1	...	...	1	...	...	...	...	4	2	...	6
Zusammen	...	1	1	31	10	3	40	8	...	26	...	1	48	3	5	124	8	16	50	7	21	...	...	...	319	36	46	401

19-22. SUMME DER KRANKHEITEN DER GESCHLECHTS- UND HARNORGANE.

Städte.	0-1 Jahr.			1-5 Jahre.			5-20 Jahre.			20-30 Jahre.			30-40 Jahre.			40-60 Jahre.			Über 60 Jahre.			Alter unbekannt.			Summe der Über-einjährigen.			
	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Zusammen.
Budapest	...	1	...	30	8	1	51	7	1	49	...	3	44	2	4	104	3	16	49	5	19	...	...	...	327	25	44	396
Debreczen	...	...	...	3	...	1	4	2	...	3	...	...	3	...	1	7	3	...	1	...	...	...	...	...	21	5	2	28
Pressburg	...	...	1	2	...	...	3	...	...	1	...	1	4	1	...	8	...	1	6	3	1	...	...	...	24	4	3	31
Arad	...	...	...	1	...	...	...	...	...	3	...	...	7	...	2	12	1	...	4	...	...	...	...	...	27	1	2	30
Makó	...	...	...	...	...	...	...	...	...	1	...	...	...	...	...	5	...	...	...	...	...	...	...	...	6	...	...	6
Cronstadt	...	...	...	1	3	...	4	1	...	2	...	1	3	...	1	3	...	...	2	...	...	...	...	...	15	4	2	21
Stuhlweissenburg	...	...	...	1	1	1	1	...	...	...	...	...	...	...	1	...	...	4	...	...	4	...	...	...	2	1	10	13
Jászberény	...	...	...	...	...	...	...	...	...	2	...	...	2	...	...	1	...	...	...	...	...	...	...	...	5	...	...	5
Raab	...	...	...	...	...	...	...	...	...	2	...	...	1	...	...	4	...	...	1	...	...	...	...	...	8	...	...	8
Szatmár-Németi	...	1	...	1	...	...	...	...	...	3	...	...	...	...	1	5	1	...	...	2	...	...	...	...	9	3	1	13
Zusammen	...	2	1	39	12	3	63	10	1	66	...	5	64	3	10	149	8	21	63	10	24	...	...	...	444	43	64	551
23. ENGLISCHE KRANKHEIT (RACHITIS).																												
Budapest	1	14	...	14	17	...	2	...	...	...	...	...	...	...	...	...		...	...	...	...	...	...	...	16	17	...	33
Debreczen	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...		...	...	...	...	...	...	...	...	...	...	...
Pressburg	1	...	...	...	4	1	...	...	...	...	...	...	...	...	...	...		...	...	...	...	...	...	...	...	4	1	5
Arad	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...		...	...	...	...	...	...	...	...	...	...	...
Makó	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...		...	...	...	...	...	...	...	...	...	...	...
Cronstadt	...	...	...	2	...	...	...	...	...	...	...	...	...	...	...	...		...	...	...	...	...	...	...	2	...	...	2
Stuhlweissenburg	...	1	...	1	1	...	2	...	1	...	...	...	...	...	...	...		...	...	...	...	...	...	...	3	1	1	5
Jászberény	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...		...	...	...	...	...	...	...	...	...	...	...
Raab	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...		...	...	...	...	...	...	...	...	...	...	...
Szatmár-Németi	...	...	...	...	1	...	...	...	...	...	...	...	...	...	...	...		...	...	...	...	...	...	...	...	1	...	1
Zusammen	2	15	...	17	23	1	4	...	1	...	...	...	...	...	...	...		...	...	...	...	...	...	...	21	23	2	46
24. ANDERE KRANKHEITEN DER BEWEGUNGSORGANE.																												
Budapest	...	2	1	3	3	...	18	3	...	9	...	1	18	...	...	19	...	2	7	...	8	...	...	...	74	6	11	91
Debreczen	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
Pressburg	...	2	...	...	1	...	...	...	...	1	...	...	1	...	...	2	2	...	2	1	1	...	...	...	6	4	1	11
Arad	...	...	...	...	...	...	1	...	...	1	...	...	1	...	...	2	...	...	2	1	...	...	...	...	7	1	...	8
Makó	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
Cronstadt	...	1	...	...	...	...	...	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	1	...	1
Stuhlweissenburg	...	...	...	...	...	...	1	...	...	...	...	...	...	...	...	1	...	...	...	...	...	...	...	...	2	...	...	2
Jászberény	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	2	...	...	...	...	...	2	...	...	2
Raab	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	1	...	...	...	...	...	...	...	...	1	...	...	1
Szatmár-Németi	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
Zusammen	1	5	1	3	4	...	20	4	...	11	...	1	20	...	...	25	2	2	13	2	9	...	...	...	92	12	12	116

23–24. SUMME DER KRANKHEITEN DER BEWEGUNGSORGANE.

Städte.	0–1 Jahr.			1–5 Jahre.			5–20 Jahre.			20–30 Jahre.			30–40 Jahre.			40–60 Jahre.			Über 60 Jahre.			Alter unbekannt.			Summe der Übereinjährigen.			
	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Zusammen.
Budapest	1	16	1	17	20	...	20	3	...	9	...	1	18	...	...	19	...	2	7	...	8	...	...	...	90	23	11	124
Debreczen	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
Pressburg	1	2	...	...	5	1	...	...	...	1	...	...	1	...	...	2	2	...	2	1	1	...	...	...	6	8	2	16
Arad	...	...	...	...	...	...	1	...	...	1	...	...	1	...	...	2	...	...	2	1	...	...	...	...	7	1	...	8
Makó	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
Cronstadt	...	1	...	2	...	...	...	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	2	1	...	3
Stuhlweissenburg	...	1	...	1	1	...	3	...	1	...	...	...	...	...	...	1	...	...	...	...	...	...	...	...	5	1	1	7
Jászberény	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	2	...	...	...	...	...	2	...	...	2
Raab	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	1	...	...	...	...	...	...	...	...	1	...	...	1
Szatmár-Németi	...	...	...	...	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	1	...	1
Zusammen	3	20	1	20	27	1	24	4	1	11	...	1	20	...	...	25	2	2	13	2	9	...	...	...	113	35	14	162
25. SUMME DER HAUTKRANKHEITEN.																												
Budapest	...	10	4	3	1	...	3	...	...	3	...	...	4	...	1	4	...	...	6	...	2	...	...	...	23	1	3	27
Debreczen	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	1	...	...	...	...	...	...	...	...	1	...	...	1
Pressburg	...	2	...	...	...	...	...	...	...	...	...	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	1	1
Arad	...	2	...	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	1	...	...	1
Makó	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
Cronstadt	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
Stuhlweissenburg	...	...	...	...	...	...	...	...	...	...	...	...	1	...	...	1	...	...	...	...	...	...	...	...	2	...	...	2
Jászberény	...	...	...	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	1	...	...	1
Raab	...	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	1	...	...	...	...	...	1	...	...	1
Szatmár-Németi	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
Zusammen	...	15	4	5	1	...	3	...	...	3	...	1	5	...	1	6	...	...	7	...	2	...	...	...	29	1	4	34
26. MASERN (MORBILLI).																												
Budapest	3	17	...	43	44	...	6	2	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	49	46	...	95
Debreczen	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
Pressburg	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
Arad	1	...	...	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	1	...	...	1
Makó	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
Cronstadt	...	7	...	3	10	...	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	4	10	...	14
Stuhlweissenburg	...	...	...	2	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	2	...	...	2
Jászberény	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
Raab	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
Szatmár-Németi	...	2	...	...	...	...	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	1	...	...	1
Zusammen	4	26	...	49	54	...	8	2	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	57	56	...	113

27. SCHARLACH (SCARLATINA).

Städte.	0-1 Jahr.			1-5 Jahre.			5-20 Jahre.			20-30 Jahre.			30-40 Jahre.			40-60 Jahre.			Über 60 Jahre.			Alter unbekannt.			Summe der Überjährigen.			
	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Zusammen.
Budapest	16	18	...	233	73	...	163	29	1	7	2	...	1	...	...	...	...	...	...	...	1	...	...	...	404	104	2	510
Debreczen	...	...	...	3	1	...	3	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	6	1	...	7
Pressburg	...	1	...	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	1	...	...	1
Arad	1	...	...	9	...	...	2	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	11	...	...	11
Makó	1	1	...	14	1	...	2	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	16	1	...	17
Cronstadt	...	...	...	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	1	...	...	1
Stuhlweissenburg	...	...	1	1	...	1	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	2	...	1	3
Jászberény	3	5	...	21	3	4	8	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	29	4	4	37
Raab	2	...	...	26	...	...	...	...	...	...	...	...	1	...	...	...	...	...	...	...	...	...	...	...	27	...	...	27
Szatmár-Németi	...	1	...	1	2	...	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	2	2	...	4
Zusammen	23	26	1	310	80	5	180	30	1	7	2	...	2	...	...	...	...	...	...	...	1	...	...	...	499	112	7	618

28. BLATTERN (VARIOLA).

Budapest	9	245	...	44	498	1	54	304	1	48	74	1	32	9	1	18	7	...	5	1	...	...	...	...	201	893	4	1098
Debreczen	3	37	...	8	53	1	16	50	1	...	4	...	1	4	...	...	1	...	...	...	...	...	...	...	25	112	2	139
Pressburg	...	12	...	1	18	...	1	11	...	...	3	...	1	...	...	...	...	...	...	...	...	...	...	...	3	32	...	35
Arad	...	...	...	1	2	...	1	...	...	1	...	...	2	...	...	...	...	...	...	1	...	...	...	...	5	3	...	8
Makó	...	1	2	...	1	4	1	1	...	...	...	...	...	...	1	...	...	...	...	...	...	...	...	...	1	2	5	8
Cronstadt	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
Stuhlweissenburg	...	1	...	1	...	...	...	1	...	...	1	...	...	...	...	...	...	...	...	...	...	...	...	...	1	2	...	3
Jásberény	...	5	...	2	4	1	...	3	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	2	7	1	10
Raab	...	...	...	...	...	...	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	1	...	...	1
Szatmár-Németi	...	1	...	...	1	...	...	2	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	3	...	3
Zusammen	12	302	2	57	577	7	74	372	2	49	82	1	36	13	2	18	8	...	5	2	...	...	...	...	239	1054	12	1305

29. TYPHUS.

Budapest	...	...	...	2	1	...	35	2	...	38	2	4	37	2	1	47	...	3	9	1	1	...	...	...	168	8	9	185
Debreczen	1	...	...	3	3	...	11	2	...	1	...	...	2	...	1	2	...	...	5	...	...	...	...	...	24	5	1	30
Pressburg	...	1	...	1	...	...	6	...	...	2	1	...	...	...	...	1	...	...	1	...	...	...	...	...	11	1	...	12
Arad	...	...	...	...	...	...	2	...	...	2	...	...	1	1	...	2	...	...	...	...	...	...	...	...	7	1	...	8
Makó	1	...	...	1	...	...	15	1	...	3	...	...	1	...	...	...	...	...	...	...	...	...	...	...	20	1	...	21
Cronstadt	...	...	...	...	2	...	2	...	...	...	...	...	1	1	...	1	...	...	1	...	...	...	...	...	5	3	...	8
Stuhlweissenburg	...	1	...	...	...	...	3	...	...	1	...	1	...	1	1	...	...	1	...	...	...	...	...	...	4	1	3	8
Jászberény	...	...	...	1	...	...	2	...	...	2	...	...	...	...	...	...	...	...	1	...	...	...	...	...	6	...	...	6
Raab	...	...	...	...	...	...	1	...	...	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	2	...	...	2
Szatmár-Németi	...	...	...	...	...	...	1	...	...	...	1	1	1	...	...	...	1	...	...	3	...	...	...	...	2	5	1	8
Zusammen	2	2	...	8	6	...	78	5	...	50	4	6	43	5	3	53	1	4	17	4	1	...	...	...	249	25	14	288

30. CHOLERA.

Städte.	0–1 Jahr.			1–5 Jahre.			5–20 Jahre.			20–30 Jahre.			30–40 Jahre.			40–60 Jahre.			Über 60 Jahre.			Alter unbekannt.			Summe der Überjährigen.			
	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Zusammen.
Budapest	5	2	...	30	1	2	47	2	1	119	2	8	122	7	10	124	5	14	50	5	8	1	...	...	493	22	43	558
Debreczen	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
Pressburg	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
Arad	...	...	...	...	...	...	...	...	...	...	...	...	3	...	...	2	...	...	...	...	1	...	...	...	5	...	1	6
Makó	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
Cronstadt	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
Stuhlweissenburg	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
Jászberény	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
Raab	...	...	...	6	...	...	4	...	...	5	...	...	5	...	...	8	...	...	2	...	...	...	...	...	30	...	...	30
Szatmár-Németi	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
Zusammen	5	2	...	36	1	2	51	2	1	124	2	8	130	7	10	134	5	14	52	5	9	1	...	...	528	22	44	594

31. SKROPHELN.

Budapest		5	2	15	9	...	12	1	...	3	...	...	...	...	...	...	...	...	...		...	...		...	30	10	...	40
Debreczen		1	...	...	...	...	2	...	...	...	...	...	...	...	...	...	...	...	...		...	...		...	2	...	...	2
Pressburg		...	...	...	...	...	2	...	...	...	...	...	...	...	...	...	...	...	...		...	...		...	2	...	...	2
Arad		...	...	4	1	...	1	...	...	...	...	...	...	...	...	...	...	...	...		...	...		...	5	1	...	6
Makó		...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...		...	...		...	...	...	...	...
Cronstadt		...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...		...	...		...	...	...	...	...
Stuhlweissenburg		1	...	...	...	...	1	...	...	...	...	...	...	...	...	...	...	...	...		...	...		...	1	...	...	1
Jászberény		1	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...		...	...		...	...	...	...	...
Raab		...	...	...	...	...	2	...	1	...	...	...	...	...	...	...	...	...	...		...	...		...	2	...	1	3
Szatmár-Németi		4	1	1	...	...	...	3	...	1	...	...	...	...	...	...	...	...	...		...	...		...	2	3	...	5
Zusammen		12	4	20	10	...	20	4	1	4	...	...	...	...	...	...	...	...	...		...	...		...	44	14	1	59

32. ANGEBORENE SYPHILIS (SYPHILIS CONGENITA).

Budapest	...	14	12	2	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	2	...	...	2
Debreczen	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
Pressburg	...	2	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
Arad	...	...	...	...	...	...	1	...	...	...	...	...	1	...	...	...	...	...	...	...	...	...	...	...	2	...	...	2
Makó	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
Cronstadt	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
Stuhlweissenburg	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
Jászberény	...	2	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
Raab	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
Szatmár-Németi	...	3	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
Zusammen	...	21	12	2	...	...	1	...	...	...	...	...	1	...	...	...	...	...	...	...	...	...	...	...	4	...	...	4

33. SYPHILIS.

Städte.	0-1 Jahr.			1-5 Jahre.			5-20 Jahre.			20-30 Jahre.			30-40 Jahre.			40-60 Jahre.			Über 60 Jahre.			Alter unbekannt.			Summe der Über-einjährigen.			
	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Zusammen.
Budapest	...	...	...	...	...	...	...	...	...	1	...	...	1	...	...	3	...	...	...	...	...	...	...	...	5	...	...	5
Debreczen	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
Pressburg	...	...	...	...	...	...	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	1	...	...	1
Arad	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
Makó	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
Cronstadt	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	1	...	...	...	...	...	1	...	...	1
Zusammen	...	...	...	...	...	...	1	...	...	1	...	...	1	...	...	3	...	...	1	...	...	...	...	...	7	...	...	7

34. ROTHLAUF (ERYSIPELAS).

Budapest	1	4	1	1	...	...	1	...	...	...	1	...	1	...	...	4	...	...	4	...	3	...	...	...	11	1	3	15
Debreczen	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	1	...	...	...	...	...	1	...	...	1
Pressburg	...	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
Arad	...	4	...	...	...	...	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	1	...	...	1
Makó	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
Cronstadt	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	1	...	...	...	...	...	...	...	...	1	...	...	1
Stuhlweissenburg	...	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	1	...	...	1	...	...	...	...	...	2	2
Jászberény	...	...	...	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	1	...	...	1
Raab	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
Szatmár-Németi	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
Zusammen	1	10	1	2	...	...	2	...	...	...	1	...	1	...	...	5	...	1	5	...	4	...	...	...	15	1	5	21

35. BLUTVERGIFTUNG (PYÆMIA).

Budapest	1	...	2	1	...	...	...	...	...	4	...	...	2	...	1	1	...	2	...	1	2	...	...	...	8	1	5	14
Debreczen	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
Pressburg	...	1	...	...	...	...	...	...	...	...	...	...	...	...	...	1	1	...	...	...	...	...	...	...	1	1	...	2
Arad	...	1	...	...	...	...	...	...	...	...	...	...	...	...	...	2	...	...	...	...	...	...	...	...	2	...	...	2
Makó	...	...	...	...	...	...	1	...	...	1	...	...	...	...	...	...	...	1	1	...	1	...	...	...	3	...	2	5
Cronstadt	...	...	...	...	...	...	1	...	...	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	2	...	...	2
Stuhlweissenburg	...	...	...	...	...	...	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	1	...	...	1
Jászberény	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
Raab	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	1	...	...	...	...	...	1	...	...	1
Szatmár-Németi	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
Zusammen	1	2	2	1	...	...	3	...	...	6	...	...	2	...	1	4	1	3	2	1	3	...	...	...	18	2	7	27

36. ANDERE CONSTITUTIONELLE KRANKHEITEN.

Städte.	0-1 Jahr.			1-5 Jahre.			5-20 Jahre.			20-30 Jahre.			30-40 Jahre.			40-60 Jahre.			Über 60 Jahre.			Alter unbekannt.			Summe der Überjährigen.			
	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Zusammen.
Budapest	...	1	2	4	2	...	4	3	...	4	...	1	7	...	2	13	1	3	13	2	3	...	...	...	45	8	9	62
Debreczen	3	7	...	8	7	...	5	2	...	1	...	...	...	...	...	4	1	1	12	2	...	...	...	...	30	12	1	43
Pressburg	...	2	...	...	...	...	...	...	...	...	...	...	...	1	...	2	...	...	5	1	...	...	...	...	7	2	...	9
Arad	...	...	...	...	...	...	1	...	...	...	...	...	...	...	...	...	3	...	1	1	1	...	...	...	2	4	1	7
Makó	...	...	...	...	...	...	...	...	...	1	...	...	...	...	...	...	...	...	...	...	1	...	...	...	1	...	1	2
Cronstadt	...	2	...	1	2	...	...	...	...	1	...	...	1	...	...	2	...	...	2	...	...	...	...	...	7	2	...	9
Stuhlweissenburg	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	1	...	...	...	...	...	1	1
Jászberény	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
Raab	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	2	...	...	...	...	...	...	...	...	2	...	...	2
Szatmár-Németi	...	2	...	1	2	...	4	...	...	2	...	1	...	...	...	1	...	...	1	...	...	...	...	...	9	2	1	12
Zusammen	3	14	2	14	13	...	14	5	...	9	...	2	8	1	2	24	5	4	34	6	6	...	...	...	103	30	14	147
26-36. SUMME DER CONSTITUTIONELLEN KRANKHEITEN.																												
Budapest	36	306	19	375	628	3	322	343	3	224	81	14	203	18	15	210	13	22	81	10	18	1	...	...	1,416	1,093	75	2,584
Debreczen	7	45	...	22	64	1	37	54	1	2	4	...	3	4	1	6	2	1	18	2	...	...	...	...	88	130	4	222
Pressburg	...	20	...	3	18	...	10	11	...	2	4	...	1	1	...	4	1	...	6	1	...	...	...	...	26	36	...	62
Arad	2	5	...	15	3	...	9	...	...	3	...	...	7	1	...	6	3	...	1	2	2	...	...	...	41	9	2	52
Makó	2	2	2	15	2	4	19	2	...	5	...	...	1	...	1	...	...	1	1	...	2	...	...	...	41	4	8	53
Cronstadt	...	9	...	5	14	...	4	...	...	2	...	...	2	1	...	4	...	...	4	...	...	...	...	...	21	15	...	36
Stuhlweissenburg	...	4	1	4	...	1	6	1	...	1	1	1	...	1	1	...	...	2	...	...	2	...	...	...	11	3	7	21
Jászberény	3	13	1	25	7	5	10	4	...	2	...	...	...	...	...	...	...	...	1	...	...	...	...	...	38	11	5	54
Raab	2	...	...	32	...	...	8	...	1	6	...	...	6	...	...	10	...	...	3	...	...	...	...	...	65	...	1	66
Szatmár-Németi	...	13	1	3	5	...	7	5	...	3	1	2	1	...	...	1	1	...	1	3	...	...	...	...	16	15	2	33
Zusammen	52	417	24	499	741	14	432	420	5	250	91	17	224	26	18	241	20	26	116	18	24	1	...	...	1,763	1,316	104	3,183
37. ANGEBORENE SCHWÄCHE (DEBILITAS CONGENITA).																												
Budapest	3	610	...	5	6	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	5	6	...	11
Debreczen	2	85	1	3	10	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	3	10	...	13
Pressburg	...	62	...	...	2	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	2	...	2
Arad	1	122	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
Makó	5	82	...	25	8	...	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	2	...	26	10	...	36
Cronstadt	...	28	1	...	2	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	2	...	2
Stuhlweissenburg	...	49	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
Jászberény	17	76	1	3	1	...	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	4	1	...	5
Raab	1	31	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
Szatmár-Németi	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
Zusammen	29	1,145	4	36	29	...	2	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	2	...	38	31	...	69

38. ERNÄHRUNGSMANGEL (ATROPIA ET INANITIS).

Städte.	0-1 Jahr.			1-5 Jahre.			5-20 Jahre.			20-30 Jahre.			30-40 Jahre.			40-60 Jahre.			Über 60 Jahre.			Alter unbekannt.			Summe der Über-einjährigen.			
	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Zusammen.
Budapest	...	41	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
Zusammen	...	41	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...

39. ALTERSSCHWÄCHE (MARASMUS SENILIS).

Budapest	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	10	1	4	236	31	100	1	...	...	247	32	104	383
Debreczen	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	2	...	1	63	17	9	...	...	...	65	17	10	92
Pressburg	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	66	33	7	...	...	...	66	33	7	106
Arad	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	1	...	...	28	12	27	...	...	...	29	12	27	68
Makó	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	53	2	9	...	...	...	53	2	9	64
Cronstadt	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	56	12	6	...	...	...	56	12	6	74
Stuhlweissenburg	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	1	...	35	...	...	...	1	...	35	36
Jászberény	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	52	4	11	...	...	...	52	4	11	67
Raab	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	1	...	...	22	1	...	...	...	...	23	1	...	24
Szatmár-Németi	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	10	16	...	...	...	...	10	16	...	26
Zusammen	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	14	1	5	587	128	204	1	...	...	602	129	209	940

40. WASSERSUCHT (HYDROPS).

Budapest	...	...	...	3	6	...	11	3	1	3	1	...	2	...	3	7	...	6	14	...	4	...	...	...	40	10	14	64
Debreczen	...	...	...	1	...	...	3	1	...	1	...	...	...	...	...	2	...	1	3	...	...	...	...	...	10	1	1	12
Pressburg	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	3	...	...	6	...	1	...	...	...	9	...	1	10
Arad	...	...	...	1	...	...	2	...	...	...	...	...	...	...	...	1	...	1	3	...	4	...	...	...	7	...	5	12
Makó	...	...	...	1	...	...	1	...	...	2	...	...	1	...	1	17	1	2	13	...	...	...	...	...	35	1	3	39
Cronstadt	...	...	...	...	2	1	1	...	...	1	...	...	...	1	...	...	2	2	...	...	...	...	...	...	2	5	3	10
Stuhlweissenburg	...	...	...	...	...	...	1	...	...	...	...	...	...	...	...	...	1	...	...	...	2	...	...	...	1	1	2	4
Jászberény	...	...	...	1	1	...	2	...	...	1	...	...	2	...	...	3	...	...	3	2	5	...	...	...	12	3	5	20
Raab	...	...	...	...	...	...	2	...	...	1	...	...	...	...	...	3	...	...	5	...	...	...	...	...	11	...	...	11
Szatmár-Németi	1	...	...	...	...	...	1	...	...	...	...	...	1	...	...	2	...	...	...	...	...	...	...	...	4	...	...	4
Zusammen	1	...	...	7	9	1	24	4	1	9	1	...	6	1	4	38	4	12	47	2	16	...	...	...	131	21	34	186

41. SONSTIGE VERSCHIEDENEN KRANKHEITEN.

Städte.	0-1 Jahr.			1-5 Jahre.			5-20 Jahre.			20-30 Jahre.			30-40 Jahre.			40-60 Jahre.			Über 60 Jahre.			Alter unbekannt.			Summe der Über-einjährigen.			
	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Zusammen.
Budapest	...	3	8	2	3	...	2	1	1	7	...	1	20	1	8	36	3	4	12	...	1	...	...	2	79	8	17	104
Debreczen	...	2	...	1	1	...	1	...	...	1	...	...	2	...	...	...	1	...	2	...	1	...	...	...	7	2	1	10
Pressburg	...	4	...	...	...	...	1	...	...	...	...	...	1	...	...	1	...	...	...	...	...	...	...	...	3	...	...	3
Arad	...	...	...	...	...	...	...	...	1	4	...	4	3	...	3	13	...	3	4	...	1	...	...	1	24	...	13	37
Makó	1	4	...	1	...	...	1	...	...	...	...	...	...	...	...	1	...	...	...	...	...	...	...	...	3	...	...	3
Cronstadt	...	3	1	1	2	...	...	...	...	...	...	...	1	...	...	...	1	...	...	...	...	...	...	...	2	3	...	5
Stuhlweissenburg	...	17	1	6	2	...	3	...	...	1	...	...	...	...	1	...	...	...	...	...	...	...	...	1	10	2	2	14
Jászberény	4	3	2	8	1	5	14	4	2	3	1	...	4	1	...	4	2	1	3	...	...	...	...	...	36	9	8	53
Raab	...	1	...	4	1	1	...	...	...	...	...	...	...	...	...	2	...	...	...	...	...	...	...	...	6	1	1	8
Szatmár-Németi	...	14	...	3	3	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	3	3	...	6
Zusammen	5	51	12	26	13	6	22	5	4	16	1	5	31	2	12	57	7	8	21	...	3	...	...	4	173	28	42	243
37-41. SUMME DER VERSCHIEDENEN KRANKHEITEN.																												
Budapest	3	654	8	10	15	...	13	4	2	10	1	1	22	1	11	53	4	14	262	31	105	1	...	2	371	56	135	562
Debreczen	2	87	1	5	11	...	4	1	...	2	...	...	2	...	...	4	1	2	68	17	10	...	...	...	85	30	12	127
Pressburg	...	66	...	...	2	...	1	...	...	...	...	...	1	...	...	4	...	...	72	33	8	...	...	...	78	35	8	121
Arad	1	122	1	1	...	...	2	...	1	4	...	4	3	...	3	15	...	4	35	12	32	...	...	1	60	12	45	117
Makó	6	86	...	27	8	...	3	...	...	2	...	...	1	...	1	18	1	2	66	2	9	...	2	...	117	13	12	142
Cronstadt	...	31	2	1	6	1	1	...	...	1	...	...	1	1	...	...	3	2	56	12	6	...	...	...	60	22	9	91
Stuhlweissenburg	...	66	1	6	2	...	4	...	...	1	...	...	...	...	1	...	1	...	1	...	37	...	...	1	12	3	39	54
Jászberény	21	79	3	12	3	5	17	4	2	4	1	...	6	1	...	7	2	1	58	6	16	...	...	...	104	17	24	145
Raab	1	32	...	4	1	1	2	...	...	1	...	...	...	...	...	6	...	...	27	1	...	...	...	...	40	2	1	43
Szatmár-Németi	1	14	...	3	3	...	1	...	...	...	...	...	1	...	...	2	...	...	10	16	...	...	...	...	17	19	...	36
Zusammen	35	1,237	16	69	51	7	48	9	5	25	2	5	37	3	16	109	12	25	655	130	223	1	2	4	944	209	285	1,438
42. TODTSCHLAG.																												
Budapest	...	1	2	...	...	1	...	...	1	5	...	2	1	...	1	2	...	...	...	...	...	...	...	...	8	...	5	13
Debreczen	...	...	...	...	...	...	1	...	...	1	...	...	...	...	...	...	...	...	1	...	...	...	...	...	3	...	...	3
Pressburg	...	...	...	...	...	...	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	1	...	...	1
Arad	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
Makó	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
Cronstadt	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
Stuhlweissenburg	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	1	...	...	...	...	...	...	...	...	1	1
Jászberény	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
Raab	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
Szatmár-Németi	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	1	...	...	...	...	...	...	...	...	1	1
Zusammen	...	1	2	...	...	1	2	...	1	6	...	2	1	...	1	2	...	2	1	...	...	...	...	...	12	...	7	19

43. SELBSTMORD.

Städte.	0-1 Jahr.			1-5 Jahre.			5-20 Jahre.			20-30 Jahre.			30-40 Jahre.			40-60 Jahre.			Über 60 Jahre.			Alter unbekannt.			Summe der Überjährigen.			
	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Zusammen.
Budapest	...	...	...	...	...	...	16	...	2	28	1	22	20	...	6	21	...	10	7	1	7	2	...	1	94	2	48	144
Debreczen	...	...	...	...	...	...	...	...	...	3	...	1	1	...	1	4	...	1	1	...	1	...	...	...	9	...	4	13
Pressburg	...	...	...	...	...	...	3	...	...	4	2	...	...	...	2	2	...	1	3	...	1	...	...	...	12	2	4	18
Arad	...	...	...	...	...	...	2	...	...	1	...	...	2	1	...	5	...	1	...	...	...	...	...	...	10	1	1	12
Makó	...	...	...	...	...	...	5	...	...	6	...	...	...	...	...	1	...	...	1	...	...	...	...	...	13	...	...	13
Cronstadt	...	...	...	...	...	...	2	...	...	1	...	...	...	...	1	...	...	...	...	...	1	...	...	...	3	...	2	5
Stuhlweissenburg	...	...	...	...	...	...	...	...	...	1	...	1	...	...	...	...	...	1	...	...	1	...	...	...	1	...	3	4
Jászberény	...	...	...	...	...	...	...	...	...	1	...	...	...	...	...	...	...	...	1	...	...	...	...	...	2	...	...	2
Raab	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	1	...	...	...	...	...	...	...	...	1	...	...	1
Szatmár-Németi	...	...	...	...	...	...	1	...	...	1	...	1	...	...	1	1	...	...	...	...	...	...	...	...	3	...	2	5
Zusammen	...	...	...	...	...	...	29	...	2	46	3	25	23	1	11	35	...	14	13	1	11	2	...	1	148	5	64	217

44. UNFÄLLE.

Städte.	0-1 Jahr.			1-5 Jahre.			5-20 Jahre.			20-30 Jahre.			30-40 Jahre.			40-60 Jahre.			Über 60 Jahre.			Alter unbekannt.			Summe der Überjährigen.			
	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Zusammen.
Budapest	1	2	1	3	2	2	15	...	...	18	1	5	22	1	4	23	2	3	5	...	2	1	...	...	87	6	16	109
Debreczen	...		...	2	...	...	1	...	...	...	...	...	...	...	...	1	...	1	...	...	...	...	...	...	4	...	1	5
Pressburg	...	1	...	...	...	...	...	...	2	6	...	1	2	...	...	2	2	1	2	...	...	...	...	...	12	2	4	18
Arad	...		...	2	...	...	2	...	...	1	...	...	1	...	...	2	...	...	1	...	...	...	...	...	9	...	...	9
Makó	...		...	2	...	...	1	...	...	1	...	...	...	...	...	...	...	...	1	...	...	...	...	...	5	...	...	5
Cronstadt	...		...	...	...	...	...	...	...	1	...	1	...	...	...	...	...	...	...	...	...	...	...	...	1	...	1	2
Stuhlweissenburg	...		...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
Jászberény	...		...	1	...	...	1	...	...	1	...	...	...	...	...	...	...	...	1	...	...	...	...	...	4	...	...	4
Raab	...		...	...	...	...	1	...	...	1	...	...	...	...	...	1	...	...	...	...	...	...	...	...	3	...	...	3
Szatmár-Németi	...		...	...	...	1	...	1	...	...	...	...	...	...	1	...	...	...	...	...	...	...	...	...	...	1	2	3
Zusammen	1	4	1	10	2	3	21	1	2	29	1	7	25	1	5	29	4	5	10	...	2	1	...	...	125	9	24	158

45. ZWEIFELHAFTE FÄLLE.

Städte.	0-1 Jahr.			1-5 Jahre.			5-20 Jahre.			20-30 Jahre.			30-40 Jahre.			40-60 Jahre.			Über 60 Jahre.			Alter unbekannt.			Summe der Überjährigen.			
	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Zusammen.
Budapest	...	1	5	...	...	...	2	...	7	1	...	7	1	...	12	...	1	9	1	1	1	...	...	8	5	2	44	51
Debreczen	...	...	...	...	...	...	...	...	...	...	...	...	1	...	1	...	...	1	...	...	1	...	...	...	1	...	3	4
Pressburg	...	...	...	...	...	...	2	...	...	...	...	11	...	...	10	...	...	2	2	...	...	...	...	...	4	...	23	27
Arad	...	1	...	...	...	...	2	...	...	...	...	...	1	...	1	...	...	1	1	...	...	...	...	...	4	...	2	6
Makó	...	1	...	1	1	...	2	...	...	1	...	...	...	...	...	1	...	...	...	...	...	...	...	...	5	1	...	6
Cronstadt	...	...	...	...	...	...	2	...	...	1	...	...	...	1	...	1	...	1	...	...	...	...	...	...	4	1	1	6
Stuhlweissenburg	...	...	...	...	...	...	1	...	...	...	...	...	...	...	...	...	...	2	...	...	...	...	...	...	1	...	2	3
Jászberény	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
Raab	...	...	...	...	...	...	2	...	...	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	3	...	...	3
Szatmár-Németi	...	1	...	...	...	...	2	...	...	2	...	...	...	...	...	...	...	...	...	1	...	...	...	...	4	1	...	5
Zusammen	...	4	5	1	1	...	15	...	7	6	...	18	3	1	24	2	1	16	4	2	2	...	...	8	31	5	75	111

41. SONSTIGE VERSCHIEDENEN KRANKHEITEN.

Städte.	0-1 Jahr.			1-5 Jahre.			5-20 Jahre.			20-30 Jahre.			30-40 Jahre.			40-60 Jahre.			Über 60 Jahre.			Alter unbekannt.			Summe der Überjährigen.			
	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Zusammen.
Budapest	...	3	8	2	3	...	2	1	1	7	...	1	20	1	8	36	3	4	12	...	1	...	...	2	79	8	17	104
Debreczen	...	2	...	1	1	...	1	...	...	1	...	...	2	...	...	...	1	...	2	...	1	...	...	...	7	2	1	10
Pressburg	...	4	...	...	...	...	1	...	...	...	...	...	1	...	...	1	...	...	...	...	...	...	...	...	3	...	...	3
Arad	...	...	...	...	...	...	...	...	1	4	...	4	3	...	3	13	...	3	4	...	1	...	...	1	24	...	13	37
Makó	1	4	...	1	...	...	1	...	...	...	...	...	...	...	...	1	...	...	...	...	...	...	...	...	3	...	...	3
Cronstadt	...	3	1	1	2	...	...	...	...	...	...	...	1	...	...	...	1	...	...	...	...	...	...	...	2	3	...	5
Stuhlweissenburg	...	17	1	6	2	...	3	...	...	1	...	...	...	...	1	...	...	...	...	...	...	...	...	1	10	2	2	14
Jászberény	4	3	2	8	1	5	14	4	2	3	1	...	4	1	...	4	2	1	3	...	...	...	...	...	36	9	8	53
Raab	...	1	...	4	1	1	...	...	...	...	...	...	...	...	...	2	...	...	...	...	...	...	...	...	6	1	1	8
Szatmár-Németi	...	14	...	3	3	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	3	3	...	6
Zusammen	5	51	12	26	13	6	22	5	4	16	1	5	31	2	12	57	7	8	21	...	3	...	...	4	173	28	42	243

37-41. SUMME DER VERSCHIEDENEN KRANKHEITEN.

Budapest	3	654	8	10	15	...	13	4	2	10	1	1	22	1	11	53	4	14	262	31	105	1	...	2	371	56	135	562
Debreczen	2	87	1	5	11	...	4	1	...	2	...	...	2	...	...	4	1	2	68	17	10	...	...	...	85	30	12	127
Pressburg	...	66	...	...	2	...	1	...	...	...	...	...	1	...	...	4	...	...	72	33	8	...	...	...	78	35	8	121
Arad	1	122	1	1	...	...	2	...	1	4	...	4	3	...	3	15	...	4	35	12	32	...	...	1	60	12	45	117
Makó	6	86	...	27	8	...	3	...	...	2	...	...	1	...	1	18	1	2	66	2	9	...	2	...	117	13	12	142
Cronstadt	...	31	2	1	6	1	1	...	...	1	...	...	1	1	...	...	3	2	56	12	6	...	...	...	60	22	9	91
Stuhlweissenburg	...	66	1	6	2	...	4	...	...	1	...	...	...	...	1	...	1	...	1	...	37	...	...	1	12	3	39	54
Jászberény	21	79	3	12	3	5	17	4	2	4	1	...	6	1	...	7	2	1	58	6	16	...	...	...	104	17	24	145
Raab	1	32	...	4	1	1	2	...	...	1	...	...	...	...	...	6	...	...	27	1	...	...	...	...	40	2	1	43
Szatmár-Németi	1	14	...	3	3	...	1	...	...	...	...	...	1	...	...	2	...	...	10	16	...	...	...	...	17	19	...	36
Zusammen	35	1,237	16	69	51	7	48	9	5	25	2	5	37	3	16	109	12	25	655	130	223	1	2	4	944	209	285	1,438

42. TODTSCHLAG.

Budapest	...	1	2	...	...	1	...	...	1	5	...	2	1	...	1	2	...	...	...	...	...	...	...	...	8	...	5	13
Debreczen	...	...	...	...	...	...	1	...	...	1	...	...	...	...	...	...	...	...	1	...	...	...	...	...	3	...	...	3
Pressburg	...	...	...	...	...	...	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	1	...	...	1
Arad	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
Makó	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
Cronstadt	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
Stuhlweissenburg	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	1	...	...	...	...	...	...	...	...	1	1
Jászberény	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
Raab	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
Szatmár-Németi	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	1	...	...	...	...	...	...	...	...	1	1
Zusammen	...	1	2	...	...	1	2	...	1	6	...	2	1	...	1	2	...	2	1	...	...	...	...	...	12	...	7	19

43. SELBSTMORD.

Städte.	0-1 Jahr.			1-5 Jahre.			5-20 Jahre.			20-30 Jahre.			30-40 Jahre.			40-60 Jahre.			Über 60 Jahre.			Alter unbekannt.			Summe der Überjährigen.			
	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Zusammen.
Budapest	...	...	...	...	...	...	16	...	2	28	1	22	20	...	6	21	...	10	7	1	7	2	...	1	94	2	48	144
Debreczen	...	...	...	...	...	...	...	...	...	3	...	1	1	...	1	4	...	1	1	...	1	...	...	...	9	...	4	13
Pressburg	...	...	...	...	...	...	3	...	...	4	2	...	...	...	2	2	...	1	3	...	1	...	...	...	12	2	4	18
Arad	...	...	...	...	...	...	2	...	...	1	...	...	2	1	...	5	...	1	...	...	...	...	...	...	10	1	1	12
Makó	...	...	...	...	...	...	5	...	...	6	...	...	...	...	...	1	...	...	1	...	...	...	...	...	13	...	...	13
Cronstadt	...	...	...	...	...	...	2	...	...	1	...	...	...	...	1	...	...	...	...	...	1	...	...	...	3	...	2	5
Stuhlweissenburg	...	...	...	...	...	...	...	...	...	1	...	1	...	...	...	...	...	1	...	...	1	...	...	...	1	...	3	4
Jászberény	...	...	...	...	...	...	...	...	...	1	...	...	...	...	...	...	...	...	1	...	...	..	...	...	2	...	...	2
Raab	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	1	...	...	...	...	...	..	...	...	1	...	...	1
Szatmár-Németi	...	...	...	...	...	...	1	..	...	1	...	1	...	...	1	1	...	...	...	...	...	...	...	...	3	...	2	5
Zusammen	...	...	...	...	...	...	29	...	2	46	3	25	23	1	11	35	...	14	13	1	11	2	...	1	148	5	64	217

44. UNFÄLLE.

Städte.	0-1 Jahr.			1-5 Jahre.			5-20 Jahre.			20-30 Jahre.			30-40 Jahre.			40-60 Jahre.			Über 60 Jahre.			Alter unbekannt.			Summe der Überjährigen.			
	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Zusammen.
Budapest	1	2	1	3	2	2	15	...	...	18	1	5	22	1	4	23	2	3	5	...	2	1	...	...	87	6	16	109
Debreczen	...	...	...	2	...	...	1	...	...	...	...	...	...	...	...	1	...	1	...	...	...	...	...	...	4	...	1	5
Pressburg	...	1	...	...	...	...	...	...	2	6	...	1	2	...	...	2	2	1	2	...	...	...	...	...	12	2	4	18
Arad	...	...	...	2	...	...	2	...	...	1	...	...	1	...	...	2	...	...	1	...	...	...	...	...	9	...	...	9
Makó	...	...	...	2	...	...	1	...	...	1	...	...	...	...	...	...	...	...	1	...	...	...	...	...	5	...	...	5
Cronstadt	...	1	...	...	...	...	...	...	...	1	...	1	...	...	...	...	...	...	...	...	...	...	...	...	1	...	1	2
Stuhlweissenburg	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
Jászberény	...	...	...	1	...	...	1	...	...	1	...	...	...	...	...	...	...	...	1	...	...	...	...	...	4	...	...	4
Raab	...	...	...	...	...	...	1	...	...	1	...	...	...	...	...	1	...	...	...	...	...	...	...	...	3	...	...	3
Szatmár-Németi	...	...	...	...	...	1	...	1	...	...	...	...	...	...	1	...	...	...	...	...	...	...	...	...	...	1	2	3
Zusammen	1	4	1	10	2	3	21	1	2	29	1	7	25	1	5	29	4	5	10	...	2	1	...	...	125	9	24	158

45. ZWEIFELHAFTE FÄLLE.

Städte.	0-1 Jahr.			1-5 Jahre.			5-20 Jahre.			20-30 Jahre.			30-40 Jahre.			40-60 Jahre.			Über 60 Jahre.			Alter unbekannt.			Summe der Überjährigen.			
	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Zusammen.
Budapest	...	1	5	...	...	...	2	...	7	1	...	7	1	...	12	...	1	9	1	1	1	...	...	8	5	2	44	51
Debreczen	...	...	...	...	...	...	...	...	...	...	...	...	1	...	1	...	...	1	...	...	1	...	...	...	1	...	3	4
Pressburg	...	...	...	...	...	...	2	...	...	...	...	11	...	...	10	...	...	2	2	...	...	...	...	...	4	...	23	27
Arad	...	1	...	...	...	...	2	...	...	...	...	...	1	...	1	...	...	1	1	...	...	...	...	...	4	...	2	6
Makó	...	1	...	1	1	...	2	...	...	1	...	...	...	...	...	1	...	...	...	...	...	...	...	...	5	1	...	6
Cronstadt	...	...	...	...	...	...	2	...	...	1	...	...	...	1	...	1	...	1	...	...	...	...	...	...	4	1	1	6
Stuhlweissenburg	...	...	...	...	...	...	1	...	...	...	...	...	...	...	...	...	...	2	...	...	...	...	...	...	1	...	2	3
Jászberény	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
Raab	...	...	...	...	...	...	2	...	...	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	3	...	...	3
Szatmár-Németi	...	1	...	...	...	...	2	...	...	2	...	...	...	...	...	...	...	...	...	1	...	...	...	...	4	1	...	5
Zusammen	...	4	5	1	1	...	15	...	7	6	...	18	3	1	24	2	1	16	4	2	2	...	...	8	31	5	75	111

42–45. SUMME DER GEWALTSAMEN TODESURSACHEN.

Städte.	0-1 Jahr.			1-5 Jahre.			5-20 Jahre.			20-30 Jahre.			30-40 Jahre.			40-60 Jahre.			Über 60 Jahre.			Alter unbekannt.			Summe der Über-einjährigen.			
	Gelmpft.	Ungeimpft.	Zweifelhaft.	Gelmpft.	Ungeimpft.	Zweifelhaft.	Gelmpft.	Ungeimpft.	Zweifelhaft.	Gelmpft.	Ungeimpft.	Zweifelhaft.	Gelmpft	Ungeimpft.	Zweifelhaft.	Gelmpft.	Ungeimpft.	Zweifelhaft.	Gelmpft.	Ungeimpft.	Zweifelhaft.	Gelmpft.	Ungeimpft.	Zweifelhaft.	Gelmpft.	Ungeimpft.	Zweifelhaft.	Zusammen.
Budapest	1	4	8	3	2	3	33	...	10	52	2	36	44	1	23	46	3	22	13	2	10	3	...	9	194	10	113	317
Debreczen	...	...	...	2	...	...	2	...	...	4	...	1	2	...	2	5	...	3	2	...	2	...	...	...	17	...	8	25
Pressburg	...	1	...	...	...	...	6	...	2	10	2	12	2	...	12	4	2	4	7	...	1	...	...	...	29	4	31	64
Arad	...	1	...	2	...	...	6	...	...	2	...	...	4	1	1	7	...	2	2	...	...	...	...	...	23	1	3	27
Makó	...	1	...	3	1	...	8	...	...	8	...	...	...	...	...	2	...	...	2	...	...	...	...	...	23	1	...	24
Cronstadt	...	1	...	...	...	...	4	...	...	3	...	1	...	1	1	1	...	1	...	...	1	...	...	...	8	1	4	13
Stuhlweissenburg	...	...	...	...	...	...	1	...	...	1	...	1	...	...	...	...	...	4	...	...	1	...	...	...	2	...	6	8
Jászberény	...	...	...	1	...	...	1	...	...	2	...	...	...	...	...	...	...	...	2	...	...	...	...	...	6	...	...	6
Raab	...	...	...	...	...	...	3	...	...	2	...	...	...	...	...	2	...	...	...	...	...	...	...	...	7	...	...	7
Szatmár-Németi	...	1	...	...	...	1	3	1	...	3	...	1	...	...	2	1	...	1	...	1	...	...	...	...	7	2	5	14
Zusammen	1	9	8	11	3	4	67	1	12	87	4	52	52	3	41	68	5	37	28	3	15	3	...	9	316	19	170	505
HAUPTSUMME.																												
Budapest	211	2,418	601	1,244	1,291	36	908	400	48	903	111	125	995	41	150	1,553	64	278	993	87	372	9	...	13	6,605	1,994	1,022	9,621
Debreczen	45	353	3	138	130	4	99	63	4	48	5	3	39	5	10	90	15	21	142	30	18	...	...	...	556	248	60	864
Pressburg	26	378	1	54	75	2	76	12	2	74	7	15	49	8	15	134	33	7	221	65	18	...	...	1	608	200	60	868
Arad	71	311	2	143	12	...	80	2	2	66	2	5	81	4	13	135	13	23	99	23	50	...	...	1	604	56	94	754
Makó	22	200	10	97	24	6	57	3	1	42	...	...	24	...	4	76	1	6	99	6	17	4	3	...	399	37	34	470
Cronstadt	2	130	6	43	105	5	48	11	4	30	1	4	27	7	3	66	6	6	90	15	10	4	3	...	308	148	32	488
Stuhlweissenburg	11	193	2	77	14	5	49	2	4	15	4	12	11	2	19	12	3	46	9	1	82	1	...	3	174	26	171	371
Jászberény	96	219	5	146	48	11	116	21	9	44	4	1	24	3	3	55	5	3	92	10	19	...	3	...	477	94	46	617
Raab	24	150	...	90	5	1	30	...	1	31	...	...	27	...	...	59	1	...	65	2	...	...	1	...	302	9	2	313
Szatmár-Németi	3	132	3	27	23	4	36	8	3	41	3	3	21	...	5	48	13	2	36	34	5	...	...	...	209	81	22	312
Zusammen	511	4,484	633	2,059	1,727	74	1,499	522	78	1,294	137	168	1,298	70	222	2,228	154	392	1,846	273	591	18	10	18	10,242	2,893	1,543	14,678
In Procenten	9.1%	79.7%	11.2%	53.4%	44.7%	1.9%	71.4%	24.9%	3.7%	80.9%	8.6%	10.5%	81.6%	4.4%	14.0%	80.3%	5.5%	14.2%	68.1%	10.1%	21.8%	39.1%	21.8%	39.1%	69.8%	19.7%	10.5%	100%
Summe der "Nichtblattern"	499	4,183	631	2,002	1,150	67	1,425	150	76	1,245	55	167	1,262	57	220	2,210	146	392	1,841	271	591	18	10	18	10,003	1,839	1,531	13,373
In Procenten	9.4%	78.7%	11.9%	62.2%	35.7%	2.1%	86.3%	9.1%	4.6%	84.9%	3.7%	11.4%	82.0%	3.7%	14.3%	80.4%	5.3%	14.3%	68.1%	10.0%	21.9%	39.1%	21.8%	39.1%	74.8%	13.8%	11.4%	100%

IMPFZUSTAND DER IN DER AMBULANZ DES BUDAPESTER STEPHANIE-KINDERSPITALS IM JAHRE 1874 BEHANDELTEN KINDER, NACH KRANKHEITEN UND MIT UNTERSCHEIDUNG DES ALTERS.

Krankheiten.	Unter 1 Jahr.					1-3 Jahre alt.					3-7 Jahre alt.					7-14 Jahre alt.					Über 14 Jahre.					Summe der über Einjährigen.				
	Geimpft.	Ungeimpft.	Geblattert.	Zweifelhaft.	Zusammen.	Geimpft.	Ungeimpft.	Geblattert.	Zweifelhaft.	Zusammen.	Geimpft.	Ungeimpft.	Geblattert.	Zweifelhaft.	Zusammen.	Geimpft.	Ungeimpft.	Geblattert.	Zweifelhaft.	Zusammen.	Geimpft.	Ungeimpft.	Geblattert.	Zweifelhaft.	Zusammen.	Geimpft.	Ungeimpft.	Geblattert.	Zweifelhaft.	Total.
Meningitis	...	...	...	...	...	1	...	...	...	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	1	...	...	...	1
Hydrocephalus	1	6	...	...	7	...	...	...	...	...	2	...	...	...	2	...	...	...	...	...	...	...	...	...	...	2	...	...	...	2
Convulsions	4	22	...	...	26	3	3	...	...	6	2	...	...	...	2	...	...	...	...	...	...	...	...	...	...	5	3	...	...	8
Vitia cordis	...	...	...	...	...	...	1	...	...	1	...	1	...	...	1	4	...	...	...	4	...	...	...	...	...	4	2	...	...	6
Croup	...	...	...	...	...	1	2	...	...	3	2	...	...	...	2	1	1	...	...	2	...	...	...	...	...	4	3	...	...	7
Dyphteritis	3	1	...	...	4	9	5	...	...	14	14	4	...	...	18	5	3	...	...	8	...	...	...	...	...	28	12	...	...	40
Pertussis	9	25	...	...	34	42	21	...	...	63	57	10	...	...	67	13	2	...	...	15	...	...	...	...	...	112	33	...	...	145
Pneumonia	27	223	1	1	252	99	118	2	...	219	133	35	...	3	171	22	5	...	...	37	2	...	...	...	2	266	158	2	3	429
Tuberculosis	4	...	...	...	4	4	2	...	...	6	2	...	...	...	2	2	...	...	...	2	1	...	...	...	1	9	2	...	...	11
Peritonitis	...	...	...	...	...	...	...	...	...	...	3	...	...	...	3	3	...	...	...	3	...	...	...	...	...	6	...	...	...	6
Enteritis	10	42	...	...	52	24	29	...	...	53	3	2	...	...	5	...	...	...	...	...	...	...	...	...	...	27	31	...	...	58
Diarrhœa	88	364	...	...	454	161	147	1	1	310	49	10	1	...	60	9	1	...	...	10	1	...	...	...	1	220	158	2	1	381
Rachitis	8	75	...	...	83	86	83	...	1	170	16	9	...	...	25	3	...	...	...	3	...	...	...	...	...	105	92	...	1	198
Morbilli	1	7	...	...	8	17	10	...	...	27	43	7	...	2	52	7	1	1	...	9	...	...	...	...	...	67	18	1	2	88
Scarlatina	2	1	...	...	3	16	3	...	...	19	20	7	...	1	28	8	...	...	...	8	...	...	...	...	...	44	10	...	1	55
Variola	**3**	**40**	...	...	**43**	**4**	**33**	**2**	...	**39**	**3**	**13**	**1**	...	**17**	**7**	**8**	...	...	**15**	**1**	...	...	...	**1**	**15**	**54**	**3**	...	**72**
Varicella	1	3	...	...	4	4	1	...	2	7	6	1	1	1	9	2	...	...	...	2	...	...	...	...	...	12	2	1	3	18
Typhus	...	...	...	...	...	3	4	...	...	7	17	1	...	...	18	19	...	...	...	19	...	...	...	...	...	39	5	...	...	44
Scrophulosis	...	5	...	...	5	18	18	...	...	36	38	5	...	...	43	23	...	...	...	23	11	...	...	...	11	90	23	...	...	113
Hydrops	1	...	...	...	1	3	1	...	...	4	8	4	...	...	12	3	2	...	...	5	...	...	...	...	...	14	7	...	...	21
Syphilis	1	80	...	...	81	4	6	...	...	10	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	4	6	...	...	10
Morbi cutanei: Appendix cutaneus	...	1	...	...	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
Cicatrix	...	...	...	...	...	...	...	...	...	...	1	1	...	...	2	2	...	...	...	2	...	...	...	...	...	3	1	...	...	4
Combustio	...	...	...	...	...	1	1	...	...	2	3	1	...	...	4	1	...	...	...	1	...	...	...	...	...	5	2	...	...	7
Congelatio	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	1	...	...	...	1	...	...	...	...	...	1	...	...	...	1
Contusio	2	3	...	...	5	9	7	...	...	16	16	1	...	...	17	13	...	...	...	13	2	...	...	...	2	40	8	...	...	48
Ecthyma	...	...	...	...	...	...	...	...	...	...	4	...	...	...	4	...	...	...	...	...	...	...	...	...	...	4	...	...	...	4
Eccema	8	43	...	...	51	48	30	...	...	78	51	13	1	...	65	39	1	...	2	42	4	...	...	...	4	142	44	1	2	189
Erysipelas	1	8	...	...	9	...	...	...	...	...	1	1	...	...	2	2	...	...	...	2	1	...	...	...	1	4	1	...	...	5
Erythema	...	7	...	...	7	1	...	...	...	1	1	1	...	...	2	2	...	...	...	2	1	...	...	...	1	5	1	...	...	6
Farus	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	2	...	...	...	2	1	...	...	...	1	3	...	...	...	3
Furunculosis	4	19	...	...	23	4	6	...	...	10	9	1	...	...	10	8	1	...	...	9	4	...	...	...	4	25	8	...	...	33
Gangræna cutis	...	3	...	...	3	1	1	...	...	2	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	1	1	...	...	2

Krankheiten.	Unter 1 Jahr.					1–3 Jahre alt.					3–7 Jahre alt.					7–14 Jahre alt.					Über 14 Jahre.					Summe der über Einjährigen.				
	Geimpft.	Ungeimpft.	Geblattert.	Zweifelhaft.	Zusammen.	Geimpft.	Ungeimpft.	Geblattert.	Zweifelhaft.	Zusammen.	Geimpft.	Ungeimpft.	Geblattert.	Zweifelhaft.	Zusammen.	Geimpft.	Ungeimpft.	Geblattert.	Zweifelhaft.	Zusammen.	Geimpft.	Ungeimpft.	Geblattert.	Zweifelhaft.	Zusammen.	Geimpft.	Ungeimpft.	Geblattert.	Zweifelhaft.	Total.
Morbi cutanei.																														
Herpes	...	1	...	...	1	...	...	...	...	...	2	...	...	...	2	1	...	...	...	1	...	1	...	...	1	3	1	...	...	4
Herpes zoster	1	...	...	...	1	...	1	...	...	1	1	...	...	...	1	2	...	...	...	2	...	...	...	...	...	3	1	...	...	4
Herpes tonsurans	...	1	...	...	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
Impetigo	...	1	...	...	1	1	2	...	...	3	1	...	...	...	1	1	...	...	...	1	...	...	...	...	...	3	2	...	...	5
Lichen	...	2	...	...	2	1	2	...	...	3	1	...	...	...	1	...	...	...	...	...	...	...	...	...	...	2	2	...	...	4
Lupus	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	2	...	...	...	2	...	...	...	...	...	2	...	...	...	2
Pemphigus	...	4	...	...	4	...	...	...	...	...	...	...	...	...	...	1	...	...	...	1	...	...	...	...	...	1	...	...	...	1
Pysiriasis	...	...	...	...	...	1	...	...	...	1	1	...	...	...	1	1	...	...	...	1	...	...	...	...	...	3	...	...	...	3
Prurigo	...	...	...	...	...	2	...	...	...	2	8	...	...	...	8	5	2	...	...	7	1	...	...	...	1	16	2	...	...	18
Psoriasis	...	...	...	...	...	...	...	...	...	...	1	...	...	...	1	1	...	...	...	1	...	...	...	...	...	2	...	...	...	2
Scabies	...	9	...	...	9	4	6	...	...	10	12	...	...	...	12	12	3	...	...	15	1	...	...	...	1	29	9	...	...	38
Ulcera	4	11	...	...	15	11	8	...	...	19	20	1	...	...	21	12	...	...	...	12	1	...	...	...	1	44	9	...	...	53
Urticaria	...	3	...	...	3	2	2	...	...	4	2	2	...	...	4	2	1	...	...	3	...	...	...	...	...	6	5	...	...	11
Vulnera	...	...	...	...	...	5	...	...	...	5	9	1	...	...	10	2	1	...	...	3	...	...	...	...	...	16	2	...	...	18
Veruca	...	...	...	...	...	...	...	...	...	...	2	...	...	...	2	4	...	...	...	4	...	...	...	...	...	6	...	...	...	6
Alii morbi	125	1,090	3	4	1,222	437	314	1	...	752	604	200	1	1	806	502	40	1	...	543	88	7	...	3	98	1,631	561	3	4	2,199
Summe	308	2.102	4	5	2,419	1,027	867	6	4	1,904	1,168	332	5	8	1,513	759	72	2	2	835	120	8	...	3	131	3,074	1,279	13	17	4,383*
In Procenten	12.7%	86.9%	0.2%	0.2%	100%	53.9%	45.6%	0.3%	0.2%	100%	77.2%	22.0%	0.3%	0.5%	100%	90.9%	8.7%	0.2%	0.2%	100%	91.6%	6.1%	...	2.3%	100%	29.2%	70.1%	0.3%	0.4%	100%
Summe der "Nichtblattern"	305	2,062	4	5	2,376	1,023	834	4	4	1,865	1,165	319	4	8	1,496	752	64	2	2	820	119	8	...	3	130	3,059	1,225	10	17	4,311
In Procenten	12.8%	86.8%	0.2%	0.2%	100%	54.9%	44.7%	0.2%	0.2%	100%	77.8%	21.4%	0.3%	0.5%	100%	91.7%	7.8%	0.25%	0.25%	100%	91.5%	6.2%	...	2.3%	100%	71.0%	28.4%	0.2%	0.4%	100%

* Es erscheinen im Protokolle überdies noch 55 Kinder, für welche keine Krankheit diagnostisirt wurde.

4. ÜBERIMPFUNG ANDERER KRANKHEITEN, NAMENTLICH DER SYPHILIS, DURCH DIE VACCINATION.

Bei Untersuchung dieser Frags wäre es natürlich das für die Impftheorie günstigste Resultat, wenn sich durch unsere Untersuchung weder für die Erkrankungs-, noch für die Sterbefälle ein ungünstiger Einfluss der Impfung ergeben würde.

Nun ist es aber im Vorhinein gewiss, dass von zwei Bevölkerungsklassen, wo deren eine bloss den allgemeinen Erkrankungsursachen, die andere aber neben diesen allgemeinen Ursachen auch noch einer speciellen Infectionsgefahr ausgesetzt ist (nämlich der Impfung), die letztere im Laufe einer längeren Zeit unbedingt eine grössere Anzahl von Infectionsfällen aufzuweisen haben werde. Für diesen Fall, wo nämlich die Wohlthaten der Impfung nicht ganz unentgeltlich genossen würden, sondern in der übernommenen Gefahr ein gewisser Preis für dieselbe gezahlt werden müsste, würde es sich darum handeln, zu erfahren, ob das Ding seinen Preis werth sei, ob bei Aufstellung einer Bilanz der Vaccination dieselbe activ oder passiv ausfalle, ob die Vor- oder die Nachtheile derselben überwiegen.

Indem wir nun auf diese Untersuchung der Klage wegen Impfschäden eingehen wollen, fehlt uns eigentlich ein streng formulirtes Petitum der klägerischen Partei. Es ist nicht präcise festgestellt, welche Krankheiten durch die Impfung inoculirt werden sollen. Bei Leuten, welche die Impfung selbst für Skoliose, Milcharmuth, ja sogar für Geisteskrankheiten verantwortlich machen, muss man gewärtig sein, eines schönen Tages selbst die unwahrscheinlichsten Krankheiten als durch die Impfung inoculirt genannt zu finden. Um nun solchen Klagen nach Möglichkeit zu begegnen, habe ich in meinen Tabellen die für die Berechnung der relativen Intensität nothwendigen Daten nach nahezu einem halben Hundert von Krankheiten specificirt. Man wird es mir hoffentlich erlassen, die Berechnung für jede dieser Krankheiten anzustellen und die Untersuchung, ob die Impfung nicht von Einfluss auf Herzkrankheiten, Nervenleiden, Knochenbrüche oder Selbstmorde etc. sei, ernst zu nehmen.

Unsere Untersuchung wird sich daher auf folgende Krankheiten beschränken: vor Allem auf *Syphilis* und *Rothlauf*, deren durch Impfung mögliche Uebertragung gegenwärtig anerkannt ist; ferner auf *Lungentuberculose*, *Croup* und *Diphtheritis*, *Rachitis*, *Skrophulose*, *Masern*, *Scharlach*, *Typhus*, sowie auf die *Hautkrankheiten*.

Wir können unsere Berechnungen sowohl auf die Erkrankungs- wie auf die Sterbefälle basiren. Es liegt nahe, den ersten den Vorzug zu geben, und zwar deshalb, weil das Hauptmoment, nämlich der Impfzustand, bei den Erkrankten verlässlicher erfahren werden kann, als bei den Gestorbenen. Dies erklärt auch, weshalb die Anzahl der zweifelhaften Fälle bei den Verstorbenen drei- bis viermal so viel als bei den Erkrankten beträgt.[1] Diesem grossen Procentsatze der zweifelhaften Fälle ist es also zuzuschreiben, wenn in der Mortalitätstabelle eine empfindlich geringere Anzahl von Geimpften zu finden ist, als in der Morbiditätstabelle. Wir werden uns also bei Beantwortung der Frage, ob Krankheiten durch Impfung inoculirt werden, in erster Reihe an die letztere Tabelle halten.

Bevor wir auf die Verhältnisse der einzelnen Krankheiten übergehen, müssen wir noch eine Bemerkung hinsichtlich der zum Maasse auszuwählenden Einheit vorausschicken. In den bisherigen Beweisen haben wir zwei Berechnungsarten (je nach den angewendeten zwei Maassen) durchgeführt, indem wir nämlich als jene Normalsterb-

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[1] Es findet sich nämlich unter je 100 Fällen (Uebereinjähriger) folgende Anzahl zweifelhafter Fälle:

	Bei Nichtblattern.	Bei den "indifferenten" Krankheiten.
Bei Erkrankten.....	3.4%	3.7%
Bei Verstorbenen.....	11.4%	14.3 (!)

lichkeit, welche Seitens der Impfung nicht beeinflusst wird, einerseits den Durchschnitt sämmtlicher Krankheiten mit Ausnahme der Blatternfälle ("Nichtblattern"), andererseits die Impfverhältnisse jener "indifferenten" Krankheiten betrachteten, für welche ein Einfluss der Impfung absolut nicht vorauszusetzen ist. In dem gegenwärtigen Abschnitte aber, wo wir für die einzelnen Krankheiten ebensowohl die Angaben der Morbiditäts- wie der Mortalitätstabelle zu verwerthen suchen, würden sich hiedurch für jede Krankheit je vier, ja bei Kinderkrankheiten durch Berücksichtigung der besonderen Tabelle des Kinderspitals, je sechs verschiedene Berechnungen und verschiedene Resultate ergeben! Kommt noch hinzu, dass wir die häufiger auftretenden Krankheiten auch noch nach sechs Altersgruppen, jede Altersgruppe aber wieder nach Geimpften und Ungeimpften (bez. Zweifelhaften) vorzuführen gedenken, so dass die Altersstatistik jeder dieser Krankheiten achtzehn Daten enthielte, was bei Anwendung der sechs verschiedenen Berechnungsarten die Anzahl jener Intensitätsziffern, aus denen über die Möglichkeit der Inoculation einer Krankheit geschlossen werden sollte, auf etwa 70—100 erhöhen würde, während strenge genommen eine einzige Intensitätsziffer oder höchstens — falls man die Altersklassen sondert — ein halbes Dutzend derselben vollkommen genügen würde. Man dürfte mir deshalb, nicht nur um überflüssige Mühe zu sparen, sondern auch um den Leser nicht zu verwirren, gestatten, dass ich in den nachfolgenden Ausführungen für die Mortalität nur eine Berechnungsweise, und zwar die von den indifferenten Krankheiten ausgehende, anwende. Diese bietet nämlich die grosse Bequemlichkeit, ein für alle Krankheiten gleiches, ständiges Maass abzugeben, während das andere für jede Krankheit speciell zu bestimmen wäre.

ALLGEMEINE ÜBERSICHT.

Bevor wir in eine Besprechung der Details eingehen, wollen wir erst ein übersichtliches Bild der Sachlage bieten. Es wird sich schon hieraus ein Urtheil über die Hauptfrage fällen lassen.

Die Statistik bietet nämlich in der Dispersion der Einzelnthatsachen vom Durchschnitte ein sehr bedeutsames Mittel, um erkennen zu lassen, ob die Schwankungen (die Abweichungen vom Mittel) bloss dem Spiel des Zufalles (bez. der bei Massenbeobachtungen unausweichlichen Unpräcision der Messung) oder dem Einflusse bestimmter Ursachen zuzuschreiben seien.[1] Im ersten Falle werden sich die Abweichungen in symmetrischer Abstufung rechts und links vom Mittel lagern. So ist z. B. die Anzahl der Menschen, die einen Centimeter grösser oder kleiner als die durchschnittliche Manneshöhe sind, gleich gross. Je weiter der Abstand vom Mittel ist, desto seltener werden die Fälle, und zwar wieder in nach beiden Seiten symmetrischer Abstufung, so dass z. B. die Anzahl der Riesen und der Zwerge eine gleich grosse ist: es liegt eben *im Begriffe des Zufalles*, alle Abstufungen zu berühren und ebenso häufig nach oben, wie nach unten vom Mittel abzuweichen. Sollte man nun finden, dass bei irgend einer Stufe der symmetrische Verlauf der Curven gestört wird, dass sich daselbst plötzlich eine Protuberanz oder eine Einsenkung zeigt, so kann dies kein Zufall mehr sein, sondern muss dieser Erscheinung eine an jener Stelle wirkende specielle Ursache zu Grunde liegen.

Wenn wir nun die Frage, ob die Impfung von Einfluss auf das Auftreten gewisser Krankheiten sei, durch die grössere oder geringere Anzahl der bei je einer Krankheit vorkommenden Geimpften beantworten wollen, so werden wir den Procentsatz von Geimpften in eine Scala aufstellen: der Mittelwerth ("Centralwerth"), welcher den durchschnittlichen Procentsatz von Geimpften, also jenen repräsentirt, bei welchem ein

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[1] Lexis. "Die Theorie der Massenerscheinungen in der menschlichen Gesellschaft" bietet eine sehr gründliche Darstellung dieser Theorie vom Durchschnitte.

specieller Einfluss der Impfung nicht anzunehmen ist, wird die meisten Krankheiten um sich versammeln müssen, während jene Krankheiten, welche speciellen Ursachen — darunter also eventuell auch dem Einflusse der Impfung — unterliegen, gegen die Grenzpunkte der Scala verschoben sein werden. Die Ausdehnung des Centralwerthes wird uns zugleich jene Latitude erkennen lassen, innerhalb welcher die aus Beobachtungsfehlern und sonstigen zufälligen Ursachen herrührenden Schwankungen oscilliren können. Bei einer genügend grossen Anzahl von Krankheiten müssten die grösseren Schwankungen als zugleich stets seltenere erkannt werden; bei einer geringeren Anzahl wird der Centralwerth sich doch immerhin als ein compaktes Centrum repräsentiren, während die Ausnahmefälle sich durch ihre excentrische Lage bemerkbar machen werden.

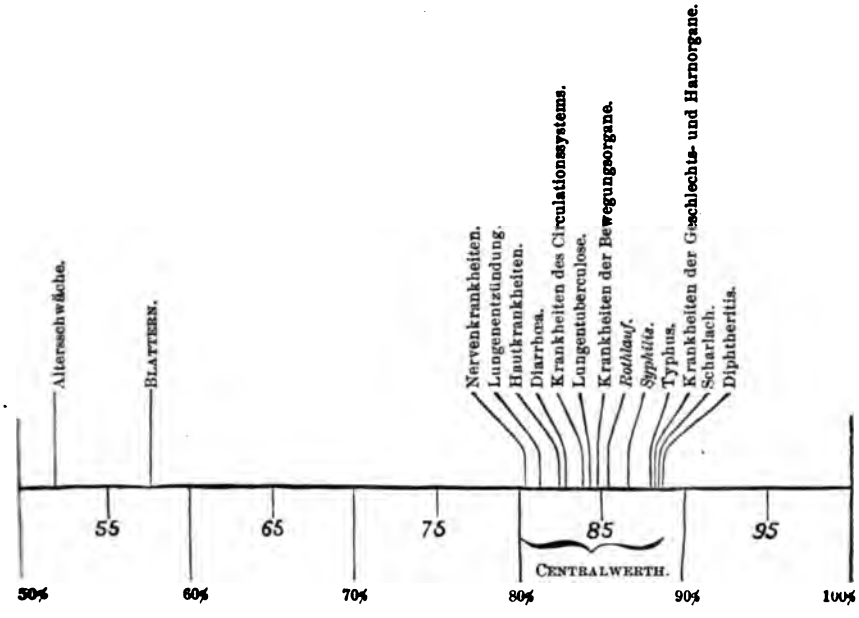
Untersuchen wir nun die Ergebnisse aus den neunzehn Spitälern:

DISPERSIONS-SCALA DER GEIMPFTEN UNTER DEN ERKRANKTEN DER NEUNZEHN SPITÄLER.

Es beträgt die Anzahl der Geimpften

unter 60 pCt. bei Altersschwäche 50.99; Blattern 56.69.
60—65 " Vacat.
65—70 " Vacat.
70—75 " Vacat.
75—80 " Vacat.
80—85 " " Nervenkrankheiten 80.57; Lungenentzündung 81.15; Unfälle 81.86; Hautkrankheiten 82.50; Diarrhœa 82.51; Krankheiten des Circulationssystems 83.24; Lungentuberculose 83.88; Krankheiten der Bewegungsorgane 84.54.
85—87½ " " Rothlauf 85.25; Syphilis 86.19; Typhus 87.28; Krankheiten der Geschlechts- und Harnorgane 87.30; Scharlach 87.44; Diphtheritis 87.50.

Wir versuchen im Nachfolgenden die graphische Darstellung dieser Verhältnisse:



Wir sehen demnach, dass es nur zwei excentrisch liegende Krankheiten gibt, bei denen die Anzahl der Geimpften eine auffällig geringe, also die der Ungeimpften eine auffällig grosse ist, nämlich *Blattern* und *Altersschwäche*. Es fragt sich nur noch, bevor man hieraus auf die Abhängigkeit dieser beiden Krankheiten von der Impfung

schliesst, ob es nicht bloss begleitende Umstände, so in erster Reihe die verschiedenartige Besetzung der Altersklassen sei, welche dieses abweichende Resultat verschuldet?

Was nun die Altersschwäche betrifft, so bestehen hier wirklich ganz exceptionelle Altersverhältnisse: es gibt hier nur Greise, was bei keiner anderen der Krankheiten der Fall ist. Sieht man nun nach, ob bei diesen ältesten Leuten der Impfzustand nicht ein von den übrigen Altersklassen verschiedener sei, so findet man, dass die Anzahl der Geimpften bei den alten Leuten wirklich bedeutend geringer ist, als unter den jüngeren Altersklassen. Es mag dieser Umstand darauf zurückzuführen sein, dass vor sechzig Jahren die Impfung ohne Zweifel viel seltener war, als in der späteren Zeit überdies wird aber mit vorrückendem Alter auch die Anzahl jener Fälle, wo der Impfzustand nicht bestimmt werden kann, stets zahlreicher: so beträgt dieselbe bei den 1—5-jährigen Kindern nicht einmal 1%, bei den über 60-jährigen Greisen aber über 9%, welcher Umstand ebenfalls eine Erhöhung des Procentsatzes der Geimpften hervorbringt.[1]

Die geringe Anzahl der Geimpften unter den an Altersschwäche Verstorbenen ergibt sich solcherart als natürliche Folge der Altersverhältnisse. Aber nicht so bei den Blattern: wir haben vorher gesehen, wie häufig die Ungeimpften den Blattern in den verschiedensten Lebensverhältnissen zum Opfer fallen.

Mit Ausnahme dieser beiden Krankheiten also, deren excentrische Lage wirklich auf die Wirkung specieller Ursachen zurückgeführt werden könnte, sehen wir, dass sich sämmtliche übrigen Krankheiten um den Procentsatz von 80½ bis 87½ % drängen. Auch der Durchschnitt sowohl für die Nichtblattern (83.9 %, siehe Tabelle I), als für die indifferenten Krankheiten (82.9 %, siehe Beilage I), befindet sich innerhalb dieses Schwergewichtscentrums. Berücksichtigt man, welch grosse Divergenz in jenem einem Falle eintritt, wo die Impfung wirklich einen bestimmten Einfluss ausübt (nämlich bei Blattern), so wird man schon aus dem Zusammendrängen aller übrigen Krankheiten um einen verhältnissmässig engen Centralwerth zu dem Schlusse geleitet, dass die Impfung auf *diese* nicht von Einfluss sein konnte, dass also schon im Vorhinein ausgesprochen werden kann, dass durch die Impfung keine der genannten Krankheiten befördert — also auch nicht übertragen wurde. Dies gilt für die Beobachtungen der neunzehn Spitäler, also für Erwachsene.

Um zu erfahren, ob sich diese Verhältnisse bei Kindern ändern, wollen wir die Beobachtungen des Stephanie-Spitals in gleicher Weise verwerthen. Leider finden wir daselbst bloss fünf Krankheiten, von welchen mehr als hundert Uebereinjährige zur Verfügung stehen, darunter aber die für unsere Zwecke besonders wichtige Rubrik der Hautkrankheiten, der Rachitis und der Scropheln.

Im Totale der indifferenten Krankheiten fanden sich hier 70 % Geimpfte (siehe Beilage II); hingegen

bei Rachitis	50½	Procent
bei Lungenentzündung	62	"
bei Keuchhusten	76½	"
bei Hautkrankheiten	78.8	"
und bei Scropheln	79.6	"

Die Dispersion ist demnach eine beträchtliche, und zwar mehr nach unten als nach oben.

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[1] Die in den höheren Altersklassen stetig abnehmende Zahl der Geimpften und die starke Zunahme der zweifelhaften Fälle ergibt sich sehr klar aus der nachfolgenden Zusammenstellung:

Unter je 100 Kranken gab es:

	Geimpfte	Zweifelhafte Fälle
im Alter von 20—30 Jahren	87.4%	2.2%
" " " 30—40 "	85.4%	3.7%
" " " 40—60 "	79 1%	5.1%
über 60 "	25.5%	9.1%.

Der Umstand, dass bei den an Rachitis erkrankten Kindern sich auffällig wenig Geimpfte befinden, spricht immerhin für die Unwahrscheinlichkeit der Ansicht, als ob diese Krankheit durch die Impfung verbreitet würde; die sich bei Scropheln und Hautkrankheiten zeigende geringe Steigerung hingegen müsste, falls sich ähnliche Ergebnisse auch bei Fortsetzung dieser Beobachtungen, und auch bei reicherem als dem gegenwärtigen Material, bestätigen sollten, wohl auf Rechnung der Impfung geschrieben werden.[1]

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[1] Der Vollständigkeit halber wollen wir hier auch die Dispersion der Todesfälle mittheilen, obgleich dieselben in Folge der oben erwähnten Umstände weniger verlässlich erscheinen.

DISPERSIONS-SCALA DES PROCENTSATZES DER GEIMPFTEN UNTER DEN VERSTORBENEN AUS ZEHN UNGARISCHEN STÄDTEN VOM JAHRE 1886.

Es beträgt die Anzahl der Geimpften

unter 60 pCt. bei Blattern 18.31; Masern 53.44; Diarrhœa 54.55.
60—65 " " Altersschwäche 64.05.
65—70 " " Selbstmord 68.20; Pneumonia 69.69: Krankheiten der Bewegungsorgane 69.75.
70—75 " " Wassersucht 70.43; Nervenkrankheiten 72.09.
75—80 " " Bauchfellentzündung 76.03; Diphtheritis 76.23; Croup 76.93; Unfälle 79.11.
80—85 " " Krankheiten der Geschlechts- und Harn-Organe 80.58; Scharlach 80.75; Krankheiten des Circulationssystems 81.31; Lungentuberculose 81.61.
über 85 " " Typhus 86.46; Cholera 88.89.

Der Durchschnitt sämmtlicher "Nichtblatternfälle" beträgt in diesen Städten (für die Uebereinjährigen) 74.8 pCt.,* jener der "indifferenten" Fälle 70.8.† Schon dieser empfindliche Abstand in den beiden Durchschnittsmaassen lässt die Daten unserer Mortalitätstabelle weniger anwendbar als jene der Morbiditätstabelle erscheinen. Der Centralwerth der obigen Zusammenstellung reicht von 76 pCt. bis 81½ pCt., die Dispersion ist demnach eine sehr beträchtliche. Unterhalb des Centralwerthes finden wir in erster Reihe die Blattern. Dass bei dieser Krankheit die Anzahl der Geimpften den niedrigsten Stand unter allen anderen Krankheiten erreicht, ist eben wieder jener glänzende Beweis für die Schutzkraft der Impfung, für die Thatsache, *wie selten Geimpfte an Blattern sterben.* Dass die stärkere Besetzung des Kindesalters auf diese Thatsache ohne Einfluss ist, wurde in den vorhergehenden Abschnitten zur Genüge nachgewiesen. Wohl aber ist es die eigenthümliche Besetzung der Altersklassen, welche verursacht, dass unter den an Masern und Diarrhœ verstorbenen, zumeist in dem zartesten Alter stehenden Kindern, sowie unter den an Altersschwäche verstorbenen Greisen, sich wenige Geimpfte finden.

Die Stelle, welche auf der anderen Seite Typhus und Cholera einnehmen, ist ganz exceptionell. Fälle, dass nach der Impfung sich Typhus oder Cholera eingestellt hätte, sind noch nie bemerkt worden. Ein Zusammenhang zwischen Impfung und diesen Krankheiten ist ferner schon deshalb nicht anzunehmen, weil die letzteren, namentlich aber die Cholera, nur in sporadischen Epidemieen auftreten, also ihre Entstehung ganz specifischen Krankheitskeimen zu verdanken haben, und sich überdies zwischen den Epidemieen von Blattern und Cholera gar kein Zusammenhang nachweisen lässt. Es muss weiteren Beobachtungen anheimgestellt bleiben, zu constatiren, ob die grosse Anzahl von geimpften Cholerakranken sich auch anderwärts bestätigt.

Mit Rücksicht auf den Umstand aber, dass Seitens der Impfgegner ein derartiger Zusammenhang zwischen Blattern und Typhus behauptet wird, als ob seit Abnahme der Blattern der Typhus an deren Stelle getreten sei — auf wie schwachen Füssen übrigens diese Behauptung stehe, wurde bereits im achten Kapitel nachgewiesen —, soll im Nachfolgenden auch die Eventualität von Typhus-Inoculationen im Auge behalten und namentlich auf dem Wege der Morbiditäts-Beobachtungen weiter verfolgt werden.

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* Vergl. Tabelle II.

† Vergl. Beilage I.

UNTERSUCHUNG DER EINZELNEN KRANKHEITEN.

Indem wir uns nun anschicken, auf die Untersuchung der Ueberimpfbarkeit der einzelnen Krankheiten überzugehen, wollen wir vor Allem einen Ueberblick über das zur Verfügung stehende Material bieten, und zwar in erster Reihe über jenes, welches den Beobachtungen der neunzehn Spitäler vom Jahre 1886 angehört, dann über die Beobachtungen aus dem Stephanie-Kinderspital vom Jahre 1874, wobei wir gegenwärtig nur auf die mindestens hundert Fälle zählenden Krankheiten (die nachfolgend mit einem Sternchen bezeichnet sind) reflektiren können; für die schwächer vertretenen Krankheiten uns vorbehaltend, auf dieselben zurückzukommen, sobald dieselben im Laufe der Zeit durch Fortsetzung solcher Beobachtungen die gewünschte Höhe erreichen werden. Es folgen dann in dritter Reihe die mortalitäts-statistischen Daten, welche aber nach dem Gesagten nur den Werth subsidiärer Aussagen haben.

	KRANKENFÄLLE.		TODESFÄLLE.
	a) 19 ungarische Spitäler, 1886.	*b*) Stephanie-Kinderspital, 1874.	10 ungarische Städte, 1886.
1. Syphilis	2716*	10	7
2. Rothlauf	145*	s. Hautkrankheiten	21
3. Lungentuberculose	1141*	11	3148*
4. Croup und Diphtheritis	132*	47	600*
5. Rachitis	4	198*	46
6. Scrophulose	85	113*	59
7. Typhus	283*	44	288*
8. Masern	75	88	113*
9. Scharlach	199*	55	618*
10. Hautkrankheiten	1051*	478*	34

(Stets nur Uebereinjährige gerechnet.)

1) Das Hauptinteresse wendet sich der *Syphilis* zu; ist doch die Furcht, durch die Lymphe syphilisirt zu werden, das furchtbarste der gegen die Impfung, namentlich aber der gegen die Zwangsimpfung vorgebrachten Argumente.

In den 19 Spitälern, deren Verhältnisse ich beobachten konnte, wurden nun an Syphilis behandelt:

	Geimpfte.	Ungeimpfte.	Zweifelhafte.	Zusammen.
0— 1 Jahr	5	4	—	9
1— 5 Jahre	8	9	—	17
5—20 "	607	76	20	703
20—30 "	1246	147	20	1413
30—40 "	313	47	9	369
40—60 "	151	24	14	189
Ueber 60 "	13	5	4	22
Alter unbekannt	3	—	—	3
Summe der Uebereinjährigen	2341	308	67	2716

Stellen wir diesen Zahlen jene entgegen, die sich für die indifferenten Krankheiten ergeben, so entfallen auf je 100 Behandelte Geimpfte:

	bei Syphilis	bei den indifferenten Krankheiten	falls letztere = 100 (relative Intensität)
Uebereinjährige	86.2	82.9	104
Hiervon in den Altersgruppen:			
von 1— 5 Jahren	*	71.2	*
5—20 "	86.3	88.5	98
20—30 "	88.2	87.1	101
30—40 "	84.8	86.1	98
40—60 "	79.9	79.0	101
über 60 "	*	63.1	*

Wenn wir nun vor Augen halten, dass die Gefahr, an Blattern zu erkranken, bei den Ungeimpften eine $3\frac{1}{3}$-fach grössere als bei den Geimpften ist, können wir nach Obigem

behaupten, dass sich *kein Unterschied* für das Impfprocent der syphilitischen Erkrankungen ergibt, beziehungsweise das der bei Blattern als Folge der Impfung constatirten Steigerung von 329 %, bei Syphilis nur eine von 4 % entspricht! Aus der Gegenüberstellung dieser Zahlen muss man zum Mindesten so viel ersehen, wie verschwindend gering der mögliche Schaden der Impfung gegen den durch dieselbe gebotenen Nutzen sein müsse. Man darf aber mit gutem Gewissen behaupten, dass — wenigstens in dem vorliegenden Material — die durch Impfung herbeigeführte Syphilisirung gleich Null ist: der sich oben ergebende, nicht in's Gewicht fallende, Ausschlag von 4 % darf wohl getrost bloss als Folge jener unvermeidlichen Oscillationen betrachtet werden, welche bei Abwägung von Massenerscheinungen stets vorkommen. Für die Berechtigung einer solchen Erklärung spricht auch der Umstand, dass in manchen Altersklassen das Procent der syphilitischen Geimpften geradezu eine *geringere* als die der Ungeimpften ist. Will man solche Erscheinungen nicht durch jene unvernünftige Annahme erklären, dass die Impfung die Gefahr der Syphilisirung vermindere, so muss man solche geringe Schwankungen als unausweichliche Fehlergrenzen des nicht genug präcisen Messinstrumentes betrachten. Falls die Anzahl solcher Beobachtungen sich vermehren sollte, wird — namentlich falls sich Fälle ergäben, wo unter den Syphilitischen weniger Geimpfte anzutreffen sein werden, als bei den übrigen Krankheiten — sich bald herausstellen, dass man es hier bloss mit zufälligen Oscillationsschwankungen und nicht mit einem ständigen Causalnexus zu thun habe.

2) An *Rothlauf* wurden behandelt:

124 Geimpfte,
17 Ungeimpfte,
4 zweifelhafte Fälle,

insgesammt 145 Fälle. Die Geimpften betragen 85.5 % der Erkrankten; da die indifferenten Krankheiten ein Impfprocent von 82.9 ergeben, so gelangen wir hier zu dem gleichen Resultat wie bei der Syphilis. *Es hat also auch der Rothlauf durch die Impfung keine Steigerung erfahren.*

3) An *Lungentuberculose* sind behandelt worden:

im Alter von	Geimpfte	Ungeimpfte	Zweifelhafte	Zusammen
0— 1 Jahr	—	—	1	1
1— 5 Jahren	6	1	—	7
5—20 "	94	12	2	108
20—30 "	314	29	9	352
30—40 "	233	35	13	281
40—60 "	267	46	14	327
Ueber 60 "	42	18	5	65
Alter unbekannt	1	—	—	1
Summe der Uebereinjährigen	957	141	43	1141

Der Procentsatz der Geimpften ist also bei der Lungentuberculose (83 %) *genau derselbe*, wie bei den indifferenten Krankheiten; wir können also hieraus die tröstliche Folgerung ziehen, *dass die Behauptung, als ob Lungentuberculose durch die Impfung übertragen werde, ganz unberechtigt ist.*

Bei einem Eingehen auf die Verhältnisse der einzelnen Altersklassen zersplittert sich das Material in kleine Gruppen; wenn wir es trotzdem versuchen, für jene vier Altersgruppen, für welche wenigstens 100 Fälle vorliegen, diese Berechnung anzustellen, so finden wir, dass im Ganzen auch hier die beiden Procentsätze so ziemlich zusammenfallen. Der Umstand, dass bei den Tuberculosen in einzelnen Altersgruppen 2—3 % mehr Geimpfte vorkommen, als bei den indifferenten Krankheiten, wird dadurch, dass in anderen Altersklassen geradezu weniger Geimpfte

vorkommen, balancirt und erscheint schon in Folge dieses Umstandes als blosse Schwingungszahl innerhalb der Fehlerlatitude.

Die Gegenüberstellung des Procentsatzes der Geimpften, einerseits bei den an Lungentuberculose, andererseits bei an den indifferenten Krankheiten Erkrankten, ergibt nämlich folgende Resultate:

Auf je 100 Behandelte entfallen Geimpfte:

	bei Lungentuberculose	bei den indifferenten Krankheiten	letztere = 100 gesetzt (relative Intensität)
Uebereinjährige.....	83.9	82.9	101
u. z. 1— 5 Jahre.....	*	71.2	*
5—20 "	87.0	88.5	98
20—30 "	89.2	87.1	102
30—40 "	82.9	86.1	96
40—60 "	81.7	79.0	103
Ueber 60 "	*	63.1	*

Für die Tuberculose bietet uns auch die mortalitätsstatistische Tabelle Material, und entnehmen wir derselben folgende Verhältnisse:

Es starben an Tuberculose:

im Alter von	Geimpfte	Ungeimpfte	Zweifelhafte	Zusammen
0— 1 Jahr.....	38	154	38	230
1— 5 Jahren.....	166	125	6	297
5—20 "	365	31	30	426
20—30 "	612	27	52	691
30—40 "	562	18	85	665
40—60 "	645	34	96	775
Ueber 60 "	215	13	61	289
Alter unbekannt.....	4	—	1	5
Summe der Uebereinjährigen	2569	248	331	3148

Von je 100 Personen der betreffenden Altersgruppe waren geimpft:

	bei Tuberculose	bei den indifferenten Krankheiten	die letzteren = 100 gesetzt (relative Intensität)
über ein Jahr.....	81.6	70.7	115
u. z. 1— 5 Jahre.....	55.9	58.5	96
5—20 "	85.7	86.5	99
20—30 "	88.6	78.1	113
30—40 "	84.5	78.1	108
40—60 "	83.2	78.7	106
Ueber 60 "	74.4	66.7	112

Während sich also bei den Erkrankungsfällen gar kein Ausschlag zu Ungunsten der Geimpften ergab, findet sich bei den Todesfällen doch ein solcher, wenn auch nur in der Höhe von 15 %. Vergleicht man aber die beiden Tabellen, so findet man, dass die Anzahl der zweifelhaften Fälle bei den Erkrankten eine viel geringere, nämlich nur 3.8 %, ist, als bei den Verstorbenen, wo dieselbe 10.5 % beträgt. Es möge überdies wieder darauf hingewiesen werden, dass der Durchschnitt der indifferenten Krankheiten um 4 % tiefer als jener der "Nichtblattern" steht. Zieht man diese beiden Umstände in Betracht, so reducirt sich der obige Ausschlag von 15 % auf die Hälfte, wodurch derselbe also innerhalb die Grenzen der Beobachtungsfehler fällt.

4) *Croup* und *Diphtheritis* boten nur 132 Krankenfälle. Diese zeigen aber fast gar keinen Ausschlag gegen die indifferenten Krankheiten. Die bei den Todesfällen sich zeigende geringe Steigerung von 8 % wird durch die vorher erwähnten Umstände des

zu tiefen Durchschnittes der indifferenten Krankheiten beinahe ganz aufgewogen. Croup und Diphtheritis scheinen also ebenfalls durch Impfung nicht übertragen zu werden.

5) Für *Rachitis* können wir uns nur an die 198 Fälle des Kinderspitals halten. Aus Beilage No. II ist ersichtlich, dass das Impfprocent der indifferenten Krankheiten 70 % beträgt,[1] für Rachitis aber ergibt sich aus Tabelle III nur ein Stand von 53 %, nämlich 105 unter 198 (übereinjährigen) Kranken. Wir wollen nun nicht behaupten, dass die Impfung vor Rachitis geschützt habe; der geringe Procentsatz Geimpfter unter den Rachitischen muss eine Folge anderer hier mitwirkender, uns unbekannter, Ursachen sein. So viel ist aber sicher, dass nach Obigem, und insoweit man diese wenigen Beobachtungen für genügend betrachtet, *von der Gefahr einer Ueberimpfung der Rachitis nicht die Rede sein kann.*

6) Für *Scropheln* liegen noch weniger Fälle, nämlich nur 113 (und zwar wieder aus dem Kinderspitale) vor. Die Anzahl der Geimpften betrug 90, also 79 % statt der normalen 70 %. Der an und für sich geringe Ausschlag—man wolle nämlich nie aus den Augen verlieren, dass dort, wo ein Einfluss der Impfung constatirt werden konnte, dies gleich einen Ausschlag von Hunderten, ja in einzelnen Altersklassen von über 1500 Procenten verursachte! — braucht überdies schon wegen der zu geringen Anzahl der Fälle nicht in Betracht gezogen zu werden. Es dürfte aber angezeigt sein, die eventuell dennoch mögliche Uebertragung dieser Krankheit bei Erweiterung der diesbezüglichen Beobachtungen besonders aufmerksam zu verfolgen.

7) *Typhus.* Von 283 Erkrankten waren 247 geimpft, = 87 %, demnach kaum etwas mehr, als im Durchschnitte der indifferenten Krankheiten (83 %). Auf Grund der Erkrankungen liesse sich also auch die Wahrscheinlichkeit der Ueberimpfung von Typhus nicht vertheidigen. Bei den Verstorbenen zählte man 86½ %; hier finden

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[1] Bei den aus den allgemeinen Spitälern geschöpften Beobachtungen, wo alle nennenswerthen Krankheiten einzeln angeführt wurden, waren wir in der Lage, die Summe der indifferenten Krankheiten durch taxative Aufzählung festzustellen. Bei Aufarbeitung der Beobachtungen des Kinderspitals haben wir die *Hautkrankheiten* besonders eingehend specificirt, sind aber hiefür bei den übrigen Krankheiten nicht in zu weite Specificirungen eingegangen. In Folge der abweichenden Anlage der Kinderspitalstabelle ist man nun nicht in der Lage, alle vorher als indifferente bezeichneten Krankheiten in derselben vorzufinden. Wir haben in Folge dessen die Summe der indifferenten Krankheiten hier derart festgestellt, dass wir von dem Totale sämmtlicher Krankheiten jene abzogen, für welche irgend ein Verdacht der Ueberimpfbarkeit ausgesprochen werden konnte, nämlich alle Hautkrankheiten, ferner Blattern und Varicella, Syphilis, Croup, Diphtheritis, Keuchhusten, Lungentuberculose, Tpyhus, Rachitis, Scropheln, Masern und Scharlach. Es ergaben sich solcherart für die indifferenten Krankheiten folgende Zahlen (siehe die zweite Beilage):

IMPFZUSTAND BEI DEN "INDIFFERENTEN" KRANKHEITEN DES KINDERSPITALS I. J. 1874.

	Geimpft.	Ungeimpft.	Geblattert.	Zweifelhaft.	Zusammen.
0— 1 Jahr	254	1749	4	5	2012
1— 3 Jahre	726	613	4	1	1344
3— 7 "	802	252	2	4	1060
7—14 "	551	48	1	—	600
Ueber 14"	90	7	—	3	100
Summe der Uebereinjährigen..	2169	920	7	8	3104

Von je 100 Behandelten waren demnach geimpft:

Uebereinjährige	69.9
und zwar im Alter von 1— 3 Jahren	54.0
3— 7 "	75.7
7—14 "	91.8
über 14 Jahren	90.0

sich also um 22 % mehr Geimpfte, als bei den indifferenten Krankheiten. Der Ausschlag erscheint demnach höher, als bei den bisher aufgezählten Krankheiten. Wenn auch hiebei in Betracht gezogen werden sollte, dass die als indifferent ausgewählten wenigen Krankheiten bei den Todesfällen einen notorisch um etwa 30 % zu tiefen Procentsatz von Geimpften aufweisen, so dass z. B., an dem Durchschnitte der "Nichtblattern" gemessen, der Ausschlag nunmehr 15 % betrüge; so auch, dass die Ergebnisse der Morbiditätsstatistik — hinter welchen jene der Mortalitätsstatistik zurückstehen müssen — keinen Anlass zur Annahme von durch Impfung übertragbarem Typhus gaben: wollen wir doch, angesichts des widersprechenden Inhaltes der Morbiditäts- und der Mortalitäts-Beobachtungen, die Frage von der Ueberimpfbarkeit des Typhus bis zum Hinzutritt der Morbiditäts-Beobachtungen der nächsten Jahre in Schwebe lassen.

8) *Masern.* Hier stehen wieder nur Mortalitätsbeobachtungen, und zwar in sehr geringer Anzahl, zur Verfügung; man wird also wohlthun, auch hier das Anwachsen der Morbiditätsbeobachtungen abzuwarten. Da der Procentsatz der Geimpften unter den Masern-Verstorbenen nur 50 %, also viel weniger als bei den indifferenten Krankheiten, beträgt, liegt vorläufig gewiss kein Grund zu einer Annahme der Ueberimpfung vor.

9) *Scharlach.* Die Morbiditätstabelle zeigt uns, dass unter den Erkrankten 87.4 % geimpft waren, was also gegen den Normalsatz von 82.9 % einen kaum ernst zu nehmenden, schwachen Ausschlag ergibt. Mit Rücksicht auf die Wichtigkeit dieser Frage habe ich aber auch die in der Klinik des Kinderspitals (1874) behandelten, freilich zu spärlichen, Scharlachfälle ausgezogen. Ich fand im Ganzen 24 (Uebereinjährige); addiren wir hierzu die 55 in der Ambulanz vorgekommenen Fälle, so gewinnen wir

für die Ambulanz	44 Geimpfte,	10 Ungeimpfte,	1 Zweifelh.,	Zus. 55
" " Klinik	19 "	3 "	2 "	" 24
Zusammen....	63 Geimpfte,	13 Ungeimpfte,	3 Zweifelh.,	Zus. 79

also 79 % Geimpfter, während der Durchschnitt der Ambulanz und Klinik (mit Ausschluss der Blattern) einen Procentsatz von nur 71½ ergibt.[1] Da sich nun auch hier ein, zwar geringer, aber doch zu Ungunsten der Impfung sprechender Ausschlag ergibt, und auch die Mortalitätsbeobachtungen einen geringen, ungünstigen Ausschlag aufweisen, werden die weiteren Beobachtungen über Scharlach mit Interesse verfolgt werden müssen.

10) *Hautkrankheiten.* Zu den wichtigsten und lehrreichsten Aufschlüssen sind unbedingt jene zu rechnen, welche in den Verhältnissen der Hautkrankheiten geboten werden. Nach den von *Reitz* aus dem St. Petersburger Findelhause mitgetheilten Daten sollte die Impfung an sich — selbst ohne Annahme der Ueberimpfung einer fremden Krankheit — sehr häufig Hautkrankheiten hervorrufen und sollen tödtliche Fälle nicht sehr selten, sondern recht oft vorkommen.[2]

Um nun vor Allem mit den Todesfällen ins Reine zu kommen, wollen wir bemerken, dass unter den unserer Beobachtung unterworfenen 14,678 Todesfällen im Ganzen

[1] Es betrug nämlich die Anzahl der (übereinjährigen) Geimpften unter den Nichtblatterfällen

in der Klinik.....	520 unter	691 Fällen,
" " Ambulanz.....	3059 "	4312 "
Zusammen.....	3579 unter	5002 Fällen, — 71½ %.

[2] "Ich fand, dass erysipelatöse Processe und Septicæmia post vaccin. häufig die Todesursachen waren; diese aber sind solche Erkrankungen, welche direkt durch das Einimpfen des Kuhpockengiftes hervorgerufen werden." (Reitz. "Versuch einer Kritik der Kuhpockenimpfung." Seite 34.) Ferner Seite 38: "Diese Zahlen widerlegen schon vollkommen die Ansicht, dass die Vaccination nur 'ausnahmsweise' den Tod herbeiführt, denn die Ausnahmen waren zu häufig."

nicht mehr als **34** (!) durch Hautkrankheiten verursacht wurden[1]: es kann also davon, dass die Impfung auf dem Wege der Hautkrankheiten häufig zum Tode führe, absolut keine Rede sein. Desto zahlreicher sind aber die dermatologischen Erkrankungsfälle, nämlich 1051. Hier böte sich nun günstige Gelegenheit, die gesundheitsschädliche Wirkung der Impfung auf ihrem eigensten Gebiete zu untersuchen. Der Durchschnitt der indifferenten Krankheiten zeigt, wie bekannt, einen Procentsatz von 82.9%, jener der Nichtblattern von 83.9% an Geimpften, die Hautkrankheiten aber nur 82.5%! Es scheint demnach, dass man mit grösster Beruhigung die Behauptung aussprechen darf, dass bei Erwachsenen die Impfung weder ernstere Hautreize hervorrufe (mindestens keine solche, derentwillen die Betreffenden ein Spital aufsuchen), noch aber andere Krankheiten überimpft würden. Wir werden hierin noch bekräftigt, wenn wir darauf übergehen, dieselben Verhältnisse in den einzelnen Altersklassen zu untersuchen. Ein Blick auf die Procentualwerthe, bez. auf die aus denselben berechneten Intensitätsziffern belehrt uns, dass diese Werthe stets nahe um das Mittel, also nur innerhalb der Fehlerlatitude, schwanken.

An Hautkrankheiten wurden nämlich behandelt:

Im Alter von	Insgesammt.	Hiervon geimpft.	= in Procenten.	Impfproc. der indiff. Krankheiten.	Letztere = 100 (relat. Intensität).
0— 1 Jahr.....	2	2	*	*	*
1— 5 Jahren.....	20	18	*	*	*
5—20 "	304	271	89.1	88.5	101
20—30 "	312	271	86.9	87.1	100
30—40 "	152	121	79.6	86.1	92
40—60 "	187	143	76.5	79.0	97
über 60 "	70	40	*	63.1	*
Alter unbekannt...	6	3	*	*	*
Summe der Uebereinjährigen	1051	867	82.5	82.9	100

Es ergibt sich demnach gar kein Anhaltspunkt für die Wahrscheinlichkeit einer Ueberimpfung.

Es darf aber nicht übersehen werden, dass unsere Spitalsbeobachtungen nur Erwachsene umfassen; bei Kindern dürfte es anders bestellt sein. Wir haben zu diesem Behufe den reichen Aufzeichnungen der Ambulanz des Budapester Stephanie-Kinderspitals und namentlich der daselbst vorgekommenen Hautkrankheiten besondere Aufmerksamkeit gewidmet. Auf Tabelle No. III finden sich dieselben nach nicht weniger als *achtundzwanzig* Titeln specificirt. Unter diesen achtundzwanzig Krankheitsformen sind im Ganzen 478 übereinjährige Kinder behandelt worden. Von denselben waren 376 (=78.7%) geimpft, 96 ungeimpft, 1 geblattert und 2 zweifelhaft. Von *sämmtlichen* im Kinderspitale behandelten übereinjährigen 4311 Kindern (72 Blattern abgerechnet) waren 3059 geimpft, =70.9%, und von den an indifferenten Krankheiten behandelten 3104 (übereinjährigen) Kindern 2160, d. i. nur 69.9%. Da nun bei den Hautkrankheiten die Quote der Geimpften 78.7% betrug, so ist diese geringe Steigerung von 13% wirklich auf Rechnung der Impfung zu schreiben. Im allerschlimmsten Falle, also, wenn man nämlich diesen schwachen Ausschlag nicht als Fehlerschwankung, sondern als Folge der Impfung betrachtet, wäre dies ungefähr der Preis, der für den Schutz der Impfung gezahlt werden muss. Bedenkt man jedoch, wie unvergleichlich mehr Kinder durch die Impfung der Blatternerkrankung entzogen werden, zieht man ferner in Betracht, dass diese Hauterkrankungen fast nie lethal verlaufen, während anderseits die Blattern bei den Nichtgeimpften zu den perniciösesten Krankheiten gehören: so muss man gestehen, dass der Schutz vor Blatternerkrankung wahrlich sehr billig erkauft wird.

[1] Nämlich bei Uebereinjährigen; aber unter 5629 im ersten Lebensjahre Verstorbenen fanden sich ebenfalls nur 19 an Hautkrankheiten Verstorbene und hierunter *Keiner* geimpft.

Eine Zusammenstellung für die einzelnen Altersklassen der in der Ambulanz des Kinderspitals (1874) behandelten Kinder ergibt Folgendes:

An Hautkrankheiten wurden behandelt:

Im Alter von	Insgesammt.	Hiervon geimpft.	In Procent.	Impfproc. d. indiff. Krankheiten.	Letztere =100 (relat. Intensität.
0— 1 Jahr.....	138	22	15.9	12.6	126
1— 3 Jahren.....	159	93	58.5	54.0	108
3— 7 "	172	148	86.0	75.7	114
7—14 "	129	118	91.5	91.8	100
über 14 Jahren.....	18	17	*	90.0	*
Summe der Uebereinjährigen.....	478	376	78.5	69.9	113

Im ersten Lebensjahre sind also die durch die Impfung hervorgerufenen Hautkrankheiten zahlreicher: unter den behandelten 138 Kindern waren 32 geimpft, während es nach dem Durchschnitt der indifferenten Krankheiten deren nur 17 hätte geben sollen. Wie man aber sieht, macht sich die Wirkung der Impfung nur bis zum siebenten Lebensjahre geltend: von da ab zeigt die relative Intensität der Hautkrankheiten 100% des Normals, d. h. ist mit demselben gleich, lässt also mehr keinen Einfluss der Impfung erkennen.

Wir haben auch für die Hautkrankheiten die Ergebnisse der Klinik des Kinderspitals ausgezogen: es wurden daselbst im Ganzen 33 Kinder behandelt, hierunter 32 über ein Jahr alt; von diesen 32 waren 26 geimpft, 6 ungeimpft, eines zweifelhaft. Die Geimpften betrugen also 84% der Behandelten, während der Durchschnitt aller Fälle 83% betrug: hier hätte sich also gar kein Ausschlag zu Ungunsten der Geimpften ergeben. (Gestorben ist nur ein ungeimpftes Kind, und zwar an Pyämie nach Erysipelas.)

Wir können demnach das Ergebniss unserer Untersuchungen in Folgendem zusammenfassen:

Eine genügende Anzahl von Fällen stand für Syphilis, Lungentuberculose, Croup, Diphtheritis, Hautkrankheiten und Scharlach zur Verfügung. Das Hauptbesorgniss nun, als ob *Syphilis* oder *Lungentuberculose* durch die Impfung verbreitet würden, hat sich im Bereiche unserer Beobachtungen *als vollkommen unbegründet erwiesen*. Für *Scharlach* sind weitere Beobachtungen abzuwarten. Bezüglich der *Hautkrankheiten* lässt sich über eine grössere Mortalität der Geimpften nichts aussagen, weil diese Krankheiten fast nie zum Tode führen; bezüglich der Morbidität kann aus den über 20,000 Fällen umfassenden Beobachtungen von neunzehn Spitälern ausgesagt werden, *dass es absolut unrichtig ist, als ob erwachsene Geimpfte den Hautkrankheiten stärker als Ungeimpfte ausgesetzt wären;* wohl aber ergibt sich eine geringe Steigerung der *Erkrankungswahrscheinlichkeit* für geimpfte *Kinder* bis etwa zum siebenten Jahre.

Von den nur mit einer geringen Anzahl von Fällen vertretenen übrigen Krankheiten ist für *Rothlauf* und *Rachitis* keine Steigerung der Erkrankungs-; beziehentlich der Sterbenswahrscheinlichkeit bemerkbar gewesen, während beim *Typhus* die Morbiditätsbeobachtungen ebenfalls zu einem negativen Resultat, die weniger verlässlichen Mortalitätszahlen jedoch zu einer geringen Steigerung der Sterbenswahrscheinlichkeit der Geimpften führten. Angesichts dieses Widerspruches lässt sich über diese Krankheit vorläufig kein Urtheil fällen, was übrigens in Folge der Geringfügigkeit der vorliegenden Fälle allgemein für alle letztgenannte Krankheiten gilt, weshalb für dieselben auch der Hinzutritt weiterer Beobachtungen abzuwarten ist.

5. BILANZ DER SCHUTZPOCKENIMPFUNG.

Als einzige Belastung der Impfpraxis ergab sich demnach, dass bei geimpften kleinen Kindern die Wahrscheinlichkeit einer Hauterkrankung um etwa 13% grösser ist, als bei ungeimpften. Dies sind aber Erkrankungen, welche nur äusserst selten zum Tode führen. So ergaben sich z. B. in Budapest, unter 75,318 in den Jahren 1876–'81 Verstorbenen, im Ganzen nur 288 an Hautkrankheiten Verstorbene, darunter 153 Kinder bis zu fünf Jahren; desgleichen in den neunzehn Spitälern unter 223 behandelten einjährigen Kindern nur 2 Todesfälle; unter 20,351 behandelten übereinjährigen Personen nur 9 (!) Todesfälle.

Dem gegenüber ist für die Nichtgeimpften die Gefahr, an Blattern zu erkranken, eine dreifach grössere (also=300%), als für die Geimpften; für die ungeimpften Blatternkranken ist aber die Gefahr, an Blattern zu sterben, eine *sechsfach* grössere (=600%), als für die geimpften Blatternkranken. Unter solchen Verhältnissen brauchte man auf eine ziffernmässige Bilanz des Werthes der Schutzpockenimpfung eigentlich gar nicht einzugehen. Es ist zwar die Impfoperation nicht absolut gefahrlos: aber welche andere Operation wäre es denn? Wer würde eine lebenrettende Operation deshalb verdammen, weil sie nicht ohne alle Gefahr ist? Und wenn dem schon so wäre, dass wir die Segnungen der Kuhpockenimpfung nicht ganz umsonst geniessen dürften, sondern dieselben mit einer geringen Steigerung der Hautkrankheiten bezahlen müssten: wie unvergleichlich werthvoller ist dieser Nutzen als der hiefür gezahlte geringe Preis?

Gar nicht zu sprechen davon, dass vor Einführung der Impfung jeder Mensch, ohne Ausnahme, an Pocken erkrankte; gar nicht zu rechnen jenes ungeheure Kapital an Lebenskraft und Wohlstand, welches hierdurch consumirt wurde, wollen wir bloss die gewonnenen Menschenleben in die Bilanz einstellen. Angenommen, dass im vorigen Jahrhundert, vor Entdeckung der Schutzpocke, die Mortalität an Pocken wirklich nur 8% der Gesammt-Mortalität betragen haben, so heisst dies, dass bei einer Mortalität von 3%, unter je 100,000 Lebenden jährlich 3000 und hievon 240 an Pocken starben. Da die Gefahr für die Nichtgeimpften eine sechsfach (ja in einzelnen Altersklassen eine fünfzehnfach) grössere ist, so würden bei allgemeiner Einführung der Impfung von je 100,000 Lebenden *höchstens* 40 an Blattern sterben, demnach, schon auf Grund einer so oberflächlichen Aufstellung, jährlich zum Mindesten 200 Menschenleben (also in den Nordamerikanischen Freistaaten z. B. jährlich 100,000 Menschenleben) erspart werden. Was könnte es nun dem gegenüber bedeuten, wenn bei 12,000 Kindern, die sich unter den obigen 100,000 Menschen finden mögen, jene geringe Quote derselben, welche Hautkrankheiten bekommt, um 13% vergrössert würde? — zumal wenn man bedenkt, dass diese Krankheiten nur in den seltensten Fällen zum Tode führen.

Man versuche einmal eine praktische Anwendung dieser Bilanz. In Budapest z. B. starben in den erwähnten sechs Jahren 153 Kinder (bis mit 5 Jahren) an Hautkrankheiten. Nach den in Tabelle No. II enthaltenen Angaben über den Procentsatz der Geimpften bei Hautkrankheiten (im Alter von 0—5 Jahren), dürften hierunter 30 geimpft gewesen sein. Angenommen, dass die Sterbewahrscheinlichkeit dieselbe Steigerung von 13% aufwiese wie die Erkrankungswahrscheinlichkeit, würden aus obigen dreissig Fällen vier zu Lasten der Impfung zu schreiben sein, also für je ein Jahr $\frac{4}{5}$ Fälle nach etwa 350,000 Einwohnern, demnach für 100,000 Einwohner und Jahr etwa $\frac{1}{5}$. In einer Stadt von einer Million Einwohner würden also, in Folge der Impfung, schlimmstenfalls *zwei* Kinder mehr an Hautkrankheiten sterben, hingegen zwei Tausend vor den Blattern gerettet werden. Diese Bilanz ergibt noch immer einen Gewinn von 1998 Menschenleben! Einem solchen Vorsprung gegenüber hat es dann nichts zu bedeuten, wenn man selbst jene Krankheiten, für welche bloss eine vage Vermuthung der Ueberimpfbarkeit vorliegt, auch noch in das Passivum der Schutzpockenbilanz einstellte: man mag so strenge rechnen wie man will, die Bilanz

der Kuhpockenimpfung wird doch noch immer viel hundertfach grösseren Nutzen als Schaden aufweisen !

SCHLUSS.

Fasst man nun die Gesammtheit der durch die neue Methode gewonnenen Resultate zusammen, so muss man zugeben, dass dieselben nicht nur eine sehr nachdrückliche Widerlegung der gegen den Impfschutz geltend gemachten Zweifel enthalten, sondern den Werth und die Macht dieses Schutzes ziffernmässig, und zwar in der überzeugendsten Weise darlegen. Wir haben die Vaccination als ein so wirksames Präservativmittel kennen gelernt, dass wohl auf dem ganzen Gebiete der Medicin keines, noch weniger aber irgend ein Heilmittel sich mit demselben messen dürfte. Die Impfung schützt in der imponirendsten Weise vor der Gefahr der Blatternerkrankung, die schon Erkrankten aber vor jener des Sterbens, und erstreckt sich dieser Schutz auf alle Altersklassen. Dabei ist die Impfung eine beinahe alsolut ungefährliche Operation; die Gefahren, denen man sich durch dieselbe aussetzt, sind nicht nur im Verhältniss zu der Grossartigkeit der durch dieselben gewonnenen Vortheile, sondern auch an und für sich minime, für alle Fälle unvergleichlich geringere als jene, welche irgend eine andere Operation begleiten, und können selbst diese Nachtheile der Impfung durch vorsichtige Wahl des Impfstoffes und gehörige Durchführung der Operation noch weiter herabgemindert werden. Wir waren in der Lage, die Bilanz der Impfung aufzustellen, und haben gefunden, dass wer sich nicht impfen lässt, seine Gesundheit, ja sein Leben der grössten Gefahr aussetzt, gleichsam der Absicht geziehen werden kann, sich diese scheussliche Krankheit geflissentlich zugezogen zu haben.

Die Entdeckung Jenner's hat nach den ingeniösen Generalisationen Pasteur's den Charakter der Unbegreiflichkeit, der Zusammenhanglosigkeit mit den übrigen pathologischen Erscheinungen abgestreift, und man darf nach Allem, was wir erfahren, dieselbe getrost als eine der wunderbarsten Errungenschaften glücklicher Empirie und tief eindringender Naturerforschung betrachten.

Ob nun in Folge dessen eine Regierung die Bevölkerung zur Impfung zwingen solle oder nicht, das ist eine ausserhalb des Rahmens unseser statistischen Aufgabe liegende, praktische Frage. Von der Statistik dürfte die Legislative in diesem Betrachte vor Allem wohl darüber Aufklärung zu fordern haben, ob es richtig sei, dass in einer Umgebung von Ungeimpften auch die Geimpften leichter erkranken? Eine bejahende Antwort, also eine Bestätigung der Behauptung, dass Ungeimpfte nicht nur sich schaden, sondern auch eine Gefahr für Andere sind, würde gewiss das stärkste Argument dafür bieten, um in diesem Streite zwischen dem Interesse des allgemeinen Wohles und der Freiheit des Individuums die Waagschale zu Gunsten des Ersteren sinken zu lassen. Die statistische Erhärtung dieses Beweises erscheint uns aber noch nicht genügend erbracht, weshalb auch zu erwarten wäre, dass die nächsten Untersuchungen der Vaccinationsstatistik sich in dieser Richtung bewegen dürften.

Man mag übrigens darüber, ab man ein Volk zu seinem Vortheile zwingen dürfe oder nicht, wie immer denken; über die wissenschaftliche Bedeutung der Vaccination, über die Realität und die Grossartigkeit des durch dieselbe gebotenen Schutzes scheint uns ein ernster Zweifel nicht mehr möglich. Die Entscheidung über diese Frage darf eben nicht mehr dem Experimente anheimgestellt sein, das ja gar nie jene unbekannten Bedingungen, jenes geheimnissvolle Agens wirken lassen kann, das eben dem Ausbruch der Epidemie vorausgeht; auch den *subjectiven* Ansichten des Arztes nicht, weil sich gegen Ansichten mit Gegenansichten, gegen subjective Ueberzeugung wieder mit subjectiver Ueberzeugung kämpfen lässt. Ein Abschluss dieser seit so langer Zeit controversen Frage steht nur zu erwarten, sobald man das Urtheil den Beobachtungen von Massenthatsachen, von Massenwirkungen überträgt, sobald man den Einfluss der Schutzpockenimpfung statistisch zu erfassen und die Wirkungen desselben *objectiv* zu fixiren versucht.

IMPFZUSTAND FÜR DIE, DER INOCULIRUNGSGEFAHR GEGENÜBER, INDIFFERENTEN KRANKHEITEN.

A. ERKRANKUNGEN IN NEUNZEHN SPITÄLERN, 1886.

Krankheiten.	0–1 Jahr.			1–5 Jahre.			5–20 Jahre.			20–30 Jahre.			30–40 Jahre.			40–60 Jahre.			Über 60 Jahre.			Alter unbekannt.			Summe der Überjährigen.			
	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Geimpft.	Ungeimpft.	Zweifelhaft.	Zusammen.
Totale	38	180	5	438	295	6	3,744	586	91	5,660	677	141	2,862	366	120	3,150	628	201	877	342	122	35	8	2	16,766	2,902	683	20,351
Hiervon indiffer. Krankheiten:																												
Nervenkrankheiten	1	4	...	13	7	...	132	17	7	158	25	9	168	26	12	182	29	18	63	17	5	1	...	1	717	121	52	890
Krankh. des Gefässystems	2	...	...	4	...	...	54	5	...	67	2	1	47	9	1	99	15	5	41	22	3	1	...	...	313	53	10	376
Lungenentzündung	1	6	1	18	10	...	99	9	2	158	26	3	117	14	7	126	35	6	32	10	6	1	...	...	551	104	24	679
Krankh. der Verdauungsorg.	14	37	...	31	16	...	243	35	5	420	41	12	293	35	8	367	73	19	98	37	6	4	2	...	1,456	239	50	1,745
" " Geschlechts- und Harnorgane	...	...	...	25	3	...	375	37	6	764	88	23	289	24	5	233	32	14	33	11	8	6	...	...	1,725	195	56	1,976
Altersschwäche	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	77	52	22	...	...	...	77	52	22	151
Gewaltsame Todesfälle	...	2	2	20	8	1	263	21	8	376	47	12	305	39	17	324	88	20	63	32	7	3	...	...	1,354	235	65	1,654
Summe der indiffer. Krankh.	18	49	3	111	44	1	1,166	124	28	1,943	229	60	1,219	147	50	1,331	272	82	407	181	57	16	2	1	6,193	999	279	7,471
Von je 100 der betreff. Altersgruppe sind	25.7	70.0	4.3	71.2	28.2	0.6	88.5	9.4	2.1	87.0	10.3	2.7	86.1	10.4	3.5	79.0	16.1	4.9	63.1	28.1	8.8	84.2	10.5	5.3	82.9	13.4	3.7	100.0

B. TODESFÄLLE IN ZEHN STÄDTEN, 1886.

Krankheiten.	0–1 Jahr. Geimpft.	Ungeimpft.	Zweifelhaft.	1–5 Jahre. Geimpft.	Ungeimpft.	Zweifelhaft.	5–20 Jahre. Geimpft.	Ungeimpft.	Zweifelhaft.	20–30 Jahre. Geimpft.	Ungeimpft.	Zweifelhaft.	30–40 Jahre. Geimpft.	Ungeimpft.	Zweifelhaft.	40–60 Jahre. Geimpft.	Ungeimpft.	Zweifelhaft.	Über 60 Jahre. Geimpft.	Ungeimpft.	Zweifelhaft.	Alter unbekannt. Geimpft.	Ungeimpft.	Zweifelhaft.	Summe der Überjährigen. Geimpft.	Ungeimpft.	Zweifelhaft.	Zusammen.
Totale	511	4,485	633	2,059	1,727	74	1,499	522	78	1,294	137	168	1,298	70	222	2,228	154	392	1,846	273	591	18	10	18	10,242	2,893	1,543	14,678
Hiervon indiffer. Krankheiten:																												
Nervenkrankheiten	81	742	123	286	132	10	166	14	7	63	3	21	103	8	22	239	19	65	185	25	72	7	6	2	1,049	207	199	1,455
Krankh. des Gefässystems	1	6	1	4	2	...	40	2	...	46	2	4	63	3	11	211	14	33	169	13	38	2	...	1	535	36	87	658
Lungenentzündung	57	409	92	264	209	13	74	10	2	54	4	2	80	3	8	170	13	24	126	16	29	...	...	1	768	255	79	1,102
Krankh. der Verdauungsorg.	186	1,296	298	276	231	5	69	7	9	51	1	5	66	1	7	245	10	42	145	24	49	...	1	...	852	275	117	1,244
" " Geschlechts- und Harnorgane	...	2	1	39	12	3	63	10	1	66	...	5	64	3	10	149	8	21	63	10	24	...	...	...	444	43	64	551
Altersschwäche	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	14	1	5	587	128	204	1	...	...	602	129	209	940
Gewaltsame Todesfälle	1	9	8	11	3	4	67	1	12	87	4	52	52	3	41	68	5	37	28	3	15	3	...	9	316	19	170	505
Summe der indiff. Krankh.	326	2,464	523	880	589	35	479	44	31	367	14	89	428	21	99	1,096	70	227	1,303	219	431	13	7	13	4,566	964	925	6,455
Von je 100 der betreff. Altersgruppe sind	9.8	74.4	15.8	58.5	39.2	2.3	86.5	7.9	5.6	78.1	3.0	18.9	78.1	3.8	18.1	78.7	5.0	16.3	66.7	11.2	22.1	39.4	21.2	39.4	70.8	14 9	14.3	100.0

IMPFZUSTAND FÜR DIE, DER INOCULIRUNGSGEFAHR GEGENÜBER, INDIFFERENTEN KRANKHEITEN AUS DEM BUDAPESTER STEPHANIE-KINDERSPITAL, VOM JAHRE 1874.

Krankheiten.	0-1 Jahr.				1-3 Jahre.				3-7 Jahre.				7-14 Jahre.				Über 14 Jahre.				Summe der Übereinjährigen.				
	Geimpft.	Ungeimpft.	Geblattert.	Zweifelhaft.	Geimpft.	Ungeimpft.	Geblattert.	Zweifelhaft.	Geimpft.	Ungeimpft.	Geblattert.	Zweifelhaft.	Geimpft.	Ungeimpft.	Geblattert.	Zweifelhaft.	Geimpft.	Ungeimpft.	Geblattert.	Zweifelhaft.	Geimpft.	Ungeimpft.	Geblattert.	Zweifelhaft.	Zusammen.
Meningitis	...	...	...	...	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	1	...	...	...	1
Wasserkopf	1	6	...	...	...	...	...	...	2	...	...	...	...	...	...	...	...	...	...	...	2	...	...	...	2
Krämpfe der Kinder	4	22	...	...	3	3	...	...	2	...	...	...	...	...	...	...	...	...	...	...	5	3	...	...	3
Herzfehler	...	...	...	...	...	1	...	...	...	1	...	...	4	...	...	...	...	...	...	...	4	2	...	...	6
Lungenentzündung	27	223	1	1	99	118	2	...	133	35	...	3	32	5	...	...	2	...	...	...	266	158	2	3	429
Bauchfellentzündung	...	...	...	...	...	...	...	...	3	...	...	...	3	...	...	...	...	...	...	...	6	...	...	...	6
Gedärmentzündung	10	42	...	...	24	29	...	...	3	2	...	...	...	...	...	...	...	...	...	...	27	31	...	...	58
Diarrhœa	88	366	...	...	161	147	1	1	49	10	1	...	9	1	...	...	1	...	...	...	220	158	2	1	381
Wassersucht	1	...	...	...	3	1	...	...	8	4	...	...	3	2	...	...	...	...	...	...	14	7	...	...	21
Verschiedene Krankheiten*	123	1,090	3	4	435	314	1	...	602	200	1	1	500	40	1	...	87	7	...	3	1,624	561	3	4	2,192
Zusammen	254	1,749	4	5	726	613	4	1	802	252	2	4	551	48	1	...	90	7	...	3	2,169	920	7	8	3,104
Von je 100 der betreff. Altersgruppe sind	12.6	86.9	0.2	0.3	54.0	45.6	0.3	0.1	75.6	23.8	0.2	0.4	91.8	8.0	0.2	...	90.0	7.0	...	3.0	69.9	29.6	0.2	0.3	100.0

* Siehe letzte Seite der Tabelle III.

ANHANG.

KRITISCHE ANALYSE NACHFOLGENDER ZWEIER IMPFGEGNERISCHER WERKE:

DR. W. REITZ,
Oberarzt am Elisabeth-Kinderhospital in St. Petersburg.
VERSUCH EINER KRITIK DER SCHUTZPOCKENIMPFUNG.
St. Petersburg.
Buchdruckerei der Academie. 1873. 81 S.

DR. ADOLF VOGT,
Ordentl. Professor der Hygiëne und der Sanitätsstatistik an der Hochschule in Bern.
FÜR UND WIDER DIE KUHPOCKENIMPFUNG UND DEN IMPFZWANG,
oder polemische, kritische und statistische Beiträge zur Pocken- und Impf-Frage, mit zahlreichen statistischen Tabellen.
Den schweizerischen Bundesbehörden gewidmet.
Bern. Dalp. 1879. 248 S.

I. ALLGEMEINE BEMERKUNGEN ÜBER ANTI-VACCINATORISCHE DEBATTIR-TAKTIK.

Wenn man den impfgegnerischen Ansichten nicht von Vornherein starren Unglauben, nicht etwa den Autoren Misstrauen entgegenbringt, wenn man im Allgemeinen geneigt ist, auch die den eigenen entgegenstehenden Ansichten als mögliche und berechtigte zu betrachten, wird man nach Durchsicht irgend einer der besseren impfgegnerischen Arbeiten, sich in nicht geringe Unsicherheit versetzt fühlen. Die allgemein gehaltenen Verdächtigungen gegen die Verlässlichkeit der impffreundlichen Statistik werden hiebei noch am wenigsten verfangen, zumal wenn man sich das Material verlässlicher Beobachter vor Augen hält; noch weniger natürlich, wenn man in der glücklichen Lage ist, selbst Beobachtungen angestellt zu haben, von der Lauterkeit des eigenen Vorganges also am besten überzeugt ist. Verwirrender wirkt schon die Mannigfaltigkeit der Beweisgänge, die verschiedenen, oft gekünstelten und dabei mit kaleidoskopischer Unruhe wechselnden Argumentationen. Um in diesem unruhigen Chaos der Beweisarten, in diesem Wirbel von Wahrheit, Irrthum, Täuschung und Selbsttäuschung die Ruhe des Urtheils zu wahren, ist es rathsam, sich alle Argumente, pro und contra, in ein System zu bringen, dieselben zuerst theoretisch auf ihre Beweiskraft zu prüfen, um dann, wenn im Streite Schläge und Gegenschläge rasch fallen, schon im Vorhinein über die Tragweite, den Werth oder Unwerth jedes derselben im Reinen zu sein.

Aber selbst derart gewappnet, konnte ich mich bei Durchsicht der impfgegnerischen Schriften von Vogt, Lorinser, Reitz u. A. eines beunruhigenden Gefühles nicht erwehren, wenn in langen Tabellenreihen Zahlen auf Zahlen, Thatsachen auf Thatsachen aufgefahren werden, die da alle an den Fundamenten der Theorie vom Impfschutze rütteln. Bei einer ehrlichen Polemik muss man trachten, sich in den Gedankengang des Gegners hineinzudenken. Hält man sich nun nicht für unfehlbar, und glaubt man nicht, dass einmal aufgestellte wissenschaftliche Ansichten unumstösslich seien, wie wollte man da seine Ueberzeugung nicht schwanken fühlen, wenn z. B. Direktor und Primarius eines grossen Krankenhauses, sich auf ihre eigenen Erfahrungen stützend und dieselben *citirend*, die Nutzlosigkeit der Impfung beweisen; wenn verdienstvolle Schriftsteller über Kinderkrankheiten uns versichern, dass ihre eigenen, sowie die Erfahrungen der enragirtesten Impffreunde eigentlich zu Gunsten des Impfunglaubens sprächen, und hierauf wirklich aus Impf-Autoritäten ganze, wörtlich citirte

und sogar mit Angabe der Seitenzahl belegte, Stellen anführen, aus denen hervorgeht, dass jene Autoritäten ihre eigenen Zahlen nicht verstanden haben, da aus diesen gerade das Gegentheil, nämlich die Nutzlosigkeit der Impfung folge; wenn man liest, dass in gutimpfenden Ländern und Armeen die Pocken stärker aufgetreten seien als in schlechtimpfenden; dass in England und Schottland, London und Wien u. s. w. nach Einführung des Impfzwanges die Pocken viel ärger gewüthet haben sollen als je zuvor; dass in Oesterreich die bestimpfenden Provinzen die grössten Epidemieen erdulden mussten, u. s. w. All dies wird aber nicht nur in die Luft hinein behauptet, sondern Schritt für Schritt mit Zahlen und Procenten bewiesen, die ja Jedermann nachzurechnen freisteht, die man dann auch nachrechnet, aber immer und wieder richtig die Negation des Impfschutzes in denselben findet. Unter solchen Umständen ist es wohl nicht zu verwundern, wenn die Einen, des statistischen Streites müde, in ihrem Gefühle aber dennoch des Impfschutzes sicher, lieber auf alle statistischen Beweise verzichten wollen, während Andere sich einer theilweisen Skepsis ergeben und sich sagen, es scheine doch Manches in der Schutzpockentheorie nicht ganz richtig zu sein. Ich muss wenigstens gestehen, dass mich die Lecture solcher impfgegnerischen Schriften stets in Unruhe versetzt hat, und dass die vielen, Keulenschlägen gleich wirkenden Argumentationen, namentlich aber die Menge der unwiderlegbar scheinenden Thatsachen, mich gleichsam zu betäuben pflegten. Wer sich im Vorhinein damit beruhigt, dass die gegnerischen Thatsachen gewiss unrichtig oder gefälscht seien, der bewahrt freilich leicht jene Ruhe und Sicherheit, die uns der Glaube in allen Lagen des Lebens bietet.

Um nun dieser, durch die Ziffermässigkeit der Beweise stets ermüdenden, Polemik gegenüber einen festen Standpunkt zu gewinnen, habe ich mir die Mühe auferlegt, einige der ernsteren Erscheinungen der deutschen impfgegnerischen Literatur bis auf ihre Elemente zu zerlegen und mich durch Vordringen bis auf die letzten Quellen von der Stichhältigkeit aller in denselben enthaltenen statistischen Angaben nach der einen oder der anderen Richtung zu überzeugen. Ich habe zu diesem Behufe — unter Beseitigung des sehr unverlässlichen, unsystematisch denkenden und zumeist doch nur copirenden Werkes von Prof. *Germann* — mir die Aufgabe gestellt, die zerstreuten Arbeiten von Direktor *Lorinser* (beziehentlich seines Primarius *Hermann*), von *Keller*, ferner die Werke von *Vogt* und *Reitz* einer möglichst gründlichen Analyse zu unterziehen.

Ein derartiges Unternehmen ist ein in vielen Beziehungen recht ermüdendes. Statistische Beweise aus den Quellen zu überprüfen, verursacht beinahe so viel Mühe, wie die ganze Arbeit noch einmal zu machen. Es kommt dazu, dass der Autor seine Informationen aus den ihm eben zur Verfügung stehenden Quellen schöpft, die Quellen eines Anderen aber wieder ganz andere sind. Reitz citirt z. B. auf Seite 24 seines Buches, in *einem* Beweise, also in einem Athemzuge, die Epidemie von Preussen 1865 und Unterfranken 1866, die Krankenaufnahme des Frankfurter Rochusspitals 1861–'69, des Stuttgarter Mathildenspitals von 1869–'70, des Brester Spitals 1870, des Münchener und Berliner Pockenspitals 1871, des Wiener allgemeinen Krankenhauses 1871. Für den Schreiber dieser Zeilen standen in ganz Budapest nicht einmal die nächstgelegenen Wiener Spitalsberichte zur Verfügung und mussten selbst diese erst von deren Direktionen erbeten werden; ich zweifle aber, ob es in ganz Europa auch nur eine einzige Fachbibliothek gäbe, wo die genannten Spitalsberichte beisammen zu finden wären. Die Ueberprüfung der Keller'schen Daten insbesondere erforderte eine gar langwierige Correspondenz mit einem Viertelhundert von Personen, und sogar Reisen nach Wien. Man wird es unter solchen Umständen verzeihlich finden, wenn einige unbedeutende Auslassungen dennoch übergangen werden mussten.

Die Kritik der Arbeiten Keller's, Hermann's und Lorinser's wurde nun in einem Vortrage in der ungarischen Gesellschaft für Hygiëne vor die Oeffentlichkeit gebracht, und ist in dem *Orvosi Hetilap*, sowie in der *Deutschen Vierteljahresschrift für Oeffentliche*

Gesundheitspflege (1887) veröffentlicht worden.[1] Die umfangreichere Ueberprüfung der Werke von *Vogt* und *Reitz* findet im vorliegenden Anhange statt.

Bevor ich aber an diese Analyse gehe, kann ich doch nicht umhin, dem Gesammteindruck, den das Studium *dieser* Schriften auf mich hervorgebracht hat, in dem Bekenntnisse Ausdruck zu geben, dass ich am Schlusse desselben mich, mit einiger Beschämung, gar nicht mehr so weit von dem — meinerseits gemiedenen — Pfade jener entschiedeneren Vorkämpfer des "Impfdogmas" sah, die impfgegnerischen Thatsachen und statistischen Angaben schon im Vorhinein mit Misstrauen zu begegnen pflegen und durchaus nicht geneigt sind, sich durch irgend eine Tabelle, oder selbst durch ein noch so gewissenhaft dareinschauendes Citat aus irgend einer Autorität imponiren zu lassen. Ich will mich trotzdem nicht zu dem intoleranten und im Impfstreite öfters wiederkehrenden Ausspruche hinreissen lassen, als ob alle impfgegnerische Statistik unverlässlich sei; ich muss aber leider zugeben, dass jene hervorragenderen Leistungen der impfgegnerischen Schule, denen ich mich nicht verdriessen liess, Schritt für Schritt bis auf die Quellen nachzugehen, sich im Ganzen genommen als unverlässlich und irreführend ergeben haben: Von hundert der ihrerseits gegen die Existenz des Impfschutzes vorgeführten statistischen Thatsachen, bleiben bei näherer Prüfung kaum zwei stehen, und auch diesen wohnt keine zwingende Beweiskraft inne. Da man aber nicht fordern kann, dass Jemand allen möglichen, theilweise aus Fanatismus herrührenden, unleugbar irrigen Behauptungen bis auf ihre Quellen nachgehe, ist es begreiflich, wenn schliesslich auch die angeblich entschiedensten Beweise vom Nichtschutze der Impfung Seitens der Imffreunde mit einem blossen Achselzucken aufgenommen werden.

Man lernt bei dem Studium der impfgegnerischen Literatur eine ganze Stufenleiter der unrichtigen Beweisführung kennen. Der sehr treffenden Bemerkung *Schopenhauer's* gedenkend, wie nützlich es eigentlich wäre, die Gedankenwege der Rechthaberei in ein System zu bringen, überdies aber auch in der Absicht, durch eine allgemeine Würdigung und Widerlegung der in der Schutzpocken-Statistik beobachteten falschen Folgerungen, den Leser der Wiederholungen zu entheben, die sonst bei Analyse der einzelnen concreten Fälle entstehen müssten, will ich hier einen kleinen stufenförmig geordneten Ueberblick der beobachteten Irrungen geben.

1) Am leichtesten wiegt und leichtest entschuldigt ist das einfache Versehen, der gewöhnliche Schreib- und Rechenfehler. Wer viel mit Zahlen zu thun gehabt, wird in dieser Beziehung nachsichtig sein. Niemand ist gegen die Schwäche der menschlichen Organe gefeit; es gibt keinen Statistiker — von Engel und Kussmaul angefangen, bis herab auf den Schreiber dieser Zeilen — dem man nicht unwillkürliche Irrungen nachweisen könnte. Ist ja selbst die Rechenmaschine vor diesen nicht sicher. Da überdies bei diesen Irrthümern das Hauptcriterium der Strafbarkeit, die böse Absicht, vollkommen fehlt, sollte man in dieser Beziehung nie zu strenge ins Gericht gehen. *Hanc damus, petimusque vicissim veniam.* Schliesslich ist ja der Werth einer statistischen Arbeit doch nicht ausschliesslich in der Correctheit der Rechnungsoperationen, sondern auch in dem Gedankengange, der Originalität der Auffassung, der mehr oder minder scharfsinnigen Beobachtung, der Logik des Folgerns zu suchen. Dass freilich eine statistische Arbeit, die von Rechenfehlern wimmelt, unbrauchbar ist, dass, wie in anderen Wissenschaften, und selbst in der Kunst, so auch in der Statistik, die technische Fertigkeit mit der geistigen Conception Hand in Hand gehen muss, bedarf keines Beweises.

2) Eine merkliche Fehlerquelle bietet die ungenügende Beachtung der absoluten Grössenverhältnisse. Die Statistik muss alle Verhältnisse auf gleichwerthige Nenner

[1] Auch als Separatdruck ("Die Wiener impfgegnerische Schule und die Statistik") bei Vieweg, 1887, erschienen.

bringen: sie rechnet also eigentlich stets nur in Verhältnisszahlen, in Procenten. In dieser Form gleicht aber ein Verhältniss dem andern, und Niemand erkennt an einer Procentzahl jenen grossen Unterschied, der in dem verschiedenen Gewichte der zu Grunde liegenden absoluten Beobachtungen liegt. Wenn in einem Lande von 100,000 Ungeimpften 30,000, und in einem kleinen Spitale von zehn Kranken drei starben, so gibt dies gleichmässig 30%. Aber selbst Zahlen müssen nicht nur gezählt, sondern auch gewogen sein. Zum Mindesten sollte es nach abwärts eine Grenze geben, unterhalb welcher Verhältnissberechnungen als unstatthaft betrachtet würden. Schon der gewöhnliche Sprachgebrauch, der wohl den Begriff des Procent (pro hundert), aber nicht jenen des *Prodecim* (= pro zehn) geschaffen, könnte es nahe legen, dass Procentwerthe nur zur Noth aus weniger als hundert Fälle zählenden Beobachtungen gezogen werden sollten. Wir werden aber in unseren Anführungen Beispiele dafür finden, dass man nicht nur aus zehn, sondern selbst aus sieben, ja sogar aus drei, sage *drei* Fällen, Procente berechnete; ja man findet in der Spitalsstatistik auch Fälle, wo, wenn *ein* Kranker aufgenommen wurde und dieser starb, die Lethalität mit 100% berechnet wurde![1]

3) Die Frage des Durchschnittes und des Maasses ist eine in der Statistik vielumstrittene. Man macht mit Recht Unterschied zwischen wirklichen Mittelwerthen, die die Sache so darstellen, wie sie in der überwiegenden Mehrzahl der Beobachtungen zu erscheinen pflegt, und zwischen rein numerischen Durchschnitten, die oft nur ein Abstractum sind, denen in der Wirklichkeit gar nichts entspricht und die Allem eher ähneln, als der Sache, die sie bezeichnen sollen.[2] Wie immer man aber über die Beschaffenheit der richtigen Mittelwerthe denken möge, so viel ist sicher, dass Niemand das durchschnittliche Maass in den Extremen, in den Ausnahmsfällen, suchen dürfte. In der Vaccinationsstatistik werden wir aber der Auswahl solcher Ausnahmsmaasse wiederholt begegnen. Hand in Hand hiermit geht dann auch der Fehler der maasslosen Vergrösserung. Um nämlich diese Ausnahmsfälle zu einer scheinbaren Geltung zu bringen, muss man sie sozusagen unter ein Vergrösserungsglas setzen: wo im Ganzen ein paar Fälle beobachtet wurden, bringt man ein Objectiv mit zehntausendfacher Vergrösserung an und sagt dann: auf eine *Million* entfielen u. s. f. Sieht man dann in den Quellen die absoluten Zahlen nach, so findet man, dass in dem zum Maasse erwählten kleinen Gebiets- oder Zeitabschnitt in Wirklichkeit vielleicht nur *ein* Fall vorkam. Wir werden z. B. in der schottischen Pockenstatistik Beispiele finden, wie die daselbst zum Maasse gewählte Pockensterblichkeit einer Altersklasse oft nur ein Zehntel Person per Jahr repräsentirt, d. h., dass in einem ganzen Decennium nur ein Mensch starb, also jährlich ein Zehntel! Und wie wenn dieser Eine zufällig genesen wäre? In diesem Falle ergäbe die Vergleichung mit dem Maasse Null lauter unendliche Grössen!

Neben diesen, sich halb unbewusst geltend machenden Fehlerquellen, gibt es aber noch bewusste und mit Absicht hervorgegangene. Diese unterliegen freilich einem strengeren Urtheil. Hierher rechnen wir

4) wenn bei Vergleichung weit auseinander liegender Zeiten nur die absolute Anzahl

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[1] Schon in meinem Volkszählungswerke über die Stadt Pest i. J. 1870 habe ich Tausende von Percentualwerthen unberechnet gelassen und statt der Ziffern ein blosses Zeichen (△) in die Percentrubrik gestellt, wenn die Anzahl der beobachteten Fälle weniger als fünfzig betrug, während ich selbst noch dort, wo die Anzahl der Fälle über fünfzig, aber unter hundert betrug, das Percentverhältniss zwar berechnete, aber durch Hinzufügung eines Sternchens warnte, aus solchen Percenten Schlüsse zu ziehen.

[2] Wenn es z. B. in einer Stadt 1000 einstöckige und 1000 vierstöckige Häuser, aber gar keine zwei- und dreistöckigen gibt, so würde die Rechnung, dass daselbst die Häuser im Durchschnitt 2½-stöckig wären, den wirklichen Verhältnissen nicht im Geringsten entsprechen.

der Pockentodesfälle angegeben wird, deren Zunahme in der Gegenwart also oft als sehr bedeutend erscheint, hierbei aber unterlassen wird, auf die im Laufe dieser Zeit erfolgte Zunahme der Bevölkerung hinzuweisen. Diese Unterlassung fällt namentlich bei Grossstädten schwer ins Gewicht, weil bei diesen sich die Bevölkerung von der vor- bis auf die nach-vaccinatorische Zeit oft verzehnfacht, verzwanzigfacht hat.

5) Hier haben wir auch eines der verbreitetsten und der gefährlichsten Vergehen gegen den Codex der statistischen Beweisführung, nämlich der *tendentiösen Auswahl*, zu gedenken.

Wenn man aus einer Reihe von Beobachtungen Schlüsse ziehen will, so muss man die Einzelbeobachtungen gruppiren, classificiren. Je nach dem Zwecke der Arbeit wird auch das Princip der Classification ein anderes sein. Handelt es sich um die historische Untersuchung, ob die Pocken seit Einführung der Impfung schwächer geworden, so wird man die Beobachtungsperioden in Epochen vor und nach Einführung der Impfung oder des Impfzwanges theilen; handelt es sich um die geographische Untersuchung dieses Einflusses, wird man die der Beobachtung unterliegenden Staaten in eifrig und lässig impfende, oder in Staaten mit und ohne Impfzwang eintheilen u. s. f. Unbedingt unerlaubt ist es aber, jenes Factum im Vorhinein zum Princip der Eintheilung zu machen, das nachher den Gegenstand der Untersuchung bilden soll, das also erst bei Beendigung derselben an's Tageslicht treten kann. Die ganze statistische Aufstellung verliert hierdurch den Character der Untersuchung. Legt man das zu erforschende Resultat insgeheim hinein, so wird man es freilich wieder als Ergebniss herausziehen, wie auch der Alchymist sein vorher in den Tiegel geschmuggeltes Gold wieder aus demselben herausschmilzt; die ganze Procedur wird aber hierdurch zu einer irreführenden Farce. Wenn der Zieler an der Scheibe die Treffsicherheit der Schützen derart constatirte, dass er dem A. nur alle Nullen, dem B. aber nur alle Kernschüsse anrechnet, würde Jedermann im Klaren sein, was er hierüber zu denken habe. Was für ein Unterschied ist aber zwischen diesen Fällen und dem Statistiker, der in seiner Stube die hundertjährige Pockentabelle studirt, sich diejenigen Jahre oder Jahrzehnte heraussucht, die für ihn die Treffer bedeuten, die Nieten einfach ignorirt, und dann vor das Publikum tritt und mit vielsagender Miene die ausgesuchten Facta vorzeigt, und hiemit im Zuhörer, der nicht auf die Quellen eingehen kann, die Täuschung hervorruft, als ob nun wirklich durch Anführung aller vorhandenen Thatsachen, oder schon durch einige auf gut Glück herausgegriffene, die Richtigkeit der These statistisch bewiesen worden sei. Diese künstliche, tendentiöse Auswahl der passenden Instanzen, verbunden mit Verläugnung der nicht in den Kram passenden, ist es eben, welche die statistische Beweisart so sehr in Verruf gebracht hat, welche dahin führte, dass während in den experimentirenden Wissenschaften die quantitative Methode einem Kleinod gleich hoch gehalten wird, sie bei den beobachtenden Fachwissenschaften in den grössten Misscredit gekommen. Dieser tendentiösen Auswahl hat man es zu danken, wenn jeder junge Politiker die Statistik als "feile Dirne" bezeichnen zu dürfen glaubt, da, in seiner oberflächlichen Bekanntschaft mit derselben, er die Erfahrung gemacht, dass man durch dieselbe Alles, und auch das Gegentheil bewiesen haben könne. Der Vorwurf trifft aber nicht die Statistik, sondern Jene, die da solchen Missbrauch mit ihr treiben. Man versuche doch z. B., mit Hülfe eines reichen, aber nicht tendentiös ausgesuchten statistischen Materials zu beweisen, dass die Pocken seit Einführung der Impfung zugenommen hätten, und man wird sogleich sehen, wie unbeugsam sich die Statistik einer solchen Zumuthung entgegensetzt.

Die tendentiöse Auswahl nimmt in der impfgegnerischen Statistik mannigfache Formen an. Einmal sucht man sich aus der prävaccinatorischen Zeit die besten Jahre aus, während man aus der postvaccinatorischen die ärgsten Epidemiejahre auswählt, wobei man namentlich bemerken wird, dass solche Statistiker, wenn sie schon nicht

so naiv sind, bloss die Pandemiejahre 1870—72 zu wählen, doch mit Vorliebe solche kleine Perioden hervorheben werden, in denen diese ungünstigste Epoche unseres Jahrhunderts mitenthalten ist. Andere mischen wieder Facta notorisch lässig impfender Staaten unter ihre Beweise, um solcherart zu beweisen, wie gross die Sterblichkeit auch gegenwärtig sei, wobei sie der so nahe liegenden Versuchung, zu untersuchen, wie diese Staaten eigentlich vor Einführung der Impfung bestellt gewesen (historischer Beweis), ferner, wie sich die Pockenstatistik bei Scheidung der gut von den schlecht impfenden Staaten gestalte (geographischer Beweis), standhaft widerstehen. Andere berufen sich auf die Sterblichkeit von Grossstädten, vergleichen sie auch etwa mit der des flachen Landes in prävaccinatorischer Zeit, wobei sie verschweigen, dass in Grossstädten die Pocken stets üppigsten Boden finden, und wobei sie der Versuchung, diese selben Städte in der prä- und postvaccinatorischen Epoche zu untersuchen, merkwürdigerweise ebenfalls widerstehen.

6) Noch eine Stufe tiefer rangiren die Statistiken, die ihr Ansehen vor dem Leser aus der Verstümmelung der Gegner fristen. Man citirt die Aussagen eines Anhängers der Schutzimpfung falsch und dann beweist man, wie falsch derselbe geschlossen, zeiht ihn eventuell gleich der absichtlichen Fälschung von Thatsachen. Wenn bei solchen verstümmelten Citaten auch Seitenzahlen angegeben sind, muss dies den Effect nur erhöhen: kann es doch Jeder selbst nachlesen, dass dem so sei! Wie wenig aber selbst auf Seitenzahlen zu geben ist, kann man schon aus der in der Einleitung nachgewiesenen Verstümmelung entnehmen, die sich Prof. Germann zu Schulden kommen liess, der aus dem englischen Blaubuche, angeblich gegen den Nutzen der Impfung lautende Aussprüche der Enquête mit Angabe der Paragraphennummer citirt, während man beim Nachschlagen im Blaubuche mit Erstaunen sieht, wie die betreffenden Aussagen oft ganz überschwänglich das Lob der Impfung predigen. Eine ganz eigene Art dieser Verstümmelungen ist die der Selbstverstümmelung; so mag der Leser z. B. aus meinem Eingangs erwähnten Vortrage ersehen, dass es selbst an solchen Fällen nicht mangelt, wo Impfgegner sich auf die von ihnen selbst erlebten und publicirten Erfahrungen berufen, während sich beim Nachschlagen in dieser Quelle dann findet, dass dieselbe eigentlich zu Gunsten der Impfung spricht.

7) Am allerschwersten fallen aber selbstverständlich Entstellungen des statistischen Urmaterials in's Gewicht. Statistischen Originalbeobachtungen gegenüber gilt natürlich die Voraussetzung der scrupulösesten Wahrheitsliebe. Aus den fertigen Tabellen mag man dann immerhin nach Geschmack und Belieben tendentiös gruppiren, vergrössern, verkleinern, Unliebsames verläugnen etc. Das sind schliesslich doch offenkundige Schritte, denen man zur Noth folgen, die man also beurtheilen kann. Soweit darf es aber nie kommen, dass ein Statistiker, sei es auch im Dienste des edelsten Princips, die Thatsachen selbst, die Originalaufzeichnungen seiner Urtabellen, verändere, im Sinne seiner Ansicht verfälsche. Hört einmal das Vertrauen in diese fundamentale Treue des Statistikers auf, dann ist auch das Ende aller statistischen Untersuchung gekommen, dann wird Niemand dem Anderen, Jeder nur sich trauen — ein chaotischer Zustand des Krieges Aller gegen Alle, aus dem uns nur die Wiederaufrichtung der persönlichen Verlässlichkeit der Statistiker erlösen kann.

Wir haben leider auf dem Gebiete der Pockenstatistik auch solchem Missbrauch begegnen müssen. Zur Ehre der Statistiker aber, oder vielmehr — da die Ehre, kein unschickliches Gewerbe zu betreiben, doch nicht so gross ist — zum Glück der Statistik sei es gesagt, dass solche äusserste Fälle des Fanatismus nur vereinzelt und gemieden in der Geschichte der Statistik dastehen.

II. ANALYSE VON REITZ' "VERSUCH EINER KRITIK DER SCHUTZPOCKEN-IMPFUNG".

Der Verfasser, Direktor des Elisabeth-Kinderspitals in St. Petersburg und Verfasser eines geschätzten Handbuches der Kinderkrankheiten, hat sowohl in obiger Schrift, sowie auf dem Wiener internationalen medicinischen Congresse, unter den entschiedensten Gegnern der Vaccination Platz genommen. Die uns beschäftigende Schrift hat schon mit Rücksicht auf die Stellung und die literarischen Leistungen des Verfassers nicht verfehlt, Aufmerksamkeit zu erwecken. Dieselbe behandelt ein reiches Material in ansprechender Form und enthält auch Originalbeobachtungen. Alles concludirt aber dahin, dass die Impfung nichts genützt, eher geschadet habe; die Hoffnung auf Schutz sei eine imaginäre, ja es scheine, dass in unserer impfenden Epoche die Sterblichkeit an Blattern noch grösser wurde, als in der Zeit vor Jenner; auf Grund klinischer Beobachtungen lasse sich behaupten, dass an wirklichen Blattern mehr Geimpfte als Ungeimpfte stürben; überdies aber würden durch die Impfung Syphilis und Rothlauf inoculirt, ja die Impfung sei schon an sich schädlich, weil direkt durch dieselbe hervorgerufene Hautkrankheiten sehr häufig zum Tode führten.[1]

Was die zuletzt genannte Klage auf Impfschäden betrifft, entfällt für Jene, welche die Möglichkeit derselben zugeben, die Nothwendigkeit, diesen Theil des Impfstreites meritorisch zu behandeln; es kann sich nur um die Feststellung des Maasses solcher Impfschäden und deren Vergleichung mit dem Impfnutzen handeln. Nachdem wir die Aufstellung einer solchen Bilanz im zehnten Kapitel, und zwar mit günstigem Erfolge, unternommen, brauchen wir auf diesen Theil der Reitz'schen Ausführungen nicht nochmals einzugehen.

Reitz formulirt die Thesen der Impfvertheidigung in folgenden drei Punkten:

1) Die Kuhpockenimpfung gewähre einen vollkommen sicheren Schutz gegen die Blattern;

2) Die Impfung sei ganz ungefährlich und könne niemals die Ursache des Todes sein;

3) Die Impfung könne nie als Ursache irgend welcher später auftretenden Erkrankung angesehen werden.

Gegen diese Formulirung wenden sich denn auch seine Angriffe. Die Formulirung ist aber eben keine richtige: sie entspricht durchaus nicht dem gegenwärtigen, sondern einem schon seit längerer Zeit verlassenen Standpunkte der Impftheorie. Deshalb, weil man früher zu viel von der Impfung erwartet, deshalb, weil diese Erwartungen durch die Erfahrungen und — wie man billigerweise zugestehen sollte — auch durch die Angriffe der Impfgegner reducirt wurden, sind dieselben doch nicht in's Gegentheil verkehrt. Die richtige Formulirung dieser Thesen der Impfvertheidigung würde lauten:

1) Die Kuhpocken-Impfung gewährt Schutz gegen Blattern. Dieser Schutz ist manchmal ein lebenslänglicher, schwächt sich aber im Laufe der Zeit oft ab. Ein absolutes Maass dieser Dauer der Schutzfrist lässt sich in Folge dessen nicht angeben: im Durchschnitte dürfte dieselbe mit sieben Jahren angenommen werden. Der Vaccina-

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[1] Dass der Verfasser seine impfgegnerischen Ansichten auch seither nicht geändert, ersieht man aus seinen "Grundzüge der Physiologie, Pathologie und Therapie des Kindesalters" (Berlin, 1883), woselbst (S. 175) die Vaccination mit dem Tragen von Amuletten auf eine Stufe gestellt wird.

tionsschutz ist auch kein absoluter; aber Fälle, dass Geimpfte in den auf die Impfung folgenden nächsten Jahren von Blattern befallen würden, sind selten; noch seltener sind Fälle, dass Geimpfte während der Schutzzeit an Blattern stürben.

2) Die Impfung ist eine Operation, die, wie jede andere, Schädigungen mit sich führen kann; es gehört jedoch zu den seltensten Ausnahmen, dass die mit gewöhnlicher Vorsorge durchgeführte Impfung zu ernster Erkrankung oder gar zum Tode führte.

3) Desgleichen wird eine mit der gewöhnlichen Vorsorge durchgeführte Impfung nur in den allerseltensten Fällen als die Ursache einer später auftretenden Krankheit angesehen werden dürfen.

Reitz wendet sich in erster Reihe gegen die Behauptung, dass die Impfung *absoluten* Schutz gegen die Blatternkrankheit biete, und führt eine lange Reihe von Thatsachen auf, woraus ersichtlich ist, dass unter den an Blattern Erkrankten die Anzahl der Geimpften stets häufiger werde. Der obigen Formulirung gegenüber ist der ganze, von Seite 6—14 reichende Angriff, welcher beweist, dass auch Geimpfte an Blattern erkranken oder sterben können, gegenstandslos. Die Behauptung aber, dass solche Daten den Beweis erbrächten, wie die Impfung vor Pocken *nicht* schütze, ist einfach irrig; diese Behauptung ist ganz unbewiesen. Dass selbst Geimpfte an Blattern erkranken, ist durchaus nicht identisch mit dem von Reitz aufgestellten Satze, dass Geimpfte ebenso häufig erkranken, wie Ungeimpfte.

Seite 17 bekämpft Reitz die im englischen Blaubuche und bei Kussmaul angeführten Daten über die geringere Lethalität der Geimpften. Das Blaubuch beweist nämlich, dass, während von ungeimpften Kranken 16—51 %, von geimpften bloss 0—12½ % verstarben. Einige Contraste sind geradezu blendend; so z. B. starben von je hundert

		Ungeimpften	Geimpften
im Wiener Krankenhause 1834—1856		30.00	5.00
" " " 1861—1862	Männer	17.24	2.40
	Frauen	19.60	5.32
" österreichischen Literale 1835		15.20	4.80

Die Art, wie sich Reitz diesen Beweismitteln gegenüber verhält, ist eine sehr auffällige, leider durchaus nicht objective. Er ersieht aus dieser Tabelle nicht etwa, dass von den Ungeimpften *vier bis achtmal mehr starben*, sondern nur "dass die Zahl der erkrankten und gestorbenen Geimpften und Ungeimpften gar nicht angegeben ist, so dass man nicht wissen kann, wie viele Fälle des Erkrankens Ungeimpfter zur Bestimmung des Sterblichkeitsprocentes gedient haben." (!) Um dann zu zeigen, wie werthlos (!) solche Art Statistik sei und wie leicht auf solche Weise falsche Anschauungen verbreitet werden können, wird das Frankfurter Rochusspital angeführt. In diesem Beispiele wird dann eigentlich nichts bewiesen, als dass, wenn von drei Kranken einer stürbe, dies procentuarisch zwar 33.3 % ausmacht, dass aber dieses Ergebniss ohne Bedeutung sei, weil es aus einer zu geringen Anzahl von Fällen gezogen wurde. Gewiss darf man aus drei Fällen nicht urtheilen. Aber die von Reitz selbst im Excerpt wiedergegebene Blaubuchtabelle enthält ja nicht weniger als 40,391 Fälle, während die vollständige Tabelle des "Bluebook" (p. XXVII) 92,342 bei Kussmaul (S. 59) sogar 118,171 Krankheilsfälle umfasst! Es ist wahr, dass für die Lethalität gleich die Procente wiedergegeben sind, und die absoluten Zahlen (aus Raumersparniss) unterdrückt wurden. Aber glaubt Reitz wirklich, dass unter diesen 118,171 Kranken 118,168 Geimpfte und nur drei Ungeimpfte waren, oder umgekehrt? Und wenn er dies für wahrscheinlich hält, warum nicht in den Quellen nachsehen, bevor eine solche Verdächtigung in die Welt geschickt wird? Wenn auch nicht für alle Quellen, so wäre es doch für einige derselben ein leichtes Spiel gewesen, sich die absoluten Daten zu verschaffen. Reitz, der gewesene Zögling der Wiener Schule, der — wie wir

sahen — in den Berichten des Wiener Krankenhauses so gut bewandert ist, hätte doch auch bei diesem wichtigen Momente nur diese Berichte zur Hand zu nehmen gebraucht, um für die oben citirten Jahre, auf Seite 162 des Jahresberichtes pro 1862, jene absoluten Ziffern zu finden, deren Alibi ein so schweres Verdachtsmoment abgeben musste. Es beträgt nämlich daselbst die Lethalität:

bei Geimpften:	für die Männer	2.40	pCt.,	weil	von	1040	Kranken	25	starben,
	" " Frauen	5.32	"	"	"	902	"	48	"
bei Ungeimpften:	" " Männer	17.24	"	"	"	116	"	20	"
	" " Frauen	19.60	"	"	"	102	"	20	"

Gleich günstige Ergebnisse hätte Reitz noch für die ganze Reihe der angeführten Angaben in den Quellen finden müssen. Wäre es nicht loyaler gewesen, dies zu thun, statt die so verdienstliche Statistik des Blaubuches und Kussmaul's, vor dem — ob der vielen Zahlen ohnehin schon betäubten — Leser ungerechterweise zu verdächtigen? Und ist es nicht eine Fortsetzung und Steigerung dieser Illoyalität, nach einer so unbegründeten Verdächtigung, also fortzufahren: "Auf solche (*sic!*) Daten sich stützend, verstanden die Impfer ihre Ansicht zu einem Dogma zu erheben." Das kann doch nicht anders gedeutet werden, als ob die angezogenen Daten nur scheinbar oder fälschlich zu Gunsten der Impfung sprächen und als ob Reitz oder Andere diese Irreführung aufgedeckt hätten. Dem ist aber doch nicht so: Die im "Bluebook" und bei Kussmaul angeführten Daten konnten von Niemand für unrichtig bezeichnet werden, und am wenigsten geschah dies durch Reitz. Wozu dann diese irreführende Rede?

Es folgen Seite 18 zwei Vorwürfe an die Impfer: Dass sie die Altersklassen der Ungeimpften nicht berücksichtigten, und dass sie ferner die verschiedenen Arten von Blattern (Variola vera, V. modificata, Varicella) nicht von einander unterschieden. Auf beide Anklagen ist damit zu antworten, dass dieselben nicht allein die Impfer, sondern ebenso auch die Impfgegner treffen, überdies aber mit der eigentlichen Streitfrage gar nichts zu thun haben. Ob man in der Nosologie der Blattern auf dem Standpunkte der Identität, oder auf jenem der Nichtidentität stehe, ob man nämlich die drei Formen der Blattern für ein und dieselbe, oder für drei verschiedene Krankheiten halte, das hat doch wahrlich mit der Frage, ob die Vaccination schütze oder nicht, nichts gemein. Ebensowenig der Umstand, ob man genug Statistiker sei oder nicht, um zu wissen, dass alle mortalitäts-statistischen Beobachtungen, also auch jene über die Sterblichkeit Ungeimpfter, nach Altersklassen vor sich gehen müssen. Im siebenten Abschnitte habe ich zum Ueberflusse nachgewiesen, dass diese letztere Verbesserung eigentlich von Impffreunden eingeführt wurde, und dass Hauptstimmführer der Impfgegner in ihren eigenen Beobachtungen diesen Forderungen nicht nachkamen. Die Impfgegner haben also absolut kein Recht, der Impfvertheidigung in dieser Beziehung Vorwürfe in's Gesicht zu schleudern.

Reitz geht nun einen Schritt weiter und versucht zu beweisen, dass bei echten Blattern die Lethalität der Geimpften oft eine sehr bedeutende sei (bemerken wir hierbei, dass dies eigentlich gar nichts besagt, da es sich ja darum handelt, ob die Sterblichkeit der Geimpften stärker oder schwächer als die der Ungeimpften sei) und führt er zu diesem Behufe die Erfahrungen des Berliner Pocken-Lazarethes, sowie jene Hebra's im Wiener allgemeinen Krankenhause an. Folgen wir ihm in diesen Beweisgängen:

Berliner Pocken-Lazareth 1871. Reitz citirt die Daten *Meyer's* nach dem *Monatsblatt für medicinische Statistik.* Die Daten sind daselbst, wie wohl nicht wesentlich verändert, so doch nicht ganz richtig wiedergegeben. Wir wollen also lieber die Originalmittheilungen Meyer's[1] zur Grundlage nehmen, wie sich dieselben im Jahrgang

[1] Dr. Lothar Meyer: Bericht an die kgl. Sanitäts-Commission über die Pockenepidemie d. J. 1870–'71 in dem städtischen Pockenkrankenhause; in No. 28 und 29 der *Deutschen Klinik.* Wir

1872 von Göschen's *Deutscher Klinik* von ihm mitgetheilt finden. Die sehr gründliche Arbeit laborirt leider an dem Uebel absoluter Unübersichtlichkeit. Ich glaube Allen, die sich um die Statistik des Berliner Pockenhauses zur Zeit einer der bedeutendsten Epidemieen interessiren, einen kleinen Dienst zu erweisen, indem ich die in ganzen Serien breit angelegter Tabellen zerstreuten Angaben Dr. Meyer's in der nachfolgenden kleinen Tabelle übersichtlich zusammenfasse, wobei ich bemerke, dass ich von den aufgestellten fünf Arten der Variola-Erkrankungen nur die uns interessirenden drei (Variola, Variola hæmmorrhagica, Varioloïs) in Betracht ziehe und Febris variolosa sine variola (20 Erkrankungen, alle geimpft), sowie Varicella (27 Erkrankungsfälle, hiervon 20 geimpft) ausser Betracht lasse.

Reitz theilt uns nun mit, dass an Variola vera von den erwachsenen (überfünfzehnjährigen) Geimpften 31.46%, hingegen von Ungeimpften nur 14.28% starben. Die Percente wären richtig; man darf aber nicht übersehen, dass die Erfahrungen dieses ganzen Lazarethes sich für unsere Streitfrage eigentlich gar nicht verwerthen lassen, weil es nämlich unter den Ueberfünfzehnjährigen — die Reitz allein in Betracht zieht — zwar 1459 Geimpfte, aber nur fünfzehn Revaccinirte und gar nur *sieben* Nichtgeimpfte gab! Was will man aus solchen sieben Fällen herauslesen: wenn einer lethal verlief, so macht das 14%, wenn aber zufällig noch ein zweiter Todesfall dazu kam, so stiege die Lethalität schon auf 28%! Statistische Beweise bedürfen stets grossen, massenhaften Materials; in unseren eigenen Berechnungen haben wir z. B. bei allen Aufzeichnungen, denen weniger als hundert Fälle unterlagen, die Percentberechnung unterlassen. Reitz wies vorher selbst darauf hin, wie unberechtigt die Folgerungen aus kleinen Zahlen seien: er hätte also besser gethan, das Berliner Lazareth, wo ganze sieben Ungeimpfte den Gegenstand der statistischen Massenbeobachtung abgeben mussten, einfach zu übergehen. Wir würden ein Gleiches mit den Reitz'schen Auseinandersetzungen thun, hätten wir nicht ein theoretisches Interesse daran, zu zeigen, wie vorsichtig man die Seitens der Impfgegner vorgeführten Zahlen oft aufnehmen müsse. Nur aus dieser Ursache gehen wir in eine weitere Prüfung der Reitz'schen Beweisführung ein.

Fragen wir vor Allem, wie die Diagnose auf Variola vera gestellt worden sei, so erfahren wir, dass diese — wie im Wiener Krankenhause so auch hier — erst *nach* dem Ablauf der Krankheit gemacht wurde. Es hätte dies Reitz schon aus dem Umstande auffallen müssen, dass unter den 838 Fällen von Varioloïs nicht *ein einziger* Todesfall vorfiel! So wurde z. B. selbst Variola hæmorrhagica nicht nach äusseren Symptomen, ja nicht einmal nach dem Vorkommen hämorrhagischer Efflorescenzen diagnosticirt, sondern war ausschliesslich die Perniciosität des Falles für die Classificirung massgebend.[1] Was Wunder also, wenn von den gefährlichen Fällen mehr starben, als von den ungefährlichen. Unter solchen Umständen liesse sich ein berechtigter statistischer Schluss nur auf zwei Wegen gewinnen: 1) Da die Diagnose von der These ausgeht, dass sich die drei Krankheitsformen von einander nicht unterscheiden lassen,[2] die Classificirung also sich bloss nach der Perniciosität richtet, müsste man alle

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wollen der Vollständigkeit halber auch bemerken, dass sich dieser Bericht nicht — wie Reitz anführt — auf die Ergebnisse des Jahres 1871, sondern auf die Zeit vom 1. Januar bis 20. Juli 1870 und 11. August bis 31. December 1871 bezieht.

[1] Meyer sagt Seite 263: "Unter die Gruppe Variola hæmorrhagica habe ich *nicht etwa alle diejenigen Fälle gerechnet, bei denen überhaupt....hämorrhagische Phänomene zu constatiren waren....* vielmehr glaubte ich jene Bezeichnung *nur für die allerschlimmsten Formen reserviren zu müssen.*"

[2] Auch Meyer sagt ebendaselbst: "Variola hæmorrhagica, Variola und Varioloïs, sind nur graduell verschieden, *ein und dieselbe Krankheit.*"

POCKENMORTALITÄT IM BERLINER STÄDTISCHEN POCKEN-HAUSE, 1870-71.

(Zusammenfassung der Tabellen von Dr. Lothar Meyer in Göschen's "Deutscher Klinik", 1872.)

Alter.	Impfzustand.	Variola Hæmorrhag. Erkrankte.	Variola Hæmorrhag. Gestorbene.	Variola. Erkrankte.	Variola. Gestorbene.	Variolois. Erkrankte.	Variolois. Gestorbene.	Zusammen. Erkrankte.	Zusammen. Gestorbene
0-1 Jahr	Ungeimpft.	...	...	17	15	...	...	17	15
1-5 Jahre	Ungeimpft.	2	2	5	3	...	...	7	5
6-15 Jahre	Ungeimpft.	1	1	5	2	...	...	6	3
	1 Mal geimpft.	...	...	2	1	3	...	5	1
	Zusammen.	1	1	7	3	3	...	11	4
16-20 Jahre	Ungeimpft.	...	...	2	1	...	...	2	1
	1 Mal geimpft.	14	14	98	21	301	...	413	35
	Zusammen.	14	14	100	22	301	...	415	36
21-30 Jahre	Ungeimpft.	...	...	5	...	...	...	5	...
	1 Mal geimpft.	38	36	241	26	391	...	670	62
	Zusammen.	38	36	246	26	391	..	675	62
31-40 Jahre	1 Mal geimpft.	21	19	96	23	96	...	213	42
	2 Mal geimpft.	...	...	...	...	5	...	5	...
	Zusammen.	21	19	96	23	101	...	218	42
41-50 Jahre	1 Mal geimpft.	20	15	49	14	15	...	84	29
	2 Mal geimpft.	...	...	...	...	3	...	3	...
	Zusammen.	20	15	49	14	18	...	87	29
51-60 Jahre	1 Mal geimpft.	13	12	34	9	9	...	56	21
	2 Mal geimpft.	...	...	...	...	6	...	6	...
	Zusammen.	13	12	34	9	15	...	62	21
61-70 Jahre	1 Mal geimpft.	6	6	8	4	5	...	19	10
	2 Mal geimpft.	...	...	...	...	1	...	1	...
	Geblattert.	...	...	1	...	1	...	2	...
	Zusammen.	6	6	9	4	7	...	22	10
Über 70 Jahre	1 Mal geimpft.	3	2	1	1	...	...	4	3
	Geblattert.	1	1	2	2	2	...	5	3
	Zusammen.	4	3	3	3	2	...	9	6
RECAPITULATION NACH DEM IMPFZUSTAND.									
Ungeimpft		3	3	34	21	...	...	37	24
Einmal geimpft		115	104	529	99	820	...	1,464	203
Zweimal geimpft		...	...	...	...	15	...	15	...
Geblattert		1	1	3	2	3	...	7	3
Zusammen		119	108	566	122	838	...	1,523	230

Formen zusammenfassen; für diesen Fall ergäbe sich, dass (bei den Ueberfünfzehnjährigen)

von 1459	Geimpften starben	202,
" 7	Ungeimpften starb	1,
" 15	zwei Mal Geimpften starb	0,
" 7	Geblatterten (alle über 60 Jahre alt) starben	3.

Oder man müsste 2) fragen, wie viele der Geimpften und wie viele der Ungeimpften an der perniciösesten Form erkrankten? In diesem Falle ergibt sich, dass derselben angehörten:

von 1459	Geimpften	115,
" 7	Ungeimpften	1,
" 15	zwei Mal Geimpften	0,
" 7	Geblatterten (alle über 60 Jahre alt)	1.

Dies wären die allein richtigen Aufstellungen gewesen. Percentualberechnungen sind bei der geringen Anzahl der Ungeimpften unmöglich;[1] aber es ist selbst ohne diese unbegreiflich, wie man solche Daten als Beweise des Nichtschutzes verwerthen konnte.

Wenn Reitz den welterschütternden Beweis von der grösseren Sterblichkeit nicht auf armselige sieben Fälle hätte gründen wollen, wenn er dieser wichtigen Frage doch so viel Liebe zugewendet hätte, sich zum Mindesten auch noch darüber zu informiren, was für Ergebnisse sich denn bei den übrigen Pockenspitälern eben dieser Stadt ergäben, so hätte er auf folgende Thatsachen stossen müssen:

1) In derselben Zeitschrift, aus welcher er die Daten über die städtische Pocken-Anstalt schöpfte, nämlich in Göschen's *Deutscher Klinik* (1872), findet man auch den durch mehrere Nummern sich hinziehenden Bericht von Dr. Guttstadt über das von ihm geleitete grösste der vier Berliner Pockenlazarethe, nämlich No. 4, in welchem über die Hälfte mehr (d. i. 1853) Blatternkranke Aufnahme fanden, als in der Meyer-'schen Klinik. Wie bei Meyer, gab es auch hier fast gar keine Kinder, im Ganzen nur 3 %.[2] Wir könnten also füglich die Gesammtheit als erwachsen betrachten. Um aber ganz wie Reitz zu verfahren, schliessen wir die Unerwachsenen aus und erhalten dann folgende Lethalitätszahlen:

Es starben bei den erwachsenen

Geimpften		Ungeimpften	
von 742 Kranken	128,	von 13 Kranken	10,

also beinahe alle Ungeimpften, während von den Geimpften nur 16 % starben!

Unserm Autor ist ferner die vielcitirte Arbeit Müller's im siebenzehnten Bande (1872) der Eulenberg'schen Vierteljahresschrift bekannt, denn er citirt dieselbe. Ebendaselbst hätte er nun die Lethalität der Geimpften und Ungeimpften *für alle vier*

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[1] Um nicht den Verdacht aufkommen zu lassen, als ob diese und die noch folgenden Verzichtleistungen auf Percentualberechnungen einer tendentiösen Absicht zuzuschreiben wären, will ich darauf hinweisen, dass ich (siehe die Vorbemerkungen dieses Anhanges) solchen Berechnungen schon vor siebenzehn Jahren auszuweichen liebte. Es lässt sich nicht läugnen, dass jenen Bedenken, die in dem zu geringen Gewichte der absoluten Zahlen ihren Grund haben, im Allgemeinen nicht genügende Berücksichtigung gezollt wird. Auf allen Wegen der Statistik, so auch in der Vaccinationsstatistik, und zwar ebenso bei Impffreunden wie bei Impfgegnern, begegnen wir bis auf Hundertstel und Tausendstel berechneten Percentualwerthen, deren Grundlage kaum ein Dutzend von Beobachtungen bildet. So berechnet z. B. in obigem Falle Reitz aus dem *einen* (!) Todesfall, der bei sieben Ungeimpften vorfiel, ruhig eine Lethalität von 14 % (ja sogar von 14.28!).

[2] Es gab im Ganzen im Alter bis 1 Jahr 14,
1— 5 " 15, und
5—10 " 14 Kranke.

Pockenlazarethe Berlins finden können, und zwar in nachfolgenden Ziffern, die ich nach Dr. Müller (l. c. S. 319) wiedergebe:

Es gab in der Pockenheilanstalt

	No. I	No. II	No. III	No. IV	Zusammen
ungeimpfte Erkrankte....	44	31	9	48	132
einmal geimpfte "	1510	1400	1376	1662	5948
revaccinirte "	19	73	34	143	269
zweifelhafte Fälle.....	—	—	8	—	8
	1580[1]	1504	1427	1853	6357

Es starben daselbst:

ungeimpfte Erkrankte....	24	22	6	39	91
einmal geimpfte "	203	207	220	241	871
revaccinirte "	—	3	2	13	18
zweifelhafte Fälle.....	—	—	2	—	2
	227	232	230	293	982

Berechnen wir nun die Lethalität für alle vier Pockenheilanstalten, so ergibt sich, dass

von 132 Ungeimpften verstarben ..... 91 = **68.9** pCt. (!)
" 5948 Geimpften aber nur ..... 871 = **14.6** "
und " 269 Revaccinirten sogar nur ..... 18 = **6.7** "

Der aus den Erfahrungen der Berliner Pockenspitäler geholte Beweis ist also *als total verunglückt zu betrachten.* Die diesbezüglichen Erfahrungen geben wieder *ein eclatantes Zeugniss für die Schutzkraft der Impfung und noch mehr für jene der Revaccination ab.*

2) Ein Gleiches muss aber auch hinsichtlich der aus Hebra's Klinik im Wiener allgemeinen Krankenhause angezogenen Beobachtungen bemerkt werden. Es wurde im sechsten Kapitel eingehend nachgewiesen, dass die Erfahrungen dieses Krankenhauses, *entschiedenst für den Schutz der Impfung sprechen* und keineswegs zur Unterstützung impfgegnerischer Ansichten dienen können. Auch bezüglich der Seite 19 citirten, angeblich impffeindlichen Beobachtungen Fleischmann's aus dem St. Josephs-Kinderspital in Wien haben wir im selben Kapitel nachgewiesen, dass diese, *nach Fleischmann's eigenen Worten*, den durch die Impfung gebotenen Schutz als "über allen Zweifel erhaben" hinstellen.

Die Nutzlosigkeit der Impfung soll ferner auch dadurch bewiesen werden, dass man einzelne Fälle von geringer Lethalität bei Ungeimpften und daneben einzelne Fälle von exorbitanter Lethalität bei Geimpften einander gegenüberstellt. Mit Hülfe einer solchen "tendentiösen Auswahl" liesse sich in der Statistik freilich Alles vertheidigen, ob aber auch *beweisen*, das ist eine andere Frage. In einem Codex der statistischen Beweisführung würde eine tendentiöse Auswahl der Fälle als unerlaubt bezeichnet werden müssen; namentlich würde man es verpönen müssen, integrirende Theile ein und derselben Beobachtungen tendentiös zusammenzulesen und dabei alle jene Theile, welche gegen die verfolgte Tendenz sind, einfach zu verläugnen. Es involvirt ein solches Vorgehen auch eine Illoyalität gegen den Leser, der all den angeführten Quellen nicht nachgehen kann, sich also mit Recht darauf verlässt, dass die Beobachtung der als Autorität angeführten Quelle taliter qualiter wiedergegeben wurde.

Eine solche tendentiöse Verstümmelung der Originalbeobachtung hat sich nun

[1] Die (aus Müller übernommene) Addition ist unrichtig.

Reitz Seite 27 und 28 zu verschiedenenmalen zu Schulden kommen lassen. Wir wollen hier der Illustration halber nur zwei Citaten bis auf die Quelle nachgehen. So werden u. A. die Angaben Förster's und Strickers's citirt, um zu beweisen, "dass geimpfte und ungeimpfte Kinder gleich schwer an Pocken leiden und dass das Sterblichkeitsprocent nicht davon abhängt, ob die Kinder geimpft waren oder nicht, sondern hauptsächlich von ihrem Alter und dem allgemeinen Zustande ihrer Gesundheit vor der Erkrankung."

Bezüglich der Angaben Förster's (Dresdener Kinderheilanstalt) heisst es nun (Seite 27): "In Behandlung befanden sich im Ganzen 233 pockenkranke Kinder, darunter 9 geimpfte (alle über 2 Jahre alt), 34 zweifelhaft, ob geimpft, und 190 ungeimpfte. Die Mortalität der *nichtvaccinirten* Kinder war folgende: von 53 Kindern im Alter unter einem Jahr starben 12, von 103 im Alter von 1—5 Jahren starben 12, von 41 im Alter von 5—10 Jahren starben 5, von 11 im Alter von 10—14 Jahren starb kein einziges. Aus diesen Daten ist ersichtlich, dass die Gesammtsterblichkeit der *ungeimpften* Kinder 15.4 % betrug und dass die grösste Zahl die im ersten Lebensjahre stehenden Kinder, von denen etwa der dritte Theil starb, lieferten." Das macht doch unbedingt den Eindruck, als ob nun eine grosse Lanze für die Sache der Impfskepsis gebrochen worden sei. Eigentlich ist aber trotz aller Ziffern und Worte nicht einmal so viel zu entnehmen, ob von Geimpften oder Ungeimpften mehr gestorben seien, und das wäre doch, meinen wir, die Hauptsache. Hätte aber Reitz die Beobachtungen Förster's unverstümmelt wiedergegeben, so würde man sehen, dass im Verlaufe von 33 Jahren im Kinderheilspitale zu Dresden folgende Verhältnisse herrschten:[1]

	Erkrankt:	Gestorben:	
Ungeimpfte	190	29	= 15.4 pCt.
Geimpfte	9	—	= — "
Zweifelhafte	34	2	(= 13.3 ")

Hier kommen unbedingt die Geimpften, von denen doch kein Einziger starb, besser fort: was hatte also das ganze Wortgeplänkel bei Reitz zu bedeuten?

Aus Stricker wird zum Beweise der grösseren Sterblichkeit bei Vaccinirten erwähnt, dass in Preussen 8 % der vaccinirten Kinder verstarben. Lesen wir nun bei Stricker[2] Seite 27 selbst nach, so finden wir daselbst richtig, dass von hundert vaccinirten Kindern 8 % starben; gleich daneben ist aber auch zu lesen, dass von hundert nichtvaccinirten nicht weniger als 32 % verstarben! Reitz theilt nun seinen Lesern wohl die Sterblichkeit der Vaccinirten mit, die viel grössere Sterblichkeit der Nichtvaccinirten wird aber von ihm einfach unterdrückt! Mit solchen Waffen sollte nicht gekämpft werden.

Im weiteren Verlaufe seines anti-vaccinatorischen Streifzuges kommt Reitz (Seite 29) auf Müller's Daten über die Berliner Epidemie vom Jahre 1871 zu sprechen. Nach Dem, was wir über die Unverlässlichkeit Müller's und speciell seiner hier citirten Beobachtungen bereits im siebenten Kapitel vorgebracht, brauchen wir uns mit dieser Statistik nicht weiter zu beschäftigen.

Es erübrigt bei Reitz noch ein Hauptangriff gegen die angebliche Schutzkraft der Vaccination. Es sind dies die auf Tabelle II (Seite 32) zusammengestellten Daten über die gegenwärtige Lethalität der Pocken, verglichen mit der Lethalität in der prävaccinatorischen Zeit; es ist dies also ein "historischer Beweis" im Sinne der Impfskepsis.

Diese Tabelle enthält 25 Angaben über Epidemieen, die in verschiedenen Ländern, bezw. Städten, wo obligatorische Impfung — angeblich — eingeführt ist, geherrscht haben. Die geringste dieser Epidemieen (Brighton) umfasst 393, die grösste (Frank-

[1] Siehe "Jahrbuch der Kinderheilkunde." I. Bd., 1868, S. 136.

[2] Stricker. "Studien über Menschenblattern." Frankfurt a. M., 1861.

reich, 1826) rund 40,000 Fälle, während überdies von Frankreich für die ganze Decade von 1841 bis 1850 zusammengenommen 154,796 Erkrankungsfälle verrechnet sind. Im Ganzen umfasst diese Tabelle 313,312 Erkrankungen der nach-Jenner-'schen Zeit, mit 43,571 Todesfällen, was also einer Lethalität von 14.03 % entspricht. Da nun — meint Reitz — im ganzen vorigen Jahrhundert, vor Einführung der Impfung, die Lethalität 8½ bis 14 % betrug, so wäre hiermit bewiesen, dass die Impfung an diesen Verhältnissen gar nichts geändert habe, " dass gegenwärtig das mittlere Sterblichkeitsprocent der Blatternkranken durchaus nicht geringer, sondern eher grösser geworden ist."

Hier wirft sich vor Allem die Frage auf: wie es möglich gewesen sei, die Lethalität der Pocken aus dem vorigen Jahrhundert, in welchem es bekanntlich kaum eine Statistik gab, zu bestimmen? Es ist vorauszusehen, dass man es hier, besten Falles, mit den engeren, persönlichen Erfahrungen einzelner Beobachter zu thun haben wird. Und wirklich finden wir, dass den Reitz'schen Angaben über die Blatternlethalität des vorigen Jahrhunderts keine einzige allgemeine statistische Beobachtung, sondern bloss die Berufung auf die Erfahrungen Einzelner, und dabei sehr weniger Fachmänner, zu Grunde liegt. Reitz kann sich nur auf drei Autoritäten berufen, von denen eine, Dr. Frank, zu Ende des letzten Jahrhunderts lebte, also wenigstens den Vorzug der Gleichzeitigkeit hat. Wir wissen aber, wie dehnbar selbst von zeitgenössischen Fachmännern stammende Urtheile sind, wenn selbe sich nicht auf präcise, ziffermässige und ausgedehnte, Aufzeichnungen stützen. Die zwei weiter citirten Autoritäten sind Oesterlen und Kussmaul, die aber, als unsere eigenen Zeitgenossen, über die Sterblichkeit des vorigen Jahrhunderts keine direkte Zeugenschaft ablegen können.

Was übrigens die Berufung auf Kussmaul betrifft, so beruft sich dieser seinerseits (S. 89) auf Süssmilch, den bahnbrechenden Privat-Statistiker des vorigen Jahrhunderts. Wie man aber wieder bei Süssmilch nachlesen kann,[1] meint derselbe (und mit ihm auch Kussmaul) etwas ganz Anderes, als was Reitz darin findet. Letzterer will uns Anhaltspunkte über die *Lethalität* der Blattern geben, d. h. wie viel von 100 *Erkrankten* im Verlaufe dieser Krankheit, also *innerhalb einiger Wochen*, starben; Süssmilch aber spricht von der *Mortalität*, d. h. wie viele von hundert *Lebenden* an Blattern starben, und zwar im Laufe eines *Jahres!* Diese Mortalität bildet nun 8 % der totalen Sterblichkeit an allen Krankheiten zusammen, d. h. falls von 100,000 Menschen jährlich 4000 starben, so war es wahrscheinlich, dass hierunter 8 % = 320 an Blattern gestorben sein mochten. Das will aber durchaus nicht sagen, dass wenn 4000 Menschen an Blattern *erkranken*, hiervon im Verlaufe der Krankheit nur 320 sterben würden! Lethalität und Mortalität sind — wie wir dies in den vorigen Abschnitten genügend hervorgehoben zu haben meinen — total verschiedene Begriffe, welche Reitz hier miteinander verwechselt hat. Auf einer derartigen Confundirung dieser verschiedenen Begriffe sind dann alle weiteren Schlussfolgerungen unseres Autors aufgebaut!

Wir haben aber noch eine weitere Unzulänglichkeit zwischen den Einzelbeobachtungen und den durch Reitz citirten Massenbeobachtungen der Gegenwart zu constatiren, welcher Unterschied bei Vergleichungen mit der prävaccinatorischen Periode nicht genügend beachtet zu werden scheint.

Die Lethalitätsziffern des vorigen Jahrhunderts stammen aus einem engen, dafür aber geschlossenen Kreise. Wenn Van Swieten in seinen Ephemeriden Lethalitätsangaben für die Pocken bietet, so wusste er genau, wie viel Kranke er gehabt habe und wie viele darunter gestorben seien; desgleichen bei Lethalitätsbeobachtungen aus Spitälern. Wir haben es in diesen Fällen zwar mit nicht genug massenhaften, hingegen aber mit formal vollkommen richtig berechneten Lethalitätsziffern zu thun. Wie steht es aber um die von Reitz angeführten Epidemie-Daten? Spitalsbeobachtungen

[1] Süssmilch. "Göttliche Ordnung." I, § 267 und § 528.

werden von ihm total ausgeschlossen, und so bleiben nur jene Pseudo-Lethalitätsziffern übrig, welche aus der unverlässlichen Quelle der polizeilichen Anmeldungen geschöpft sind. Nun ist es aber klar — man vergleiche das sechste Kapitel —, dass solche Pseudo-Lethalitäten stets zu grosse sein werden: von den Erkrankungsfällen entgeht nämlich nothgedrungen stets ein Theil der Aufzeichnung, während von den Todesfällen kein einziger entgehen kann. Das Verhältniss der Todesfälle zu den Erkrankten ist in Folge dessen stets ein ungünstigeres als in Wirklichkeit, und als bei den aus geschlossenen Kreisen berechneten Lethalitätsverhältnissen. Dies würde es also erklären, wenn die aus Epidemieen geschöpften (Pseudo-) Lethalitätsangaben der modernen Statistik sich stets ungünstiger gestalteten, als die gleichzeitigen Lethalitätsangaben aus Spitälern oder die Lethalitätsangaben älterer Zeiten.[1]

Was schliesslich die principielle Ausserachtlassung der Spitalsbeobachtungen betrifft, so wollen wir hierüber nicht rechten, da Reitz diesbezüglich dieselbe Abstinenz beweist, wie auch Oesterlen und andere Impffreunde. Wir wollen hierbei aber dennoch auf das im zehnten Kapitel Gesagte hinweisen, wonach der Umstand, dass die Sterblichkeit von Geimpften selbst in Spitälern eine geringere ist, nur um so entschiedener zu Gunsten der Schutzkraft spricht, die da selbst durch die in Spitälern herrschenden ungünstigen Verhältnisse nicht gebrochen werden konnte. Ueber alle diese Bedenken hinaus greift aber noch jenes, im vorhergehenden Abschnitte entwickelte Argument, dass selbst im Falle, als es zu beweisen gelänge, dass die Blatternlethalität gegenwärtig grösser als im vorigen Jahrhundert sei — was auf dem von Reitz eingeschlagenen Wege absolut nicht zu beweisen ist —, dies für die in Verhandlung stehende Frage eigentlich absolut irrelevant wäre. Es könnte ja immerhin möglich sein, dass die Blatternlethalität gegenwärtig grösser als im vorigen Jahrhundert ist. Was hätte dies aber mit der Frage zu thun, ob Geimpfte oder Ungeimpfte gegen Blattern mehr geschützt seien? Nehmen wir an, man hätte schon bewiesen, dass im neunzehnten Jahrhundert die Blatternlethalität dreifach höher als im achtzehnten Jahrhundert war; was ficht uns dies an, nachdem wir nun — auf Grund der vorher mitgetheilten neuen Beobachtungen — ersehen haben, dass die Geimpften sechsfach besser, als die Ungeimpften gegen den Blatterntod geschützt sind? Die ganze angebliche Steigerung der Blatternlethalität im neunzehnten Jahrhundert müsste also auf Rechnung der Ungeimpften fallen! Ganz abgesehen hievon, kann aber aus dem von Reitz angeführten angeblichen Factum, "dass im vorigen Jahrhundert das mittlere Procent der gesammten Sterblichkeit in Folge von Pocken[2] 8½—14 % betragen habe," hingegen "seit Einführung der Vaccination das mittlere Sterblichkeitsprocent der Blatternkranken[3] durchaus nicht geringer, sondern eher grösser geworden ist, als das für das vorige Jahrhundert, vor der Existenz der Schutzpockenimpfung, ausgerechnete," über die Wirksamkeit der Impfung oder Nichtimpfung gar kein Schluss gezogen werden.

Um aber das von Reitz mühsam zusammengetragene Material nicht ganz unbenützt bei Seite stellen zu müssen, wollen wir versuchen, dasselbe in einer anderen, hoffentlich vom Autor selbst zu billigenden Weise, zu verwerthen. Es ist dies durch Anwendung der im vierten Kapitel als geographisch bezeichneten Methode möglich, wobei man nämlich verschiedene Staaten je nach der strengeren oder schwächeren Durchführung der Schutzimpfung in verschiedene Klassen bringt, und dann zusieht, wo die Mortalität und die Lethalität eine günstigere gewesen. Von den bei Reitz angeführten Staaten hatten nun Schweden, Dänemark, Bayern und Württemberg den Impfzwang schon zu Beginn dieses Jahrhunderts eingeführt, England erst 1871.

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[1] Um eine derartige Parallele zu ziehen, müsste man freilich nicht nur die Verstorbenen nach Altersklassen gruppiren können — wozu uns genügende Daten vorliegen —, sondern auch die Krankgemeldeten.

[2] Also *Mortalität*, beziehentlich Bruchtheile der gesammten Mortalität. [3] also *Lethalität*!

Deutschland erst 1874. In Preussen besteht die Impfpflicht seit dem Jahre 1835; sie wurde aber bis vor 1874 nicht strenge gehandhabt, was namentlich für Berlin gilt.[1] In Oesterreich-Ungarn, Italien, Frankreich und Genf besteht kein Impfzwang; in Basel wohl.

Theilen wir nun die von Reitz angeführten Epidemieen in zwei Gruppen, je nachdem die Länder, in denen sie erfolgten, Impfzwang übten oder nicht, so ergeben sich folgende Lethalitätszahlen:

a) LÄNDER OHNE IMPFZWANG:

	Erkrankt:	Gestorben:
Epidemie von Verona, 1810—1838	4,119	429
" " Hamburg, 1823—1825	1,684	273
" " Cambridge, 1825	584	84
" " Frankreich, 1826	40,000	8,000
" " Turin, 1829	4,235	761
" " Frankreich, 1841—1850	154,796	21,488
" " Genf, 1858—59	1,568	170
" " Cournon-terrale bei Montpellier, 1871—1872	750	101
" " Krems, 1871—1872	717	177

Von 208,453 Erkrankten verstarben also 31,483 = **15.11** pCt.

b) LÄNDER MIT IMPFZWANG:

Epidemie von Kopenhagen, 1823—1825	459	50
" " " 1825	1,197	98
" " Württemberg, 1831—1836	1,677	198
" " " 1848—1850	6,258	615
" " " 1854—1868	12,901	952
" " Preussen, 1857—1858	39,765	3,703
" " Ober-Bayern, Unter- und Mittel-Franken, 1866 bis 1869	12,115	971
" " Stuttgart, 1870	2,102	160
" " Basel, 1870—1871	455	57
" " München, 1871	1,472	151
" " Brighton, 1870—1871	393	88
" " Heilbronn, 1871	1,000	147

Von 79,794 Erkrankten starben also 7,200 = **9.02** pCt.

c) *Berlin.* Mit Rücksicht auf die oben angeführten Umstände können wir Berlin nicht als gutimpfend betrachten, weshalb wir die von Reitz angeführten Epidemieen dieser Stadt, 1683, 1864, 1871—1872, wo von 25,065 Erkrankten 5289, also 21.10 pCt., starben, in eine besondere Klasse verweisen.

Indem wir nun die Ergebnisse der drei Gruppen recapituliren, finden wir, dass

ad *a*)	von 208,453	Erkrankten	starben	31,483 = 15.11 pCt.	(kein Impfzwang)
" *b*)	" 79,794	"	"	7,200 = 9.02 "	(Impfzwang)
" *c*)	" 25,065	"	"	5,289 = 21.10 "	(unvollst. Impfzwang)
also starben	von 313,312	Erkrankten	.....	43,972 = 14,03 pCt.	

Wir sehen also, wie die von Reitz angeführten Epidemie-Daten noch immer *für* die grosse Schutzkraft der Impfung ein Zeugniss ablegen: Die Lethalität in den Staaten mit Impfzwang war nämlich in Epidemiejahren eine beinahe um die Hälfte geringere. Dieses günstige Ergebniss ist um so höher anzuschlagen, als der geographische Beweis nur ein indirekter ist, also die Schutzkraft der Impfung nicht vollkommen erkennen lässt, was schon aus der einen Betrachtung erhellt, dass ja selbst in gutimpfenden Staaten die Lethalität durch die grössere Sterblichkeit der Ungeimpften erhöht werden kann.

Auf den nächstfolgenden Seiten geht der Autor auf die durch die Impfung verursachten Schädigungen über; er behauptet in erster Reihe, und zwar auf Grund der Erfahrungen des St. Petersburger Kinderspitals, dass die Fälle, wo die Vaccination

[1] Nach Guttstadt entfielen von 1840 bis 1870 auf 100 Geburten im Maximum 83, im Minimum aber nur 29 (!) Impfungen.

durch Hautkrankheiten direkt den Tod herbeiführte, "häufig" waren (S. 38). Da uns die Ausweise des St. Petersburger Kinderspitals nicht zugänglich sind, und da wir über diese Frage ohnehin im zehnten Kapitel auf Grund überaus verlässlicher Spitalsbeobachtungen bereits zu einem beruhigenden Ergebnisse gelangt sind, brauchen wir uns bei diesem Theile des Buches nicht weiter aufzuhalten ; aus dem letzten Grunde auch bei den die Impfsyphilis berührenden Beobachtungen nicht, zumal die Absicht des Verfassers bloss dahin geht, den Unglauben an die Möglichkeit einer Impfsyphilis durch Anführung von Thatsachen zu bekämpfen, — ein Standpunkt, der von uns ohnehin als überwunden angesehen wird.

Auf Seite 54 tritt die Arbeit wieder in das breite Bett der Schutzpocken-Statistik. Es wird hier vor Allem die These aufgestellt, dass, wenn die Impfung schützt, dies in erster Reihe in einer geringeren Kindersterblichkeit zum Ausdruck kommen müsste. Wir wollen diesen Beweisgang, obzwar derselbe nur ein, und zwar par excellence indirekter ist, gelten lassen. Ohne eine solche statistische Untersuchung versucht zu haben, scheint es uns — trotzdem die Kindersterblichkeit durchaus nicht ausschliesslich durch die Pocken bestimmt wird und die Geburtenbewegung z. B. einen grösseren Einfluss ausüben dürfte — dennoch wahrscheinlich, dass nach Einführung der Impfung, und speciell des Impfzwanges, die Kindersterblichkeit eines Landes zurückweichen werde. Um diesen (historischen) Beweis zu erbringen, müsste also für jeden Staat die Kindersterblichkeit der prävaccinatorischen Periode mit jener der postvaccinatorischen verglichen werden. Was thut aber Reitz? Er vergleicht die gegenwärtige Kindersterblichkeit fast aller europäischen Staaten unter einander : das ist freilich ein Anderes, denn die Kindersterblichkeit kann auch in gut impfenden Staaten gross sein. In Süd-Deutschland und in Oesterreich-Ungarn herrscht grosse Kindersterblichkeit ; in den ersteren Staaten besteht aber der Impfzwang, in den letzteren wieder nicht. Reitz müsste untersuchen, ob in Süd-Deutschland die Kindersterblichkeit seit Einführung des Impfzwanges zugenommen, und selbst bejahenden Falles sein Urtheil noch suspendiren, bis er sich überzeugt, ob dies in allen Staaten mit Impfzwang der Fall sei. Statt dessen constatirt Reitz mit grosser Befriedigung, dass unter 16 angeführten Ländern die Impfzwangsstaaten Bayern und Württemberg an letzter Stelle zu stehen kommen. Dass aber Norwegen, Schweden, Dänemark und England an erster Stelle stehen, scheint der Autor nicht zu merken. Reitz wendet also den geographischen Beweis an. Dieser ist aber hier durchaus nicht am Platze, weil die verschiedenen geographischen Elemente (Staaten) zu vielen anderen solchen Factoren (grössere Geburtendichtigkeit, verschiedene Gesundheitsverhältnisse, verschiedener Bildungsgrad) ausgesetzt sind, die grössern Einfluss auf die Kindersterblichkeit ausüben, als die Impfung. Die Ergebnisse dieser unrichtigen Methode werden überdies nur verstümmelt, und zwar tendentiös verstümmelt, wiedergegeben und zum Ueberflusse wird die an sich unrichtige Methode auch noch unrichtig angewendet. Wiederholen wir, dass bei geographischen Beweisen, wenn dieselben etwas beweisen sollen, zwischen gut und schlecht impfenden Staaten unterschieden werden muss. Dies thut aber Reitz nicht. Versuchen wir diese Gegenüberstellung für die Reitz'schen Daten, so finden wir, dass auf 100 Lebendgeborene entfielen Todesfälle :

a) IN LÄNDERN MIT IMPFZWANG:		*b*) IN LÄNDERN OHNE IMPFZWANG:	
Norwegen	10.4	Belgien	15.5
Schweden	13.5	Frankreich	17.3
Dänemark	14.4	Spanien	18.6
England	15.4	Niederlande	19.6
Preussen	20.4	Italien	22.8
Bayern	32.7	Ungarn	24.7
Württemberg	34.4	Oesterreich	25.1
		Sachsen	26.3
		Baden	26.3
Durchschnitt 142.2 : 7 = **20.3**		Durchschnitt 196.2 : 9 = **21.8**;	

also herrscht geringere Kindersterblichkeit in den Impfzwangsländern, wobei aber noch zu bemerken wäre, dass die Reihe der Länder mit Impfzwang zwar vollständig, jene der Länder ohne Impfzwang unvollständig ist, indem daselbst ausser den kleinen deutschen Staaten auch noch Russland, Serbien, Rumänien, Griechenland u. s. w. mit ihren grossen Kindersterblichkeiten fehlen.

Der Autor übergeht Seite 58 auf die Angaben von Cless über die Pockensterblichkeit von Württemberg. Es heisst hier: "Dr. Cless sagt: Die Pockensterblichkeit, welche vor Einführung der Kuhpockenimpfung in Württemberg alljährlich, ohne Ausnahme, nach Tausenden zählte, war in den ersten Jahrzehnten nach Einführung der Kuhpockenimpfung auf einen verschwindenden Minimalbetrag reducirt. Dr. Cless führt auf Seite 60 desselben Buches nachstehende officielle Zahlen an:

In Württemberg starben an den Pocken:

Im Jahre 1780	1012	Im Jahre 1802	2225
" " 1781	1501	" " 1803	5659
" " 1782	1519	" " 1804	1538
" " 1783	814	" " 1805	794
" " 1784	832	" " 1806	1339
Summa	5678	Summa	11,565

Diese von Dr. Cless selbst angegebenen Zahlen zeigen, wie unzuverlässig seine Behauptung ist, dass die Pockensterblichkeit vor der Einführung der Kuhpockenimpfung in Württemberg alljährlich, ohne Ausnahme, nach Tausenden, in manchen Jahren aber nach vielen Tausenden zählte. Die Wirklichkeit zeigt uns gerade das Entgegengesetzte, denn in dem fünfjährigen Zeitraum nach der Einführung der Vaccination in Württemberg starben fast 6000 Menschen mehr, als im fünfjährigen Zeitraum vor Einführung dieser Massregel."

Man könnte dieser Beweisführung vor Allem entgegenhalten, dass die Angaben der Pockensterblichkeit der Jahre 1780—1785 nicht etwa aus jener Zeit selbst stammen, sondern so ungefähr ein Jahrhundert später (nämlich im Jahre 1863) aus den alten Kirchenbüchern zusammengeschrieben wurden, und zwar auch dies nur insoweit, als solche Kirchenbücher überhaupt vorhanden, bez. die Todesursachen in denselben registrirt waren. Nun ist es jedem Kenner der Württembergischen Pockenstatistik bekannt, dass die bis zu Ende des vorigen Jahrhunderts reichenden Angaben sehr lückenhafte sind, da eine grosse Anzahl von Kirchenbüchern nicht mehr vorhanden, in vielen die Todesursache nicht angegeben war, ja in manchen der Eintrag der kleineren Kinder ganz fehlte, wie man dies eben bei Cless nachlesen kann. Ferner möchten wir an Dr. Reitz die Frage stellen, welcher Umstand ihn eigentlich bewogen, für die nachvaccinatorische Periode gerade die Jahre 1802, 1803, 1804, 1805 und 1806 zu wählen? Wenn Reitz schon die zwei Endpunkte der von 1780—1810 reichenden Cless'schen Tabelle miteinander vergleichen wollte, warum entschied er sich nicht für die letzten fünf Jahre 1806—'10? Blieb sein impfgegnerisches Auge wohl an den Jahren mit grösster Sterblichkeit hängen? Es scheint wohl, denn die letzten fünf Jahre hätten nur 4532 Todesfälle ergeben, während die von Reitz tendentiös ausgewählten Jahre 11,565 ausweisen! In diesem Falle muss sich der Autor aber schon den Vorwurf gefallen lassen, seine Thatsachen künstlich gruppirt, tendentiös ausgewählt zu haben.

Wollten wir aus derselben Cless'schen Tabelle Daten in entgegengesetzter Richtung tendentiös gruppiren, so hätten wir für das vorige Jahrhundert etwa die folgenden Jahre wählen können:

1795	3775
1796	3630
1797	2918
1798	3255
1799	8867
also insgesammt mit	22,445 Fällen;

dies müsste die von Reitz herausgestochene nachvaccinatorische Periode mit nur 11,565 Fällen doch gewaltig in den Schatten stellen. Auf Grund so willkürlicher Auswahl hatte Reitz wahrlich kein Recht, seinem, alle Jahresziffern offen darlegenden, Gewährsmanne den Vorwurf in's Gesicht zu schleudern: "man sehe nun, wie unzuverlässig dessen Behauptungen seien!"

Die Verwunderung, mit der wir diesen Künsten der Zifferngruppirung folgen, muss aber noch steigen, wenn man Seite 59 Folgendes liest:

"Wenden wir uns jetzt zur Betrachtung der Pockensterblichkeit in Stuttgart vor Einführung der Kuhpockenimpfung und während des Bestehens der schon tief eingewurzelten Massregeln über obligatorische Vaccination und Revaccination, so werden wir noch einmal sehen, wie wenig Dr. Cless berechtigt war, zu behaupten, vor der Einführung der Vaccination wäre die Mortalität in Württemberg an den Blattern alljährlich, ohne Ausnahme, eine sehr bedeutende gewesen.

"Es starben an den Pocken in Stuttgart:		Es starben an den Pocken:	
1787	2	1863	11
1788	2	1864	56
1791	1	1865	33
1794	9	1869	50
1797	3	1870	160
Summe: in fünf Jahren starben	17	Summe: in fünf Jahren starben	310

"Nehmen wir einen sechsjährigen Zeitraum, so ergibt sich Folgendes:

Es starben an den Pocken:		Es starben an den Pocken:	
1790	17	1865	33
1791	1	1866	0
1792	28	1867	0
1793	103	1868	1
1794	9	1869	50
1795	11	1870	160
Summe der Verstorbenen	169	Summe der Verstorbenen	244

"Diese von Dr. Cless selbst angeführten Zahlen widerlegen von Neuem hinlänglich seine Behauptungen."

Mit Verlaub: Das sind nicht "*von Dr. Cless selbst angeführte Zahlen*"! Die Ziffern, die Cless meint, beziehen sich auf ganz Württemberg: was aber Reitz anführt nur auf das kleine—zu jener Zeit nur 19,000 Einwohner zählende!—Stuttgart. Cless sagt wörtlich (S. 67): "Die Pockensterblichkeit, welche vor Einführung der Kuhpockenimpfung in *Württemberg* alljährlich ohne Ausnahmen nach Tausenden zählte, war in den ersten Jahrzehnten nach Einführung der Kuhpockenimpfung auf einen verschwindenden Minimalbetrag reducirt," und die ziffernmässigen Angaben S. 58 und 60 entsprechen diesem Ausspruche vollkommen.

In dem von Reitz aus Cless citirten zweiten Beispiele soll bewiesen werden, dass in Stuttgart in der prävaccinatorischen Zeit weniger Pockenfälle vorkamen, als in der postvaccinatorischen. Zu diesem Behufe werden von Reitz aus beiden Epochen Perioden von je sechs (warum eben sechs??) Jahren herausgegriffen, oder sagen wir lieber herausgesucht: für die prävaccinatorische Periode werden nämlich die günstigsten Jahre (1790—1795), für die Gegenwart aber die ungünstigsten, nämlich jene gewählt, welche das stärkste Epidemiejahr unseres Jahrhunderts (1870) mitenthalten! Es ist dies ein nicht genug zu missbilligendes Vorgehen. Wenn man sich schon auf den historischen Beweis einlassen will, so nehme man doch die *ganze* Zeit vor und nach Einführung der Impfung und untersuche deren Pockensterblichkeit. In diesem Falle aber würde sich für Stuttgart herausstellen, dass von je 100,000 Einwohnern:

vor Einführung des Impfzwanges jährlich.....241
nach " " " " aber
(*mit* Einrechnung des Jahres 1870) **nur**..... **15**

Menschen an Pocken starben![1]

Zum Ueberflusse hat aber Reitz auch noch übersehen, dass, wenn man selbst auf seine Weise für die prävaccinatorische Periode 169, für die postvaccinatorische aber 244 Todesfälle herausrechnet, dies noch immer das Gegentheil Dessen beweist, was Reitz bewiesen sehen möchte, weil nämlich im vorigen Jahrhunderte Stuttgart kaum 20,000 Einwohner hatte, gegenwärtig aber 80,000, so dass selbst nach seiner Gruppirung auf je 100,000 Einwohner

für die sechs Jahre der prävaccinatorischen Zeit.....845,
" " " " " postvaccinatorischen Zeit aber nur 335 Todesfälle

entfallen![2]

Seite 62 und 63 folgen geographische Beweise: die Vergleichungen der Pockensterblichkeit in gut- und schlechtimpfenden Ländern; nach Dem, was wir hierüber im IV. Kapitel vorgebracht, brauchen wir auf diese Beweisart — die übrigens, wie erinnerlich, ebenfalls zu impfgünstigen Resultaten führt — nicht einzugehen.

Seite 64 und 65 folgt die unbedingt sehr merkwürdige französische Impfstatistik, mit ihren impffeindlichen Resultaten. Wir verweisen aber hierbei auf das diesbezüglich später — bei der Besprechung der Vogt'schen Arbeit — Vorzubringende, namentlich aber auf den Umstand, dass die Ergebnisse eines einzigen Jahres nicht maassgebend sein können, und wie die Resultate sich auch bei Vogt ändern, sobald man mehrere Jahre zusammenfasst.

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1 POCKENSTERBLICHKEIT IN STUTTGART.

a) Vor Einführung der Impfung.

					Auf 100,000 Einwohner. Zusammen.	Per Jahr.
1772—1781	Bevölkerung	18,000,	354	Pockentodesfälle =	1965	196
1782—1791	"	19,000,	366	" =	1926	192
1792—1801	"	20,000,	673	" =	3365	336
					7256 im 30j. Durchschn. = **241.**	

b) Während der Einführung der Impfung.

1802—1806	Bevölkerung	22,000,	154	Pockentodesfälle =	700	140
1807—1816	"	22,000,	3	" =	14	1.4
					714 im 15j. Durchschn. = **48.**	

c) Nach Einführung des Impfzwanges.

1818	Bevölkerung	26,000,	1	Pockentodesfall =	3.8	3.8
1818—1827	"	27,000,	2	" =	7.4	0.8
1828—1832	"	30,000,	2	" =	6.6	1.3
von 1833—1859 fehlen Angaben.						
1859—1863	Bevölkerung	61,000,	12	" =	19	3.4
1864—1870	"	80,000,	300	" =	375	53.6
					411.8 im 26j. Durchschn.=**15.02**	

[2] Solchen Fehlern gegenüber nimmt sich der verweisende Ton, mit dem der Autor einen einfachen Schreibfehler *Kussmaul's* behandelt, sehr eigenthümlich aus. Letzterer hat nämlich (S. 70) die Anzahl der Todesfälle von 1780—1789 mit 13,364 statt mit 23,364 notirt. Reitz fällt hierauf sogleich mit einem "Daraus sieht man, wie die Vertheidiger der Impfung mit officiellen Zahlen umgehen" ein. Wir sehen hier nur einen, jedem Statistiker möglichen Schreibfehler, der überdies den Gang der hieraus gezogenen Folgerungen nicht einmal stört. Für alle Fälle ist der in einem Schreibfehler liegende Irrthum ein unabsichtlicher, nicht auf Irreführung des Lesers berechneter, was man solchen Statistikern gegenüber, die tendentiöse Auswahl betreiben, nicht behaupten kann.

Seite 67 folgt eine Tabelle, welche die Pockensterblichkeit mehrerer Staaten in verschiedenen Perioden dieses Jahrhunderts enthält, also die Combinirung des geographischen mit dem historischen Beweise. Es soll aus dieser Tabelle bewiesen werden, "dass sich leider durch Einführung der Kuhpockenimpfung die Pockenepidemieen nicht verringert haben, sondern dass sich in letzter Zeit die Blatternsterblichkeit im westlichen Europa noch bedeutend vergrössert hat." Leider lässt sich dies aber aus dieser Tabelle durchaus nicht ersehen.

Dieselbe enthält nämlich nicht etwa Perioden vor und nach Einführung der Impfung, sondern Sterblichkeitsziffern für auf gut Glück herausgegriffene, mit der Impfeinführung in durchaus keinem Zusammenhange stehende Perioden. So wäre z. B. für Preussen das Jahr 1835 der Wendepunkt, da der Impfzwang in diesem Jahre eingeführt wurde; Reitz aber stellt folgende Perioden auf: 1810–'50, '51–'60, '61–'70. Für England wären die Grenzjahre

1841 (Gesetzliche Einführung der Impfung),
1853 bis 1867 (Gesetze betreffs Einführung des Impfzwanges),
1871 bis heute (Periode des strengsten Impfzwanges) u. s. f.;

statt dessen führt Reitz folgende Perioden an:

1837—1839
1842—1850 (!)

Was kann auf solche Weise bewiesen werden? Zum Ueberflusse verzichtet aber Reitz selbst auf die Möglichkeit dieses Beweises, indem er nämlich nicht einmal für die von ihm selbst aufgestellten Perioden die Schwankungen der Pockenmortalität berechnet, sondern die Pockenmortalität jedes Landes nur in *einer* Ziffer mittheilt. Was soll z. B. das Factum, dass in England in den Jahren 1837–'39 und 1842–'53 durchschnittlich 5192 Menschen an Pocken starben, für die Existenz oder Nichtexistenz des durch die Impfgesetzgebung beabsichtigten Pockenschutzes bedeuten?

Neben den zahlreichen fremden Daten führt der Verfasser schliesslich auch noch eine Originalbeobachtung an, welche die Ueberflüssigkeit der Impfung beweisen soll. Er hat nämlich aus der Ambulanz des seiner Direktion unterstehenden Elisabeth-Kinder-Hospitals in St. Petersburg, durch 2¾ Jahre (bis Ende 1873) die Anzahl der an Pocken erkrankten Vaccinirten und Nichtvaccinirten beobachtet und theilt das Resultat in Folgendem mit:

Alter.	Zahl der Vaccinirten.	Hierunter Pockenkranke.	Zahl der Nichtvaccinirten.	Hierunter Pockenkranke.
Bis zu 1 Jahr	877	19	5871	89
" " 2 Jahren	2064	24	2735	49
Von 2—3 "	1463	17	940	29
" 3—6 "	1839	29	639	37
Ueber 6 Jahre	2083	44	275	11
	8326	133	10,460	215

"Aus dieser Tabelle," sagt Reitz, "ersieht man, wie unbedeutend der Unterschied im Procente der Pockenkranken zwischen den Geimpften (= 1.59%) und zwischen den Ungeimpften (= 2.05%) ist."

Constatiren wir vor Allem, dass selbst die obigen Erfahrungen dafür sprechen, dass, während von 1000 geimpften Kindern nur 159 an Blattern erkranken, dies im Kreise der Ungeimpften bei 205 der Fall ist, also diese doch eine um 30 Procent grössere Gefahr laufen, pockenkrank zu werden — ein Umstand, der Einen eben nicht in das Lager der Impfgegner zu treiben brauchte.

Insofern aber der obige zu Gunsten der Impfung sich ergebende Ausschlag doch nicht so bedeutend ist, als man sonst bei Vergleichung der Morbidität Geimpfter mit jener der Ungeimpften zu begegnen pflegt, dürften die von Reitz beobachteten Thatsachen wohl den Anlass dazu bieten, dass man in Kinderspitälern bei allen Aufnahmen den Impfzustand notire, um so ähnliche Daten zu gewinnen, wie Reitz für das

Petersburger Spital. Bei dem Stephanie-Kinderspital zu Budapest ist dies schon seit seinem Bestande der Fall, und habe ich in Folge dessen eines der pockenreichsten Jahre, nämlich 1874, ausgewählt und die Impfverhältnisse der daselbst klinisch und ambulatorisch Behandelten — Dank der gütigen Unterstützung des Herrn Direktor Bökay — in dieser Richtung excerpirt.

Da die Anstalt Pockenkranke nicht aufnimmt, ist leider das Material ein gar kleines: unter 708 Kranken zählte man bloss 17 Pockenfälle. Hier folgen nun die Ergebnisse nach der Reitz'schen Aufzeichnung, nur dass wir auch die zweifelhaften Fälle ausweisen:

KLINIK DES BUDAPESTER STEPHANIE-KINDERSPITALS, 1874.

Altersklasse.	Geimpft.			Ungeimpft.			Zweifelhafte Fälle.			Geblattert.			Zusammen.		
	Blattern.	Sonstige Krankheiten.	Zusammen.	Blattern.	Sonstige Krankheiten.	Zusammen.	Blattern.	Sonstige Krankheiten.	Zusammen.	Blattern.	Sonstige Krankheiten.	Zusammen.	Blattern.	Sonstige Krankheiten.	Zusammen.
0- 1 Monat.....	...	...	...	...	3	3	...	...	...	...	...	...	...	3	3
1- 2 "	...	...	...	...	4	4	...	...	...	...	...	...	...	4	4
3- 6 "	...	...	...	1	4	5	...	...	...	...	...	...	1	4	5
6- 9 "	...	3	3	1	6	7	...	1	1	...	...	...	1	10	11
9-12 "	...	3	3	1	4	5	...	...	1	...	...	...	1	7	8
0-1 Jahr.....	...	6	6	3	21	24	...	1	1	...	...	...	3	28	31
1 -1½ Jahr.....	1	11	12	1	10	11	...	...	...	...	...	...	2	21	23
1½-2 Jahre.....	...	17	17	...	5	5	1	...	1	...	1	1	1	23	24
2 -3 "	...	51	51	1	27	28	...	2	2	...	1	1	1	81	82
3 -6 "	1	143	144	4	36	40	...	8	8	...	6	6	5	193	198
Über 6 "	1	298	299	3	30	33	1	14	15	...	3	3	5	345	350
Zusammen.....	3	526	529	12	129	141	2	25	27	...	11	11	17	691	708

NB.—Unter den Blattern sind die Varicellafälle nicht enthalten.

Hier war also unter je 20 ungeimpften Kindern ein blatternkrankes, aber bei den geimpften nur unter je 175!

Ueberdies haben wir aus demselben Epidemiejahre auch noch die reichhaltigeren Aufzeichnungen der Ambulanz einer ähnlichen Zusammenstellung unterzogen, nur dass wir, mit Rücksicht auf die in den Jahresberichten dieser Spitäler übliche Altersclassificirung, die Altersgruppen wie folgt zusammenfassen:

AMBULANZ DES BUDAPESTER STEPHANIE-KINDERSPITALS, 1874.

Alter.	Ungeimpft.		Geimpft.		Geblattert.		Zweifelhafte Fälle.		Zusammen.	
	Behandelt.	Hierunter an Blattern.	Behandelt.	Hierunter an Blattern.	Behandelt.	Hierunter an Blattern.	Behandelt.	Hierunter an Blattern.	Behandelt.	Hierunter an Blattern.
0- 1 Jahr.....	2,102	40	308	3	4	...	5	...	2,419	43
1- 3 Jahre.....	867	33	1,027	4	6	2	4	...	1,904	39
3- 7 "	332	13	1,168	3	5	1	8	...	1,513	17
7-14 "	72	8	759	7	2	...	2	...	835	15
Ueber 14 Jahre ...	8	...	120	1	...	...	3	...	131	1
Zusammen	3,381	94	3,382	18	17	3	22	...	6,802	115

NB.—Unter den Blattern sind Varicella nicht enthalten.

Wir ersehen hieraus, dass im Allgemeinen

von den Ungeimpften an Blattern behandelt wurden 2.8 Procent,
" " Geimpften aber nur..... 0.5 "

Also sprechen auch diese Beobachtungen für den Schutz der Impfung; noch mehr aber, wenn man das erste Lebensjahr ausser Betracht lässt. In diesem Falle ergibt sich, dass an Blattern behandelt wurden:

im Alter von	von Ungeimpften	von Geimpften
1— 3 Jahren	3.8 Procent,	0.9 Procent,
3— 7 "	3.9 "	0.04 "
7—14 "	circa 11 "	0.03 "

Wenn es erlaubt ist, aus so geringen Beobachtungen Schlüsse zu ziehen, so müsste man sagen, dass, bei Ausserbetrachtlassung des ersten Lebensjahres, von den ungeimpften Kindern mehr *als zehnmal* so viel erkrankten, als von den geimpften!

Wenden wir uns aber wieder zur Reitz'schen Tabelle und lassen auch dort die Kranken des ersten Lebensjahres ausser Betracht, so finden wir, dass an Pocken erkrankten

im Alter von	von hundert Ungeimpften	von hundert Geimpften
1—2 Jahren	1.7 Procent,	1.2 Procent,
2—3 "	3.0 "	1.2 "
3—6 "	6.0 "	1.5 "
über 6 "	4.4 "	2.0 "

Reitz hätte daher selbst in seinem eigenen Spitale bemerken müssen:

1) dass in allen Altersklassen die Morbidität der Nichtgeimpften eine bedeutend (zwei- bis dreifach) grössere, als bei den Geimpften sei;

2) dass mit dem fortschreitenden Alter sich die Chancen der Ungeimpften verschlimmern — Umstände, die durchaus nicht gegen den Nutzen der Impfung, am wenigsten aber für eine Schädlichkeit derselben sprechen.

Immerhin bleiben aber die bei Reitz sich zu Gunsten der Impfung ergebenden Ausschläge hinter jenen grossen Erwartungen zurück, die man an die Schutzkraft der Impfung zu stellen berechtigt ist. Seither sind übrigens dreizehn weitere Jahre verflossen. Haben dieselben zu dem gleichen Ergebnisse geführt? Herr Dr. Reitz hat es gewiss nicht unterlassen, seine diesbezüglichen, interessanten Beobachtungen fortzusetzen, und wird dieselben sicherlich wohl auch veröffentlichen? Wir sind sehr begierig, diesen Resultaten zu begegnen!

II. ANALYSE VON VOGT' "FÜR UND WIDER DIE KUHPOCKENIMPFUNG".

Das Vogt'sche Buch ist eines der umfangreichsten Beiträge auf dem Gebiete des mpfstreites. Nach *Lorinser's*, in Sachen der Impfskepsis competenter Ansicht, enthält asselbe das vollständigste Material der Pockenstatistik. "Bei ganz ruhiger Prüfung er vorliegenden Arbeit" — urtheilt derselbe über dieses Buch — "muss jeder nbefangene zu der Ueberzeugung kommen, dass in der Beurtheilung der Impffrage urch fehlerhafte Schlussfolgerung aus mangelhafter Statistik bisher sehr grobe Fehler nd Irrthümer begangen worden sind."[1]

Wir haben es also hier mit einem maassgebenden Erzeugnisse der impfgegnerischen iteratur zu thun[2] und wollen uns deshalb mit demselben eingehend beschäftigen. Vir werden dem Autor Seite für Seite folgen und seine Daten und Schlussfolgerungen uf Schritt und Tritt prüfen, nicht in der Absicht, dieselben unbedingt zu widerlegen, ondern nur, um uns ein vollständiges Urtheil über diese Arbeit zu ermöglichen.

Das Buch zerfällt

1) in eine Einleitung, welche die Entstehungsgeschichte des Buches und zugleich ne Darstellung des Impfstreites in der Schweiz enthält;

2) in eine Besprechung der allein als richtig anerkannten Statistiken von *Müller* Berlin) und *Keller* (Wien);

3) in einen methodologischen Theil, worin zugleich eine neue statistische Methode ur Einsicht in die Schutzverhältnisse entwickelt wird;

4) in eine Sammlung statistischer Beweise (zum Theil nach der neuen, Vogtischen, Iethode zusammengestellt), und zwar wird der Einfluss der Vaccination

a) zeitlich,

b) örtlich,

c) nach dem Lebensalter

n Betracht gezogen. (Eine hier folgende Specialuntersuchung über den Einfluss der mpfung in England und Schottland bildet eine etwas unsystematisch dislocirte Fortsetzung des Theiles *C.*);

5) in die Besprechung der Impfschäden.

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[1] Siehe *Wittelshöfer's Wochenschrift.* 1880.

[2] Vogt selbst will es nicht Wort haben, dass man ihn als Impfgegner betrachte: er sei nur egen den Impfzwang und wolle nur in Epidemiezeiten impfen lassen. Hiermit verträgt sich ber sehr schwer, was er gegen die Nutzlosigkeit der Impfung mit so vielem Fleisse vorgebracht. Vozu denn impfen, wenn man der Ansicht ist, dass die Impfung nur von "unmessbar kleinem influsse auf den Gang der Epidemieen ist" (S. 97)? oder gar (s. S. 49), "dass Geimpfte und ngeimpfte an der Intensität einer Epidemie in ziemlich gleicher Weise Theil nehmen?"; desleichen, wenn man überzeugt ist, dass der Mensch in einer vernünftigen Lebensweise, in allgeeinen hygiënischen Verhältnissen, einen *weit sichereren* Schutz gegen Pocken finde, als in der npfung (s. S. 241), dass diesen Einflüssen gegenüber die Vaccination in ihrer gegenwärtigen andhabung zu einer "nutzlosen Vexation" zusammenschrumpfe, etc. Um bloss gegen die wangsweise Impfung zu sprechen, hat Vogt zu weit ausgeholt: ein ganzes Leben voll der gitation gegen den "Aberglauben" des "Impfdogma", wird durch einen im letzten Momente ervorquellenden, aber auch da noch Zweifel und Widerspruch athmenden Stossseufzer nicht ett gemacht, und so wird Vogt es sich schon gefallen lassen müssen, dass sein Buch nicht unter ie impffreundlichen, sondern unter die impffeindlichen Arbeiten gezählt wird.

1. EINLEITUNG. (Seite 3—30.)

Von den persönlichen Expectorationen absehend, finden wir hier als Hauptgravamina der Impfskepsis vorgeführt, und zwar:

1) die Pockenlethalität der Geimpften und Ungeimpften werde nicht nach Altersklassen berechnet, wodurch man — da Geimpfte und Ungeimpfte ganz verschiedene Altersklassen repräsentiren — Ungleiches mit einander vergleiche. "Unter den zahlreichen (impffreundlichen) Impfschriften existirt nicht eine einzige, welche nicht . . . mittelst falscher Bilanz herausfindet, dass die Kuhpocken-Impfung die alleinige Ursache jener differenten Mortalität (zwischen Geimpften und Ungeimpften) sein müsse." — Da die Widerlegung dieses Argumentes in unserem siebenten Kapitel enthalten war, brauchen wir bei diesem Punkte nicht zu verweilen;

2) die Pockenseuche hat immer, auch schon vor der Vaccination, grosse Sprünge gemacht; der Niedergang der Blattern nach Einführung der Vaccination ist nur zufällig: die Vaccinationsgesetze fallen eben gewöhnlich in die Zeit, wo die Epidemieen ihren Ruhepunkt erreichten, also von da ab nachliessen. Dies lasse sich auch aus der schwedischen Blatternstatistik entnehmen. — Diese Argumentation richtet sich gegen den historischen Beweis: die Gegenbeweise und speciell die Vertheidigung der schwedischen Blatternstatistik siehe im dritten Kapitel dieser Arbeit;

3) die Blattern seien in der postvaccinatorischen Zeit oft stärker als in der antivaccinatorischen aufgetreten. — Ein Eingehen auf die von Vogt angeführten Züricher Daten zeigt zwar, dass die Lethalität daselbst in der postvaccinatorischen Periode *abgenommen* habe (von 20% auf 10%); aber selbst, wenn dem nicht so wäre, beweist die Zu- oder Abnahme der Lethalität im Allgemeinen noch nicht, ob die Geimpften oder die Ungeimpften häufiger sterben — und das ist doch die eigentliche Frage. Wenn Vogt sagt: "Wir wissen nicht, was die Pockenseuche in der Neuzeit wieder zu frischem Aufleben anfacht, wir wissen nur, *dass die Impfung* sie hieran nicht verhindert hat," so verwechselt er, wie so viele Impfgegner, die Möglichkeit, dass trotz Impfung Blattern auftreten, mit der Wahrscheinlichkeit, ob diese Krankheit Geimpfte oder Ungeimpfte häufiger ergreifen werde: nur dieser Umstand ist aber für die Frage des Impfschutzes von Bedeutung (man vergleiche, was wir diesbezüglich gegen Reitz vorgebracht).

Den Rest der Einleitung (Seite 19—30) füllt die Polemik des Autors gegen die Petition der schweizerischen Aerzte-Commission aus.

2. MÜLLER'S UND KELLER'S STATISTIK. (Seite 31—40.)

Wir haben im siebenten Kapitel uns mit diesen beiden Arbeiten eingehend beschäftigt; haben nachgewiesen, dass die erstere, welche die Hälfte der Fälle ausser Rechnung liess, unverlässlich gearbeitet, während die zweite absichtlich auf Irreführung berechnet war. Wenn Vogt diese beiden Arbeiten (Seite 37) "als die unbedingt besten Versuchsobjecte" betrachtet, so fiel er, gleich der ganzen Fachwelt, den Keller'schen Irreführungen zum Opfer; bezüglich der Müller'schen Arbeit hätten ihn freilich die Correcturen *Guttstadt's* aus seiner Vertrauensseligkeit reissen müssen.

3. METHODOLOGISCHER THEIL. (Seite 41—78.)

Diese Abtheilung zerfällt in drei kritische Abhandlungen und in die Darlegung eines positiven Lösungsmodus, d. i. einer neuen Methode:

a) OB DIE SCHUTZKRAFT DER IMPFUNG DURCH EINZELVERSUCH ODER DURCH DIE STATISTISCHE ERFORSCHUNG ZU ENTSCHEIDEN SEI? (Seite 41—54.)

Die Antwort lautet: "Die Einflüsse, welche Ort, Zeit und individuelle Anlage auf die Ansteckungsfähigkeit ausüben, sind noch unbekannt und trüben uns das Resultat eines jeden Einzelversuches, weil wir sie nicht willkürlich eliminiren können. Wollen

ir daher mit den Resultaten unserer wissenschaftlichen Forschungen uns an Volk nd Behörden wenden, so müssen wir einstweilen noch das *Experiment* als Beweismittel ei Seite lassen und *den statistischen Weg verfolgen.*"

Man kann dem nur zustimmen. Im Verlaufe seiner Auseinandersetzungen spricht ber Vogt Manches aus, was nicht mit Stillschweigen übergangen werden darf.

Es ist nicht richtig, dass, wenn irgendwo die Blattern pausiren, daselbst "nach der nsicht der Impfdogmatiker fürchterlich vaccinirt und revaccinirt worden sein muss". ebensowenig, dass beim Aufflackern der Seuche dies "nach dem Impfdogma *nur* in olge periodischer Vernachlässigung des Impfgeschäftes" eingetreten sei (Seite 43). ie Blattern verdanken ihren Ausbruch einerseits persönlichen Dispositionen, andererits äusserlichen Krankheitskeimen: da die Impfung nur die erstere Ursache beseitigt der schwächt, auf die letztere aber ohne allen Einfluss ist, wäre eine solche behauptung widersinnig, dürfte auch bei keinem competenten Vertheidiger der Impfheorie anzutreffen sein.

Was soll es ferner beweisen, wenn Seite 43, internationalen Vergleichungen gegenber, von dem "störenden Einfluss der Oertlichkeit" gesprochen und zu diesem Behufe aus meiner "Statistique internationale des grandes villes") gezeigt wird, dass in erschiedenen Städten zu verschiedenen Zeiten die Blattern sehr verschiedene Grade er Heftigkeit boten? Gewiss ist der geographische Beweis nur ein indirekter: so ange man aber über keinen direkten verfügte, musste man sich mit dem Möglichen egnügen und versuchen, aus den Unterschieden, die sich an verschiedenen Orten rgeben, Schlüsse auf die Ursachen zu ziehen, welche Epidemieen befördern oder erabdrücken. Die beste Correctur für die unläugbar bestehenden "störenden Einüsse" böte die Vergleichung mit der allgemeinen Sterblichkeit: ist diese in den erglichenen Staaten ziemlich gleich und trotzdem die Pockensterblichkeit in den npfenden Staaten bedeutend geringer als in den nichtimpfenden — wie wir dies isher bereits wiederholt beweisen konnten — so wird es sehr wahrscheinlich, dass die mpfung die Ursache dieser Erscheinung sei.[1]

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[1] Vogt führt bei diesem Anlasse mehrere Beispiele an, deren Logik aber manche Lücke ufweist. Um zu beweisen, dass nicht die Vernachlässigung der Impfung die Schuld an den lattern trage, soll die Thatsache dienen, die der Afrikareisende Baker erzählt, wonach nämlich, ls einmal bei den Quellen des Nils die Blattern ausbrachen, die Türken darauf bestanden, sich nd ihre Sclaven zu impfen, Baker aber dies seinen Leuten verbot, und unter denselben auch keiner verstarb. Hier fehlt vor Allem die Hauptthatsache, ob im türkischen Lager Todesfälle orkamen; ferner dürfte die Impfung der Türken höchstwahrscheinlich die Inoculation der lattern, die Variolation gewesen sein: es scheint zum Mindesten nicht sehr wahrscheinlich, ass im Innern Afrika's, an den unteren Quellen des Nils, in einem Türkenlager, Lancette und accinavirus zur Hand gewesen seien. Baker selbst, als guter Engländer, dürfte sammt seinen enossen sogar höchstwahrscheinlich geimpft gewesen sein! — Wenn ferner Curschmann die hatsache, dass in Mainz kein einziges Kind unter zwölf Jahren erkrankt sei, damit erklärt, ass in Mainz alle Kinder gut geimpft seien, hingegen, wenn in Rouen zumeist Kinder ergriffen erden, dies den schlechten — bekanntlich in ganz Frankreich sehr unzufriedenstellenden — mpfverhältnissen dieser Stadt zugeschrieben wird, so finde ich hierin, bei dem gegenwärtigen tande der Wissenschaft, nichts Gezwungenes, noch weniger etwas Widersprechendes. — Wenn ferner die Spanier die Blattern nach Amerika verschleppten und die Eingeborenen der euche erlagen, die fremden Eindringlinge aber am Leben blieben, so dürfte auch dies nicht efremden, wenn man bedenkt, dass in der prävaccinatorischen Periode die Geblatterten die eschützten repräsentirten, und dass nach dem übereinstimmenden Zeugnisse vertrauenswürdiger eitgenossen es um jene Zeit fast Niemand gab, der in seinem Leben nicht die Blattern überanden hätte! (Wir haben vorher jener characteristischen Bemerkung eines ärztlichen chriftstellers aus dem vorigen Jahrhundert gedacht, wonach jene Menschen, die in ihrem eben nicht von Blattern befallen wurden, dieselben gewiss im Mutterleibe durchgemacht haben üssten!)

b) WELCHER GRUNDLAGEN BEDARF ES ZUM STATISTISCHEN NACHWEISE DER SCHUTZKRAFT? (Seite 55–61.)

Die Kenntniss der Anzahl der Lebenden, und zwar nach Altersklassen — weil jede Altersklasse verschiedene Empfänglichkeit für Impfungen aufweist —, sei die Basis aller Vaccinationsstatistik. Da das erste Datum erst seit einigen, die Altersvertheilung aber erst seit 1½ Decennien erhoben wird, müsse man ältere Angaben über Pocken-Mortalität mit grösstem Misstrauen aufnehmen. Auf der anderen Seite müssten aber auch Erkrankte und Verstorbene nach dem Alter und nach dem Impfzustande gekannt sein. "Ein solches Document existirt aber in unserer Literatur noch nicht ..... Die einzige direkte Zählung Geimpfter in einer Stadt hat nur Flinzer in Chemnitz geliefert, allein die Altersklassen fehlen dabei." Schliesslich schlägt Vogt auch noch vor, die seit der Impfung verstrichene Frist zu constatiren, um so Anhaltspunkte über die Dauer der Schutzkraft zu gewinnen.

Allen diesen Ausführungen kann man nur zustimmen. Richtig ist auch, was Vogt über die Schwierigkeit vorbringt, zu constatiren, ob Jemand geimpft sei. Mit Documenten sei dies selten zu belegen, die Aussage der Patienten sei zweifelhaft und Impfnarben bewiesen noch nicht, ob man mit Erfolg geimpft worden sei. Zustimmen wird man ihm ferner müssen, wenn er jener möglichen Verfälschung der Vaccinationsstatistik gedenkt, die durch die tendentiöse Zu- oder Abrechnung der zweifelhaften Fälle eintreten könnte. Unbillig aber ist es von ihm, die Schwierigkeiten, welche sich der Classificirung nach Geimpften und Ungeimpften in den Weg stellen, dem "Impfdogma" in die Schuhe zu schieben, zu behaupten, dass "allein die Leidenschaft, mit welcher das Dogma seine schwankende Position wieder in ein stabiles Gleichgewicht zu bringen sucht, diese Unterscheidung in so tendentiöser Weise verwirrte, dass Niemand mehr durch das getrübte Wasser den Boden sehen könne." Die Schwierigkeiten, die sich hier bieten, sind objective, nicht subjective, was Vogt um so eher zugeben müsste, als, trotz aller Philippiken gegen das "Impfdogma", er, der doch nicht durch die Brille desselben sieht, ebenfalls keine Lösung dieser Schwierigkeiten findet.[1]

c) INWIEFERN ENTSPRICHT UNSER WISSENSCHAFTLICHES MATERIAL DEN ANFORDERUNGEN DER STATISTIK? (Seite 61—69.)

Sehr wenig. Flinzer's, Müller's und Keller's Aufnahmen blieben noch immer die werthvollsten. Wünscht man aber auch Aufklärung über die Dauer der Schutzkraft (durch Angabe der seit der Impfung verstrichenen Zeit), so finde man gar kein entsprechendes Material.

d) NEUE METHODE. (Seite 69—78.)

Diese ("Pockentafel" benannt) läuft eigentlich nur darauf hinaus, dass berechnet wird, wie viel Procente die verstorbenen Ein-, Zwei- und Dreijährigen u. s. w. von der Gesammtheit der Verstorbenen betragen. Eine solche Berechnung bietet aber nicht einmal darüber Auskunft, ob aus der einen oder der anderen Altersklasse mehr Personen verstorben seien, denn wenn es in A zweimal so viel Einjährige gibt, als in

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[1] Es ist zu begreifen, wenn die Impfvertheidigung je präcisere Angaben über das *Maass* des Impfschutzes gewinnen will. Wollte man sich aber mit dem Beweise der blossen Thatsache, ob Impfung schützt oder nicht, begnügen, so würde manche Schwierigkeit entfallen. Es genügte dann, die Erkrankten nach irgend einem brauchbaren (wenn auch nicht ausnahmslos verlässlichen) Symptome zu classificiren, sei es z. B. nach der Existenz von Narben. Theilt man nun sämmtliche Erkrankten in zwei Gruppen, je nachdem sie Narben (eventuell auch nur eine einzige) hatten oder nicht, so wird unbedingt die überwiegend grosse Mehrzahl der Geimpften in die erste, die überwiegend grosse Mehrzahl der Ungeimpften in die zweite Gruppe fallen, und, falls die Impfung wirklich Schutz bietet, wird die erste Gruppe, auch wenn dieselbe mit erfolglos Geimpften versetzt ist, unbedingt günstigere Resultate aufweisen müssen.

so wird es, bei *gleicher Mortalität*, auch zweimal so viel Verstorbene dieser Alters-
asse geben: die Behauptung also, dass in A die Sterblichkeit des ersten Lebensjahres
eimal grösser sei (d. h., dass von hundert Lebenden in A zweimal mehr stürben),
re falsch. Absolut unerfindlich bleibt es aber, wie eine solche Pockentafel über die
ssere Sterblichkeit der Ungeimpften oder Geimpften Aufschluss geben soll, nachdem
n doch die Anzahl der in je einer Altersklasse lebenden Geimpften oder Ungeimpften
ht kennt! Die ganzen umständlichen "Pockentafeln", die Vogt zur Begründung
Nutzlosigkeit der Impfung anführt, sind demnach werthlos.

Eine mathematische Widerlegung dieser "Pockentafel"-Berechnungen hat Escher
der *Zeitschrift für schweizerische Statistik* (1877) geboten. Vogt selbst scheint
wischen die Werthlosigkeit seiner Berechnungsweise eingesehen zu haben, denn in
ner zweiten Streitschrift ("Der alte und der neue Impfglaube") verlässt er dieselbe
lkommen. In der uns vorliegenden Arbeit bildet dieselbe aber einen Eck- und
undstein seiner Argumentation, auf welchen er sich in einer grossen Anzahl von
llen stützt. Diese Beweise werden also vorkommenden Falls unter Hinweis auf die
richtige Berechnung, die denselben zu Grunde liegt, einfach bei Seite gelegt werden
nnen.

4. STATISTISCHE BEWEISE. (Seite 79—228.)

Der Einfluss der Impfung auf die Blattern wird in drei Richtungen untersucht,
nlich zeitlich, örtlich und nach dem Lebensalter.

α) EINFLUSS DER VACCINATION AUF DAS ZEITLICHE AUFTRETEN DER POCKENEPIDEMIEEN.
(Seite 78—118.)

Alle epidemischen Krankheiten kommen und gehen; so auch die Blattern. Aber
von den letzteren wird behauptet, dass deren Gehen einem Präservativ, nämlich
Impfung, zuzuschreiben sei. Der Beweis hiefür mangle gänzlich. — Diesbezüglich
iehen wir uns auf unser drittes Kapitel (von dem historischen Beweise), wo nachge-
sen wurde, dass auf diesem indirekten Wege die Schutzkraft der Impfung sich nur
wach, vielleicht gar nicht beweisen lässt. Vogt und alle Impfgegner haben deshalb
nlich leichtes Spiel, wenn sie fortwährend direkte Beweise verlangen, welche auf
sem (historischen) Wege gar nicht zu finden sind. Vogt übergeht nun auf die für
zelne Länder vorgebrachten Argumentationen der Impfvertheidigung. Wir wollen
— soweit er statistisches Material in's Treffen führt — Schritt für Schritt zu
gen versuchen.

KOPENHAGEN (Seite 82): Von 1750—1801 starben jährlich 250 an Pocken, von 1801
811 nur 16, während die Impfung erst im Jahre 1810 eingeführt wurde; "hingegen
gt die Pockensterblichkeit im Jahre 1872 wieder in grellem Maasse (219 Fälle) ohne
hweisbare Veränderung der Impfverhältnisse."— Gewiss können Blattern auch ohne
pfung aufhören und trotz Impfung ausbrechen, und liegt hierin eben die Schwäche
geographischen Beweises. Trotzdem sprechen die Erfahrungen von Kopenhagen noch
ner eher zu Gunsten als zu Ungunsten des Impfschutzes; freilich dürfen dieselben
ht so gegeben werden, wie bei Vogt. Vor Allem ist zu bemerken, dass in Kopenhagen
Impfung bereits von 1801 bis 1810 prakticirt wurde, und zwar in sehr bedeutendem
asse. (Siehe Lotz, Seite 63, wonach von 1802 bis 1809 auf 100 Geburten 34
pfungen entfielen.) Die vaccinatorische Periode beginnt also schon mit 1802.
ner ist bei Vogt eine Reihe von nicht weniger als vierzig Jahren aus der nachvacci-
orischen Zeit ausgelassen worden, nämlich die Jahre von 1828 bis 1867. Schliesslich
gisst Vogt, dass die Bevölkerung Kopenhagens in der Mitte des vorigen Jahrhunderts
a 60,000, im Jahre 1872 aber an 200,000 betragen hat, dass also die Todesfälle
ht in ihren absoluten Ziffern, sondern in Procenten der Bevölkerung auszudrücken
d. Wir wollen diesen Fehlern abhelfen und lassen hier die Blatternsterblichkeit
penhagens von 1750 bis 1872, und zwar mit Hinzufügung der auf 100,000 Einwohner

berechneten Verhältnisszahlen, folgen, wobei die von Vogt ausser Rechnung gelassenen (für die These des Impfschutzes zumeist sehr günstigen Perioden!) mit einem Sternchen bezeichnet sind:[1]

Periode	Bevölkerung	Blattern-Todesfälle	Auf 100,000 Einw. jährlich
1750—1759	60,000	4,059	676.5
1760—1769	65,000	2,208	339.7
1770—1779	70,495 (1769)	1,288	182.8
1780—1789	78,451 (Mittel)	2,068	263.5
1790—1800	83,604 (1796)	2,686	321.3
im Durchschnitt jährlich			356.7
1801—1810	95,876 (Mittel)	644	66.0
1810—1819	104,790 (Mittel)	—	—
1820—1829	112,420 "	116	10.3
1830—1839	119,442 "	569	47.6*
1840—1849	126,787 "	247	19.5*
1850—1859	143,591 (1855)	4	0.3*
1868—1869	177,368 (Mittel)	5	0.3*
1870—1872	181,291 (1870)	224	12.4
im Durchschnitt jährlich			12.9

Es sind also an Blattern verstorben auf 100,000 Einwohner jährlich:

vor Einführung der Impfung 357,
während Einführung der Impfung 66,
nach Einführung der Zwangsimpfung aber nur ..... **13!**

Die Beobachtungen von Kopenhagen bilden demnach keine Verläugnung, sondern geradezu eine Glorificirung des "Impfdomas".

MARSEILLE, 1828 (Seite 84): Hier behauptet Vogt, dass die Epidemie mehr Vaccinirte als Nichtvaccinirte ergriffen hätte. Dieses Ergebniss beruht aber einfach auf einer willkürlichen *Schätzung* über die Anzahl der Geimpften und Ungeimpften, die in Marseille zur Zeit der Epidemie gelebt haben mochten! Von impffreundlicher Seite hat man für Marseille ähnliche Schätzungen versucht und ist so zu dem Resultate gelangt, dass mehr Ungeimpfte ergriffen wurden. Wir glauben am besten zu thun, wenn wir solchen, auf willkürlichen Schätzungen beruhenden Beweisen, ganz aus dem Wege gehen.

PARIS, 1828—1836, 1860—1877, FRANKREICH, 1869—1875 (Seite 86—91): Die angeführten Daten sind für die Streitfrage ganz irrelevant; auch Vogt zieht aus denselben keine Schlüsse.

SCHWEDEN (Seite 91—93): Was Vogt gegen die schwedische Blatternstatistik vorbringt, ist in unserem dritten Kapitel theils gewürdigt, theils widerlegt worden. Die Stellung, die Vogt dieser, in ihrer Art einzigen Blatternstatistik, gegenüber einnimmt, ist im Ganzen nicht objectiv zu nennen: neben einigen berechtigten Einwänden, ist doch im Ganzen das Bemühen zu erkennen, die überaus zu Gunsten der Impfung sprechenden Ergebnisse dieser Statistik in tendentiöser, das ist impfgegnerischer, Weise zu devalviren.

WÜRTTEMBERG (Seite 94): Ohne Belang. Wir begegnen hier wieder nur den bekannten Causalitätszweifeln und der Hervorhebung des Umstandes, dass die Blattern schon zehn Jahre vor Einführung der Zwangsimpfung in der Abnahme begriffen waren. Ist es aber denn wirklich ein so zwingender Schluss, dass, weil die Einführung der Zwangsimpfung in die Zeit der abnehmenden Epidemie fiel, es ausgeschlossen sei

[1] Quellen: für 1750—1850 das englische Blaubuch, Seite 171; von 1850—1859 brieflich Mittheilungen des Herrn Markus Rubin, Direktor des communalstatistischen Bureaus in Kopenhagen; von 1868—1872 Vogt; für 1860—1867 fehlen die Angaben.

dass die Impfung ein Wiederaufleben der Epidemie verhindern könne? Man vergleiche doch die Pockensterblichkeit Württembergs mit jener der lässiger impfenden Staaten, oder die Pockensterblichkeit dieses Landes vor und nach Einführung der Zwangsimpfung, und man wird den grossen Unterschied erkennen.[1]

OESTERREICH (Seite 95): Keine Daten und keine Argumente.

WIEN (Seite 95—97): Die Pockensterblichkeit von 1828 bis 1877 wird in drei Perioden getheilt, und zwar entfielen jährlich auf 100,000 Einwohner:

von 1828—1853	57.7	Todesfälle,
" 1854—1870	40.6	"
" 1871—1877	183.1	"

Die grösste Steigerung zeige sich also eben in der letzten Periode. "Die enorme Steigerung der Krankheit vom Jahre 1871 und ihr jäher Abfall von 1874 an, beweisen jedem Vorurtheilslosen deutlich genug, dass bei diesen Epidemiezügen die Impfung höchstens einen unmessbar kleinen Einfluss ausgeübt haben kann."—Für uns beweisen diese Daten nur so viel, dass mit der historischen Argumentation keine zwingenden Beweise beschafft werden können. Will man aber schon auf diesem Wege fortschreiten, so darf man die Perioden nicht willkürlich (die erste 23, die zweite 14, die dritte 7 Jahre umfassend) bestimmen, auch keine tendentiöse Auswahl in den Grenzbestimmungen walten lassen, indem man die jüngste Periode — um die Mortalität recht ungünstig zu gestalten — mit der seit Jahrzehnten furchtbarsten Epidemie der siebenziger Jahre beginnen lässt, hingegen aus der älteren Periode die noch stärkeren Epidemiejahre (1806: tausend Todesfälle auf 100,000 Einwohner, und im Jahre 1800 420) ausser Rechnung lässt! Die einzig annehmbare Form des historischen Beweises wäre die Vergleichung der prä- mit der post-vaccinatorischen Zeit, also des achtzehnten Jahrhunderts mit dem neunzehnten. Für die prävaccinatorische Zeit enthält Vogt keine Angaben. Versucht man aber eine Zusammenstellung für die Zeit vor und nach Einführung des (indirekten) Impfzwanges, so gelangt man zu folgendem, wieder für den Schutz der Impfung sprechenden, Ergebnisse:

Es starben in Wien auf 100,000 Einwohner jährlich an Pocken:

vor Einführung des Impfzwanges (1806, 1810, 1828—36)	214,
nach " " " (Vogt's 35 Jahre)	63.

PREUSSEN UND BERLIN (Seite 97—107): Nach einer berechtigten theoretischen Bemängelung, dass es nämlich richtiger sei, die Zahl der Blatterntodesfälle zur Zahl der Lebenden, als zur Zahl der Verstorbenen zu vergleichen, bietet uns Vogt die Statistik der Blatternsterblichkeit

von Brandenburg für	1776, 1780, 1789—1798, 1810—1815
" Berlin für	"
und von zehn Landestheilen (inclusive Berlin und Brandenburg)	" " "

Vogt nennt das aus dieser Aufstellung sich ergebende Resultat ein ungemein instructives. Gewiss ist es dies nach einer Richtung hin: wenn man nämlich untersuchen will, ob sich die Anzahl der Pockentodesfälle seit Verbreitung der Impfung vermindert habe oder nicht. Nun wurde die Impfung in Preussen zwar erst im Jahre 1835 gesetzlich eingeführt; aber schon durch Ministerial-Rescript vom 13. August 1810 wurde, bei Ausbruch mehrerer Fälle, Impfung vorgeschrieben. Nach *Hufeland*

[1] Leider ist selbst aus den ausgezeichneten Werken von Heim und Cless eine zusammenhängende Statistik der Pockentodesfälle in Württemberg nicht herstellbar. Die Nachweisungen der "Württembergischen Jahrbücher" beziehen sich wieder nur auf die jüngste Zeit. Man vergleiche übrigens, was wir über die Abnahme der Pockensterblichkeit in Württemberg Reitz gegenüber vorbrachten.

wurden schon von 1801 bis 1810 nicht weniger als 600,000 Menschen geimpft und nach *Augustin* in dem einen Jahre 1816 : 400,000.[1] Die Impfzeit beginnt also für Preussen mit 1810. Man darf nun begierig sein, zu erfahren, wie sich die Pockentodesfälle vor 1810, von 1810 bis 1835 und nach 1835 gestalteten. *Dieterici*, dem auch Vogt seine Angaben entnimmt, gibt uns hierüber folgende Aufklärung :[2]

In den zehn Landestheilen starben an Pocken :

a) PRÄVACCINATORISCHE ZEIT:
1776 und 1780 : je Einer von 438 ;

b) NACH VERBREITUNG DER IMPFUNG:
1810—1815 : je Einer von 968,
1816—1831 " " " 4844 ;

c) NACH GESETZLICHER EINFÜHRUNG DES IMPFZWANGES:
1835—1846 : je Einer von 3986,
1847—1850 " " " 7303.

Also starben in der Zeit nach Einführung des Impfzwanges *neunmal, bez., in den letzten drei Jahren, achtzehn Mal weniger an Pocken als vorher !*

Als Vogt sein Werk schrieb, waren ihm übrigens auch schon die ersten Ergebnisse des eigentlichen Impfzwangsgesetzes vom Jahre 1874 bekannt. Wir haben dieselben an anderer Stelle mitgetheilt ; was kann Vogt dazu sagen, dass seither die Blattern in Preussen fast ganz aufhörten, dass gegenwärtig — und zwar schon seit anderthalb Decennien ! — im ganzen Königreich Preussen kaum so viel Menschen an Pocken sterben, wie in einer einzigen unter den lässiger impfenden Grossstädten der Nachbarstaaten ?

Aber nicht solche selbstverständliche Erwägungen sind es, welche Vogt die von ihm angeführten Daten für ausnehmend instructiv halten lassen ; sie sind ihm dies nur, weil sie Anlass zu der etwas merkwürdigen Frage geben : "Warum starben denn in der gänzlich ungeimpften Mark Brandenburg von 1789—1798 weniger Menschen an Pocken als 1871 in Berlin, wo die grosse Zahl der Einwohnerschaft *lege artis* geimpft war ?"[3]

Die Antwort auf diese sehr überflüssige Frage ist sehr einfach : weil 1789—1798 keine Blatternepidemie bestand, hingegen 1871 eine beispiellos verheerende Epidemie in Berlin wüthete. Es ist im Allgemeinen ein zu missbilligendes Vorgehen, ein epidemiefreies Decennium des einen Landes mit einem verheerenden Epidemiejahr eines anderen Landes zu vergleichen. Wir dürfen diese Missbilligung um so getroster aussprechen, als Vogt an einer Stelle, wo ihm bei Cless ein angeblich ganz ähnlicher, in Wirklichkeit total entgegengesetzter und sehr loyaler Beweisgang begegnet, mit seinem abfälligen Urtheil hierüber durchaus nicht hinter dem Berge hält.[4]

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[1] Siehe *Dieterici's* "Mittheilung des statistischen Bureau's." 1857. Seite 312.

[2] Ebendaselbst. Seite 333.

[3] Nur nebenbei soll bemerkt werden, dass ich die für Brandenburg aus den Jahren 1789—1798 angegebenen Mortalitätsziffern bei Dieterici *nicht* finden konnte. Auf Seite 322 sind — ebenso wie für alle übrigen Provinzen, so auch für Brandenburg — die Jahre 1789—1798 ausgelassen.

[4] Man vergleiche Seite 117: "*Cless* nimmt die Pockenmortalität Preussens aus einer Epidemie (1853 und 1854), wie sie daselbst seit zwanzig Jahren nicht vorgekommen war, stellt sie der Pockenmortalität von Württemberg aus einem ganzen Jahrzehnt und einer ganz anderen Zeitperiode (1858—1868) gegenüber, erhält dabei für Preussen eine drei—viermal grössere Pockensterblichkeit als in Württemberg, und fragt dann nach der Ursache dieses statistischen Wechselbalges. Die Antwort kann nun jeder Kenner des Impfdogmas zum Voraus geben: 'In Württemberg ist Impfzwang, in Preussen nicht und seine Bevölkerung deshalb nur unvollkommen geimpft. Ein Wink, was wir zu erwarten hätten mit Aufhebung des Impfzwanges.' Sollte — fährt Vogt

Wenn Vogt erkennen will, ob die Impfung die Berliner Bevölkerung vor Blattern geschützt habe oder nicht, warum hält er sich nicht—da die Morbidität und Mortalität der Ungeimpften in Folge des Problems der lebenden Gesammtheiten nicht berechnet werden kann—zum Mindesten an die Spitalslethalitäten der grossen Berliner Epidemie vom Jahre 1871? Er hätte dann gesehen (vergl. unsere diesbezüglichen Nachweisungen bei Reitz), wie viel häufiger die Ungeimpften auch in Berlin von den Blattern befallen wurden.

Treffend ist übrigens auch, was hiezu *Lotz* (Seite 44) bemerkt: Abgesehen davon, dass wenn in Berlin die grosse Mehrzahl der Einwohnerschaft *lege artis* geimpft, so s. Z. in der Mark die grosse Mehrzahl der Bevölkerung *lege naturæ* bereits geblattert hätte, weist er noch darauf hin, dass es ein methodischer Fehler sei, wenn man die Seuchenmortalität einzelner Jahre einerseits in ländlichen, anderseits in städtischen Bevölkerungen vergleicht. "Eine gleich grosse Zahl Empfänglicher wohnt in einer Stadt zusammen und wird im Laufe *eines* Jahres von der Seuche erreicht, während sie auf dem Lande zerstreut ist und daher (besonders bei dem mangelhafteren Verkehr früherer Jahrhunderte) erst im Laufe mehrerer Jahre erreicht wird. Eine Stadt liefert daher unter sonst gleichen Umständen, besonders bei sonst gleicher Empfänglichkeit für eine Seuche, leicht einzelne hohe Jahresziffern, während ein Land im einzelnen Jahre nur theilweise befallen, dafür aber auch nicht leicht in allen Theilen ganz frei ist."

Der Hinweis, dass in Berlin so fleissig geimpft werde und deshalb eigentlich die Blattern unmöglich sein sollten, wird durch Vogt's eigene Berechnungen entkräftet. Er zeigt Seite 107, dass auf je hundert Kinder im ersten Lebensjahre in Berlin

im Jahre 1863.....105,
" " 1864.....169 Impfungen entfielen,

in den folgenden Jahren aber bedeutend weniger, nämlich:

1865	**58**	1868	92
1866	63	1869	93
1867	83	1870	**48** (!)

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fort — man nicht Purzelbäume schlagen in der Luft?" Vogt brauchte aber nur die Namen zu ändern, nur statt Württemberg und Preussen, Berlin und Kurmark zu setzen, und die ganze Persiflage fällt auf seine eigene Argumentation zurück. Das Komischeste — vielleicht das Traurigste — an der ganzen Sache ist aber hiebei, dass Cless, weit entfernt, sich des Vergehens einer solchen tendentiösen Auswahl schuldig zu machen, im Gegentheile mit übermässiger Strenge gegen seine eigene Ansicht vorgeht: er vergleicht nämlich nicht die epidemiefreie Periode des Impfzwangstaates mit der epidemischen eines zwangsfreien, *sondern geht gerade umgekehrt vor*, wählt also für sich die ungünstigsten Chancen und überlässt dem Gegner die günstigsten. Es heisst nämlich Seite 64: "In dem Jahrzehnt, welches die *stärkste bis dahin vorgekommene Pockenepidemie* (1864—1867) *einschliesst*, starb (in Württemberg) jährlich im Durchschnitte von 19,294 Einwohnern Einer an Pocken.... Von der preussischen Civilbevölkerung....kommt in dem 45(!)-jährigen Zeitraum 1816—1860 auf 5600 Menschen jährlich ein Todesfall an Pocken, also das Drei- bis Vierfache unserer Württembergischen Pockensterblichkeit im Jahrzehnt 1858—1868. In Württemberg ist Impfzwang, in Preussen nicht....etc." Die ganz überflüssige Strenge, die Cless hier gegen die Impftheorie bekundet, ist augenfällig: gerechterweise hätte er die gleiche 45jährige Periode Württembergs mit der Preussens vergleichen müssen, und in diesem Falle hätte sich für Württemberg ein noch günstigeres Resultat ergeben. Nebenbei vergleicht Cless dann auch das grösste Epidemiejahr Württembergs (1865) mit dem grössten Epidemiejahr Preussens (1853–'54). Die Behauptung Vogt's, dass Cless die Württembergische Blatternmortalität eines Jahrzehntes mit einer solchen Epidemie Preussens (1853–'54) verglichen hätte, wie sie daselbst seit zwanzig Jahren nicht vorgekommen war, ist also total irrig: es liegt hier, wenn man schon keine absichtliche Verstümmelung der Aussagen annehmen will, zum Mindesten eine bedauerliche Eilfertigkeit vor.

Die Ansicht der Impfer, dass eine grosse Anzahl Ungeimpfter eine grössere Ausbreitung einer ausbrechenden Epidemie befürchten lasse, wurde also durch die grossen Dimensionen, welche die Epidemie der Jahre 1871—1874 angenommen, eher bestätigt als widerlegt.

Vogt weist bei diesem Anlasse auf die hohen Mortalitäten hin, welche die jüngste Pandemie in fünfzehn Städten verursachte und welche höher sind, als die Mortalitäten, die Kussmaul für die prävaccinatorische Zeit angibt. Es wurde aber bereits darauf hingewiesen, dass, um den Einfluss der Impfung zu negiren, bewiesen werden müsste — nicht ob gegenwärtig grössere Epidemieen möglich sind, als im vorigen Jahrhundert, sondern —, ob diese jetzigen Epidemieen mehr Geimpfte oder mehr Ungeimpfte hinwegraffen! Ueberdies wurde auch darauf hingewiesen, dass in fast allen der von Vogt angeführten Städte es um die Impfung schlecht bestellt ist.

BAYERN (Seite 107—112): *Bohn* stellt die Blatternstatistik Bayerns als die günstigste der ganzen Welt dar. Wenn Vogt in den einleitenden Worten dieses Abschnittes bemerkt, dass nach den Ergebnissen in den bereits besprochenen Ländern der Glaube an solche Behauptungen schwach geworden sei, müssen wir diese captatio malevolentiæ gleich von vornherein ablehnen. Unseres Wissens wurde in dem Vogt-ischen Buche bisher zwar recht viel perorirt, aber noch nicht das Geringste gegen die Schutzkraft der Impfung bewiesen. Wohl muss man aber mit berechtigter Neugierde jener Argumentation entgegen sehen, durch welche Vogt es unternehmen will, die Nutzlosigkeit der Impfung in diesem Musterlande des Impfschutzes nachzuweisen. Man mag aber die Seiten 107—112 wie oft immer durchlesen, man findet daselbst nicht den Schatten eines Beweises. Im Gegentheil wird zugegeben, dass Bayern wirklich sehr geschützt dastehe. Seite 109 droht wohl der Autor, dass er noch zeigen werde, wie andere Länder, welche gar nicht, oder sehr mangelhaft impfen, noch viel pockenfreier seien, als Bayern. Nun, es mag ja Länder geben, wo Pocken gar nicht vorkommen, wie es Länder gibt, die die asiatische Pest oder das gelbe Fieber Amerikas nicht kennen. Für europäische Verhältnisse wäre ein solcher Beweis gewiss sehr interessant. "Schade," meint Lotz, "dass Vogt die Erfüllung dieses Versprechens schuldig geblieben ist und schuldig bleiben wird."[1]

GROSSSTÄDTE (Seite 112—113): Was soll eine Zusammenstellung der Blattern-mortalität in siebenzehn Grossstädten für die Schutzkraft der Impfung beweisen? Zum Mindesten hätte eine solche Zusammenstellung doch nach gut und schlecht impfenden Städten aufbereitet sein müssen. Vogt gibt aber die blossen Zahlen und fügt dann hinzu, es bedürfe eines "förmlichen logischen Eiertanzes, um die merkwürdigen Sprünge der Seuche nur den Impfverhältnissen anzuhängen." Wer aber wollte dies auch thun? Es ist eine Insinuation, eine solche Ansicht der Impfvertheidigung in die Schuhe zu schieben. Aber wie, wenn Vogt's Zasammenstellung, trotzdem dieselbe bloss 6—14 Jahre umfasst, doch schon einige Anhaltspunkte für eine vernünftige Untersuchung böte? Versuchen wir doch die Classification nach gut und schlecht impfenden Städten:

In Frankreich, Belgien, in den Niederlanden, Oesterreich und Ungarn besteht keine Zwangsimpfung; wir werden demnach die Städte dieser Länder zu den lässig impfenden rechnen; in Bayern, England und Schweden besteht der Impfzwang seit langer Zeit: München, London und Stockholm werden in Folge dessen zu den gut impfenden Städten gerechnet werden. Da in Preussen der Impfzwang erst seit dem Jahre 1874 in voller Strenge eingeführt ist, werden wir die preussischen Städte (Berlin, Köln, Breslau) von dieser Zeit ab in die gut, für die vorhergehenden Jahre in die mittelgut impfende Klasse einreihen. Das Gleiche gilt auch für Hamburg; Frankfurt

[1] Möglich, dass Vogt den Seite 128 erwähnten vagen Reisebericht meint, wonach in Baku am caspischen Meere die Pocken unbekannt zu sein scheinen.

a. M. hingegen war (nach Dr. Marcus in Spiess' "Frankfurt a. M. in seinen hygiënischen Verhältnissen") stets eine gut impfende Stadt.

Wir gelangen in Folge dessen zu folgender Classification:

POCKENMORTALITÄT AUF 100,000 EINWOHNER.

a) LÄSSIG IMPFENDE STÄDTE:

Paris	in 14 Jahren	960	Fälle	=	jährlich	69	zusammen 1031.
Wien	" 14 "	1324	"	=	"	94	
Prag	" 12 "	1038	"	=	"	88	
Triest	" 12 "	989	"	=	"	82	
Budapest	" 6 "	919	"	=	"	153	
Lüttich	" 14 "	744	"	=	"	53	
Haag	" 5 "	1714	"		"	343	
Rotterdam	" 10 "	1492	"	=	"	149	

Demnach durchschnittlich pro Jahr ..... 129 Fälle.

b) MITTELGUT IMPFENDE STÄDTE:

Berlin	1866—1873 in 8 Jahren	901	=	jährlich	113	zusammen 666.
Köln a. Rh	1866—1873 " 8 "	459	=	"	57	
Breslau	1866—1873 " 8 "	861	=	"	108	
Hamburg	1871—1873 " 3 "	1164	=	"	388	

Demnach durchschnittlich pro Jahr ..... 166 Fälle.

c) GUT IMPFENDE STÄDTE.

London	1864—1877 in 14 Jahren	595	=	jährlich	43	zusammen 163.
München	1869—1877 " 9 "	207	=	"	23	
Stockholm	1864—1873 " 10 "	655	=	"	65	
Frankfurt a. M.	1866—1872 " 7 "	192	=	"	28	
Berlin	1874—1877 " 4 "	9	=	"	2	
Köln a. Rh.	1874—1877 " 4 "	5	=	"	1	
Breslau	1874—1877 " 4 "	2	=	"	½	
Hamburg,	1874—1877 " 4 "	2	=	"	½	

Also durchschnittlich pro Jahr ..... 20 Fälle.

Ohne allen "logischen Eiertanz" also, rein nach der Eingebung des gesunden Menschenverstandes, finden wir in den eigenen Angaben Vogt's, welche die Schutzkraft der Impfung bekämpfen sollten, folgende Ergebnisse, welche, wenn auch nicht Schritt für Schritt den Erwartungen entsprechend, so im Ganzen doch eine nachdrückliche Rechtfertigung der Impftheorie enthalten:

Es starben jährlich an Pocken unter je 100,000 Einwohnern

	in 8 lässig	impfenden	Grossstädten	129,
	" 4 mittelgut	"	"	166,
hingegen	" 8 *gut*	"	" nur	**20!**

b) EINFLUSS DER VACCINATION AUF DAS RÄUMLICHE AUFTRETEN DER POCKEN-EPIDEMIEEN. (Seite 118—156.)

FRANKREICH (Seite 122—128): Auf Grund einer von *Tardieu* mitgetheilten Tabelle über die Impfungen und Pockentodesfälle in den französischen Departements stellt Vogt dieselben in zwei Reihen: gut und lässig impfende, und untersucht die Morbidität, Mortalität und Lethalität dieser zwei Reihen. Wir begegnen hier endlich Vogt auf jenem Wege, den wir seinen eigenen, oft unverständlichen geographischen Vergleichungen gegenüber, als den einzig richtigen bezeichnen mussten. Die Ergebnisse sind eigenthümlich: die Morbidität betrug in den gutimpfenden Departements zwar nur

[1] Es sind hierbei folgende bei Vogt eingeschlichene Rechnungsfehler corrigirt worden:

München 1871	88 Fälle statt	1
" 1872	63 " "	93
Hamburg 1871	1029 " "	1544

60 Fälle auf 100,000 Einwohner, gegen 69 in den schlechtimpfenden ; aber die Mortalität 7¼ gegen 5¾, die Lethalität 11.9 pCt. gegen 8⅓ pCt.

Ich habe mich der Mühe nicht verdriessen lassen, Vogt's Angaben Post für Post mit jenen Tardieu's zu vergleichen, und habe hierbei die Ziffern richtig befunden. Tardieu selbst hat mehrere Departements nicht angeführt ; diese habe ich nach *Block's* Annuaire nachgetragen. Die von Vogt zusammengestellten Reihen ändern sich in Folge dessen in nachstehender Weise :

IMPFUNGEN UND POCKENFÄLLE IN FRANKREICH, 1859.

(*Nach Tardieu.*)

I. GUTIMPFENDE DEPARTEMENTS.

Departements.	Volkszahl.	Geburten.	Vaccinationen.	Pockenfälle.	Auf 100 Geburten kommen Vaccinationen.	Von 100,000 Einwohnern erkrankten an Pocken.
Summe von 29 Departements. (Nach Vogt.)	10,862,344	281,713	226,118[1]	6,609	80.65	60.34
Hiezu :						
Gers	304,494	5,781	4,164	37		
Lot et Garonne	340,041	6,326	4,936	4		
Zusammen	11,506,879	293,820	235,218	6,650	80.06	57.79

Auf 100 Geburten kommen 80.06 Vaccinationen, auf 100,000 Einwohner 204.

II. LÄSSIGER IMPFENDE DEPARTEMENTS.

Summe von 19 Departements. (Nach Vogt.)	8,200,381	240,052	125,664	5,665	52.35	69.08
Hiezu:						
Charente-Inférieure	474,828	11,346*	7,036	201		
Cher	314,844	10,582	1,255	12		
Côtes-du-Nord	621,573	20,556*	12,334	98		
Creuse	278,889	6,695	4,060	11		
Finistèrre	606,552	22,937*	9,218	56		
Gard	419,697	12,220	2,261	75		
Marne-haute	372,050	5,267	3,444	3		
Oise	396,085	9,909*	4,683	33		
Pyrenées-hautes	245,856	5,688	1'910	14		
Seine-et-Marne Yonne	341,382	9,171*	5,989	156		
Zusammen	12,272,137	354,423	177,854	6,324	53.00	51.53

Auf 100 Geburten kommen 53 Vaccinationen, auf 100,000 Einwohner 144.

[1] Bei Vogt irrthümlich 266,118.

Selbst diese Correction ergibt also noch ein impffeindliches Resultat. Beruhte also der Beweis von der Schutzkraft der Vaccination nur auf den Daten Tardieu's und nur auf den Erfahrungen des einen Jahres 1859, so wäre es freilich um dieselbe schwach bestellt. Zum Glück ist dies nicht der Fall : neben den massenhaften statistischen Aussagen, die zu Gunsten der Schutzkraft abgegeben wurden, mag diese eine, halb belastende Aussage ruhig bestehen. Indem wir dieselbe demnach unparteiisch registriren, möge aber noch erwähnt sein, dass Vogt auf gleichem Wege auch für Oesterreich zu einem gleich impffeindlichen Resultate — mit dem wir uns alsbald zu beschäftigen haben werden — gelangt ist, dass aber hier die Kenntniss der herrschenden, mir näher liegenden Verhältnisse mich auf die Spur bringen konnte, wo der Schlüssel dieses Räthsels zu suchen sei. Vielleicht, dass französische Statistiker auch diese französische Statistik besser zu würdigen wissen könnten, und zwar um so leichter, als

man in Frankreich selbst dieser Statistik — die zum Theile sogar von Hebammen herrühren soll — durchaus kein Vertrauen entgegenbringt. Was wir aber vor Allem fordern dürften, bevor man in einer so wichtigen Frage ein Urtheil abgibt, wäre unbedingt die Erstreckung solcher Beobachtungen auf längere Zeiträume, denn auf *ein*, auf gut Glück herausgegriffenes Jahr. Dies gibt auch Vogt zu. Er ist so loyal, eine Erweiterung der französischen Blatternstatistik auf sechs Jahre (aber nur für fünfundzwanzig Departements), wodurch seine vorhergehende Zusammenstellung insoweit entkräftet wird, als sich die Mortalität um ein Geringes zu Gunsten der gutimpfenden Departements neigt, dem Leser nicht vorzuenthalten. Geben auch wir unsererseits zu, dass dieser geringe Ausschlag noch viel zu wenig für die Schutzkraft beweist. Eine möglichst sachverständige, möglichst lange Perioden umfassende Revision der französischen Blatternstatistik erscheint nach alledem überaus wünschenswerth.[1]

Wieso aber aus der Nothwendigkeit einer Revision der Statistik die Nichtberechtigung der Impfung gefolgert werden kann,[2] wieso Vogt zu dem Schlusse kommt, dass, weil die Zahlen *Tardieu's* unglaubwürdig sind, der *Impfung* das Kreditiv der Existenzberechtigung fehle — das bleibt freilich unklar.

Oesterreich (Seite 129—134): Es werden hier die Bezirke Cisleithaniens nach der Häufigkeit der im Jahre 1874 vorgefallenen Pockentodesfälle in pockenreiche und pockenarme Gruppen zusammengestellt und dann untersucht, ob hier oder dort die Anzahl der Impfungen im Vorjahre eine grössere gewesen sei. "Die pockenreichsten Bezirke zeigen hier im Durchschnitte eine 173 Mal grössere Pockensterblichkeit, als die gleich grosse Bevölkerung der pockenärmsten Bezirke, und dennoch war im unmittelbar vorhergehenden Jahre in jenen die Impfung eifriger betrieben worden, als in diesen." Um den Einwurf zu beseitigen, dass die im Jahre 1874 pockenärmsten Bezirke vielleicht im Vorjahre durchseucht worden wären, hat Vogt die Pockentodesfälle dieser Bezirke auch für 1873 beigefügt: die pockenreichsten derselben waren auch in diesem Jahre schwerer heimgesucht.

Ferner wird hervorgehoben, dass die Bezirke, in denen Impfrenitenten vorkamen, um die Hälfte weniger Pockentodesfälle aufweisen, als jene ohne Impfrenitenten.

Es möge vor Allem bemerkt werden, dass die Daten, aus denen Vogt diese überraschenden Ergebnisse zieht und welche von ihm aus dem Quellenwerk der "Oesterreichischen Statistik" mit anerkennenswerthem Müheaufwand zusammengestellt wurden, richtig sind.

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[1] Ich selbst verfüge zu meinem Bedauern nicht über die Blatternberichte der Académie de medécine. Bezüglich der zuletzt angeführten Beobachtungen derselben möge noch bemerkt werden, dass sich auf Grund derselben in fünfzehn gutimpfenden Departements mit 5,797,253 Einwohnern in den sechs Jahren 1869 und 1871—1875: 65,614 Blatternerkrankungen, also auf 100,000 Einwohner jährlich **189**, ergaben; von den bei Vogt angeführten zehn schlecht impfenden Departements hat derselbe aber bei den Bevölkerungsziffern auch jene des Departements de la Seine (2,167,722) irrigerweise mitgerechnet, obwohl die Blatternfälle dieses Departements unausgefüllt (bloss mit einem Fragezeichen bezeichnet) waren. Nach Abrechnung dieses Departements bleiben also neun lässig impfende Departements mit 3,915,920 Einwohnern und 40,180 Erkrankungen, d. i. jährlich auf 100,000 Einwohner **171** Fälle. Wieso Vogt für jene Gruppe 16, für diese 17 Erkrankungen pro Jahr und 100,000 Einwohner berechnete, ist mir nicht verständlich. (Es sei auch noch bemerkt, dass das Totale aller Blatternerkrankungen nicht 95,794, sondern 105,794 beträgt.) Hingegen sterben von 100,000 Einwohnern in den schlecht impfenden Departements 42, in den gut impfenden 40.

[2] Vogt sagt nämlich S. 128: "Entweder sind die Daten von Tardieu glaubwürdig: alsdann muss die Vaccination, wie früher die Inoculation, gleich einem Verbrechen bestraft werden; . . . oder die Zahlen sind nicht glaubwürdig: dann fehlt der Impfung und besonders der Zwangsimpfung das Kreditiv der Existenzberechtigung. Ich schliesse mich der letzteren Alternative an."

Wohl machen sich aber sowohl gegen die Quelle selbst, wie auch gegen die Art der Schlussfolgerung manche Bedenken geltend :

1) Wenn Vogt alle 347 Bezirke Cisleithaniens durchgenommen hätte, so wäre es gleichgültig, ob er die gut und schlecht impfenden auf ihre Pockenmortalität oder die pockenarmen und pockenreichen auf ihren Impfeifer untersucht hätte. Indem aber Vogt von den 347 Bezirken Cisleithaniens nur einen ganz kleinen Theil, nämlich 59, hervorzieht, können die verschiedenen Fragestellungen zu verschiedenen Resultaten führen. Richtiger wäre also die Untersuchung gewesen, ob die Pockenfälle in den best oder in den schlechtest impfenden Bezirken grösser gewesen. Ich habe nun eine derartige Neuaufstellung aus den Daten der "Oesterreichischen Statistik" versucht und will nicht verschweigen, dass auch auf diesem Wege nur ebensolche Resultate zu erlangen waren, als die obigen Vogt'schen.

2) Da aber in den pockenreichsten Bezirken 90¼ %, in den pockenärmsten 90 % der zu Impfenden, also in beiden Gruppen gleich viele geimpft waren, liesse sich hieraus höchstens soviel folgern, dass die Impfung ohne Einfluss auf das Auftreten der Pocken war ; nicht aber, dass die Pocken dort, wo "*eifriger*" geimpft wurde, 173 Mal stärker auftraten.

3) Vogt selbst fühlt, dass die Ergebnisse *eines* Jahres nicht maassgebend sind. Er zieht deshalb auch die Pockenmortalität des Jahres 1873 in Rechnung. Die von Vogt in Betracht gezogene Epidemie währte in Oesterreich von 1871—1874 ; handelt es sich also um die Opfer derselben, so darf man nicht bloss ein oder zwei Jahre in Betracht ziehen. Zieht man z. B. nur das eine Jahr 1873 mit in Rechnung, so sieht man, dass beide Gruppen sich schon beinahe ganz nähern (289 und 261 Todesfälle auf 100,000 Einwohner, während im Jahre 1874 : 840 und 5 !).

4) Der ganze Beweisgang ist der von uns (im vierten Kapitel) als "falscher Parallelismus" abgelehnte : weil man im Jahre 1874 in einem Bezirke fleissig impfte, folgt hieraus nicht, dass die Zahl der Geschützten daselbst wirklich grösser sei, indem die Anzahl der Geschützten ein Resultat jahrzehntelanger Impfungen, nicht aber der im letzten Jahre — also zumeist doch nur an Neugeborenen ! — vorgenommenen Impfungen ist.

Diese Einwände gelten den Schlüssen Vogt's. Das Vertrauen in die Stichhaltigkeit derselben muss aber noch mehr schwinden, wenn man in eine Kritik der Quelle eingeht und man hierbei erfährt, dass dieselbe unverlässlich ist und deshalb alle aus derselben gezogenen Schlüsse schwankend werden. Der um die Statistik so hochverdienten österreichischen statistischen Central-Commission sind diese Uebelstände auch nicht unbekannt, und so viel ich weiss, dürfte eine baldige Reorganisation der ganzen österreichischen Impfstatistik zu erwarten stehen.

Die österreichische Impfstatistik liefert nämlich anscheinend eine Evidenzhaltung der Impfpflichtigen. Es wird in den Tabellen der Stand zu Anfang des Jahres, der Zuwachs durch Geburten, der Abgang durch Tod angegeben ; andererseits wird die Anzahl der Geimpften ebenfalls nachgewiesen und die Anzahl der ungeimpft Gebliebenen auf das nächste Jahr vorgetragen. Durch Gegenüberstellung der Geimpften zu den Impfpflichtigen gewinnt dann Vogt Einsicht über den grösseren oder geringeren Impfeifer der Kronländer Oesterreichs. Die ganze Einrichtung der österreichischen Impfstatistik erinnert demnach stark an die bayerische Evidenzhaltung der Impfpflichtigen, deren Verlässlichkeit durch *Bulmering* allen Fachkreisen bekannt ist. Die in der österreichischen Statistik gebotene Evidenzhaltung der Impfpflichtigen ist aber nur eine scheinbare : *in Wirklichkeit gibt es in Oesterreich keine solche Evidenzhaltung*, d. h. es gibt keine namentlichen Impflisten, durch welche die Impflinge bis zur Erfüllung der Impfpflicht in Controle gehalten werden. Das einzige Substrat dieser Evidenzhaltung bilden die durch die Pfarrämter alljährlich einzusendenden Ausweise über die in ihrem Sprengel vorgekommenen *Geburten* ; wer aber von diesen geimpft

worden, wer für später zurückgestellt worden sei, darüber bestehen keine Amtsvormerkungen.

Dass die durch die Statistik gebotene Evidenzhaltung eine imaginäre ist, wird aus folgenden zwei Thatsachen erhellen:

1) Da die Impfpflichtigen nirgends conscribirt werden, kennt man auch weder die Anzahl derselben, noch deren Aufenthaltsort. Wie soll man dann, namentlich in grösseren Städten, die Anzahl der Impfpflichtigen und den Procentsatz der aus diesem Kreise Geimpften angeben können? Das Oberphysicat der Stadt Wien erklärt z. B. in seinem Hauptimpfberichte pro 1883—'84, Seite 41: "*dass dermalen nicht die Möglichkeit geboten ist, den dermaligen Stand des Impfwesens in Wien festzustellen.*" Es ist also nicht möglich, die Anzahl der in Wien ungeimpft verbliebenen Personen anzugeben. Das Wiener Physicat lässt in Folge dieses Umstandes die betreffende Rubrik des statistischen Ausweises über das Impfgeschäft einfach unausgefüllt. In der Impfstatistik des österreichischen Staates erscheint diese Rubrik denn auch leer, was aber an sich, sowie in der Totalsumme — da die Positionen dieser Rubrik addirt wurden! — zahlenmässig denselben Effekt hervorbringt, als ob in Wien kein einziger Impfpflichtiger ungeimpft geblieben wäre. Dass aber deren geblieben sind, das beweist schon die grosse Anzahl der unter den Pockenkranken sich vorfindenden Ungeimpften.

Desgleichen antwortet mir Herr *Parovel*, Direktor des Triester statistischen Bureau's, am 16. November 1886, Zahl 8765: "es gibt gar keine Angaben, um die Anzahl der Ungeimpften zu eruiren." Auch aus Krakau antwortet mir der Direktor des dortigen statistischen Bureaus, Prof. Dr. *Kleczynsky*, unter dem 3. December 1886, dass die Zahl der in Krakau befindlichen Ungeimpften nicht bekannt sei;[1] dass übrigens die Impfstatistik auch nur die durch die Impfärzte vorgenommenen Impfungen enthalte, die der Privatpraxis nicht, dass hingegen in den Berichten der Impfärzte auch *Revaccinationen mitgezählt* seien. Dasselbe ist — sowohl hinsichtlich der Revaccinirten als hinsichtlich der Privatimpfungen — nach den vom 14. November 1886 datirten Mittheilungen des Direktors des statistischen Bureaus, Prof. Dr. *Pilat*, auch in Lemberg der Fall.

2) Man bedenke ferner, dass der Rest der ungeimpft Gebliebenen für das nächste Jahr wieder als impfpflichtig vorgeschrieben wird. Da nun aus der Rubrik dieser Impfpflichtigen — nach der Construction der Tabelle — kein anderes Entrinnen ist, als entweder durch die Rubrik der Gestorbenen, oder durch jene der Ausgewanderten, oder durch die der Geimpften; da jedes Jahr aber ein gut Theil der Impfpflichtigen weder stirbt, noch auswandert, noch geimpft wird: so müsste unter solchen Umständen die Anzahl der Impfpflichtigen schon im Laufe einiger weniger Jahre, durch die sich stets summirenden Ueberträge der Vorjahre, stark anwachsen. Nachdem dies aber nicht der Fall ist, nachdem der Stand der Impfpflichtigen alljährlich so ziemlich sein altes Niveau einhält: folgt schon hieraus, dass in der Statistik der Impfstellen irgend welche Unrichtigkeiten vorhanden sein müssen. Von den beiden Grundzahlen also, von denen Vogt ausgeht (die Anzahl der Impfpflichtigen und die der Geimpften), ist keine einzige richtig, können es also auch die Procentualzahlen nicht sein.

Schliesslich muss noch der Vogt'schen Zusammenstellung gegenüber Folgendes bemerkt werden: Jedermann, der die Verhältnisse Cisleithaniens kennt, weiss, dass Galizien und die Bukovina, im Hinblick auf die Entwickelung der wirthschaftlichen, criminalstatistischen, administrativen, culturellen, hygiënischen etc. Verhältnisse, zu den ungünstigst, hingegen Böhmen und Mähren zu den günstigst entwickelten Provinzen gehören. Für einen Kenner dieser Verhältnisse wird es also im Vorhinein feststehen, dass, wie alle administrativen und hygiënischen Verordnungen, so auch jene, welche die Impfung betreffen, in dem westlichen Böhmen und Mähren präciser

[1] Zugleich aber, dass im dortigen Kinderspital, von 1877—1883, unter 369 an Blattern Behandelten 343 ungeimpft waren!

ausgeführt werden dürften, als in den genannten zwei östlichen Provinzen der Monarchie. Man ist es deshalb auch gewohnt, dass sowohl die allgemeine Mortalität, wie auch Cholera, Blattern und sonstige Epidemieen, in diesen zwei Provinzen die meisten Opfer fordern. Wenn man nun aus der Statistik des Impfgeschäftes (welche zugleich jene der Impfhonorare ist!) sieht, dass eben in Galizien und in der Bukovina angeblich am fleissigsten geimpft werden sollte (also auch die grössten Auslagen für Impfzwecke verrechnet werden): so wird dies wohl jeden Kenner der Verhältnisse stutzig machen. Revidirt man nun die Tabelle Vogt's in dieser Beleuchtung, so findet man, dass die als eifriger impfend angeführten Bezirke fast alle in Galizien und in der Bukovina befindlich sind (unter 23 nur 7 nicht), während die lässigere Impfung zumeist böhmische und mährische Bezirke (22 unter 36) betreffen sollte! Die Statistik der durch die Epidemieen hinweggerafften Opfer entspricht den wirklichen Verhältnissen — in Galizien und in der Bukovina starben mehr an Blattern als in Böhmen und Mähren. Es kann eben kein Verstorbener verheimlicht werden. Leichter dürfte es allerdings sein, ungeimpfte Kinder als geimpfte zu verrechnen. Nach dem, was ich darüber vernommen, wie in Galizien und der Bukovina die Controle von den (oft des Schreibens unkundigen) Ortsvorständen ausgeübt wird, ist es durchaus nicht ausgeschlossen, dass die überaus günstigen Impfverhältnisse, welche diese beiden Kronländer aufweisen, der Unverlässlichkeit der Impfstatistik zuzuschreiben sind.

Bei der Bedeutung, die ich aber den durch Vogt über die österreichischen Verhältnisse angestellten Berechnungen beilege — er selbst betrachtet das Material ebenfalls als ein "grossartiges" — und bei der Beunruhigung, mit der eine aus den Verhältnissen eines so grossen Reiches geschöpfte Behauptung auch den vorurtheilsfreien Impfgläubigen erfüllen muss, wollte ich noch ein Mittel versuchen, um endgültigen Aufschluss darüber zu erhalten, ob die Bevölkerung von Galizien und der Bukovina wirklich besser geimpft sei, als jene von Böhmen und Mähren. Eine Zählung der Geimpften und Ungeimpften dieser Provinzen existirt freilich nicht, wohl aber war im Jahre 1886 die allgemeine Revaccinirung des österreich-ungarischen Heeres — und zwar, wie es heisst, auf direkte Initiative des Kronprinzen Rudolf — vorgenommen worden. Bei diesem Anlasse wurde nun auch der gegenwärtige Impfzustand der Truppen erhoben: es war also derart die Möglichkeit geboten, den Impfzustand wenigstens der militärpflichtigen Bevölkerung dieser Kronländer zu erfahren! Bei dem Dunkel, das bisher über den Impfzustand ganzer Bevölkerungen herrscht, wird man begreifen, welch grosse Wichtigkeit diesen Aufzeichnungen zuzuschreiben ist. Dieselben werden voraussichtlich in dem durch das Kriegsministerium herausgegebenen, ausgezeichneten militärstatistischen Jahrbuche in vollem Umfange veröffentlicht werden. Um aber die Resultate wenigstens theilweise im Rahmen dieser Arbeit verwerthen zu können, wendete ich mich an den Kriegsminister, Herrn Grafen Bylandt-Rheydt, mit der Bitte, mir die diesbezüglichen Daten, wenigstens für einige Bezirke, zusammenstellen zu lassen. Der Herr Kriegsminister hatte die besondere Güte, diese Zusammenstellung mit Erlass No. 866 zu verfügen, und bin ich in Folge dessen in der Lage, diesen dunklen Punkt des Vaccinationsstreites durch folgende Angaben aufzuhellen:

Von der Gesammtzahl der im Jahre 1886 zum Dienststande einrückenden und der Impfung unterzogenen Rekruten besassen:

	Impfnarben	Blatternnarben	Gar keine Narben
in den Regimentern No. 11 und 19 (aus Böhmen) und 3 und 8 (aus Mähren) im Durchschnitte	91 pCt.	3½ pCt.	5½ pCt.
hingegen bei den Regimentern No. 15 und 80 (aus Galizien) und 41 (aus der Bukovina) im Durchschnitte	83 "	6 "	11 "

Auf hundert Geimpfte entfallen also Ungeimpfte (d. h. Personen ohne Narben):

bei den böhmisch-mährischen Regimentern 6.1 pCt.
" " galizisch-bukovinaischen " 13.4 "

Man darf also füglich behaupten, dass nicht in Galizien und in der Bukovina, sondern in Böhmen und Mähren besser geimpft wird, dass also die Angaben Vogt's, welche schliesslich darauf hinauslaufen, als ob die Pocken in den eifriger impfenden zwei östlichen Provinzen häufiger gewesen wären, einen durch die Unverlässlichkeit der Quelle verursachten Irrthum repräsentiren und dahin zu corrigiren wären, *dass auch in Cisleithanien die Pocken in den lässiger impfenden östlichen Provinzen am stärksten auftraten.*

Was schliesslich die Berufung auf die Anzahl der Impfrenitenten betrifft, so möge bemerkt werden, dass die Anzahl derselben durchaus nicht als Symptom des grösseren oder geringeren Impfeifers der Bevölkerung zu betrachten ist. Es fehlt jede gesetzliche Bestimmung darüber, wer eigentlich als Impfrenitent anzusehen sei, und hängt es ganz von dem Belieben der Ortsbehörde ab, ob und wie viel "Renitente" irgendwo nachgewiesen werden.

SCHOTTLAND (Seite 135—136): Im Durchschnitte der Jahre 1864—1873 starben daselbst von je einer Million Lebender in den Inselbezirken 57, in den Landbezirken 104, in den Stadtbezirken 427 an Blattern. Was sollen diese Daten gegen den Impfschutz beweisen? Vogt weiss nur so viel anzuführen, dass die Unterschiede zwischen Insel, Stadt und Land sehr gross sind. Das ist unläugbar. Dass die städtische Bevölkerung auch bei gleichen Impfverhältnissen den Blattern — wie überhaupt allen Epidemieen — mehr ausgesetzt sei, ist bekannt. Der Schluss, dass hier besondere Schädlichkeiten wirken müssten, ist richtig, aber auch nicht neu. Doch Vogt setzt hinzu: "Soweit wir nur irgend statistische Angaben besitzen, nirgends ergibt sich nur annähernd eine so bedeutende Differenz zwischen gut und schlecht impfenden Ländern, wie wir sie hier in dem gleichen Lande, zur gleichen Zeit und unter identischen Impfverhältnissen vorfinden," — also Beweis dafür, dass andere Umstände entschiedener auf die Abwehr der Pocken wirken müssten als die Impfung, weshalb man also "zur Abwehr der Pocken weit mehr durch andere Maassnahmen wirken könnte." Die auf den ersten Blick plausiblen Prämissen des trotzdem unplausiblen Schlusses sollen nicht ohne Richtigstellung bleiben.

Vor Allem ist es nicht richtig, exceptionelle Verhältnisse als Maassstab zu wählen. Hierzu kann nur ein, die allgemeine Regel repräsentirender, Durchschnittswerth dienen. Wenn in einer Stadt Tausende an Blattern sterben, in einem Bezirke derselben aber nur *ein* Mensch denselben erlag, ist es ganz unerlaubt, diesen exceptionell bestellten Bezirk als die Regel, als das Maass für alle anderen zu betrachten. Dass dies kein ad absurdum outrirtes Beispiel ist, sondern, dass Vogt im Stande ist, allen Ernstes so zu thun, werden wir sogleich bei dem nachfolgenden Londoner Beispiele ersehen. Der schottischen Pocken-Statistik gegenüber verhält Vogt sich in ähnlicher Weise: er wählt als Maassstab die Inselbevölkerung, die im Ganzen kaum ein Zwanzigstel der schottischen Bevölkerung ausmacht. In diesem kleinsten Bestandtheil des Königreiches kommen jährlich etwa neun, sage *ganze neun* Blatterntodesfälle vor: mit diesen werden die Epidemieen des ganzen Landes gemessen!

Nicht zufrieden hiermit, werden diese neun Fälle auch noch nach dem Alter unterschieden! und zwar acceptirt Vogt nicht weniger als vierundzwanzig Altersklassen, kommt also durchschnittlich $\frac{1}{3}$ Todter auf je ein Jahr und eine Altersklasse! . . . So heisst es z. B. S. 135, dass auf eine Million Inselbewohner im Alter von 2—3 Jahren 27 Pockentodesfälle kommen. Nun betrug aber die ganze Inselbevölkerung, sammt allen Kindern, Erwachsenen, Greisen, am 3. April 1871 nur 161,999 Seelen! Die Anzahl der zwei- bis dreijährigen Kinder aber betrug gar nur 3657! Wenn nun die magoscopische Phantasie eines Statistikers dieses Klümpchen in dreihundertfacher Vergrösserung vorführt, und auch so nur $\frac{27}{1000000}$ Todesfälle aufweisen kann, so heisst das eigentlich nur so viel, dass in Wirklichkeit von den 3657 Personen jährlich durchschnittlich $\frac{1}{10}$

Person an Blattern starb![1] Wozu solche statistischen Kunststücke? Aber auch angenommen, dass die absoluten Zahlen gehörig gross wären, ist die Behauptung, dass zwischen der Sterblichkeit Geimpfter und Ungeimpfter nirgends so grosse Unterschiede beständen, wie in Schottland zwischen Inseln und Städten, einfach unrichtig; man vergleiche z. B. die Angaben in unserm vierten Kapitel über die Epidemieen in gut und schlecht impfenden Staaten, oder die Spitalslethalitäten für Geimpfte und Ungeimpfte. Desgleichen ist die Schlussfolgerung, die hieraus gezogen wird, absolut falsch. Wenn in einer Stadt jährlich 40 % sterben und es gelingt, diese Mortalität durch hygiënische Maassnahmen auf 20 % zu reduciren, so wird es noch immer vorkommen können, dass innerhalb dieser 20 % einzelne Klassen, z. B. Findelkinder, eine drei- bis vierfach grössere Mortalität als die übrige Bevölkerung aufweisen. Es wäre aber doch ganz unvernünftig, die grossen Verdienste der Wasserleitung, Canalisation u. s. w. schmälern oder gar wegläugnen zu wollen, nur deshalb, weil trotz der evidenten Besserung der Verhältnisse im Allgemeinen, andere, specielle Uebelstände noch nicht sanirt sind. So thut es aber Vogt mit Schottland: dass vorher in Schottland jährlich Tausende von Menschen an Pocken starben, gegenwärtig aber kaum ein Dutzend, so z. B.:

im Jahre 1878	4	1882	13
" " 1879	8	1883	11
" " 1880	10	1884	14
" " 1881	19	1885	16,

dieses glänzende Ergebniss der Schutzpockenimpfung lässt Vogt kalt; dass aber diese Handvoll Menschen zumeist in Grossstädten starben und nicht auf den Inseln — das gilt merkwürdigerweise als voller Beweis dafür, dass die Impfung doch unnütz sei, und führt zu der Behauptung, dass "wenn man einmal die Impfzwangsscheuleder abgeworfen habe und etwas tiefer in die Lebensverhältnisse der Menschen hineinsieht, so schrumpft die Vaccination in ihrer gegenwärtigen Handhabung auf eine der vielen nutzlosen sanitätspolizeilichen Vexationen zusammen, welche die grossen Fragen der Hygiëne durch übergeschäftige Vielthuerei im Kleinen zu verdecken suchen."

Im Zusammenhange hiemit möge zugleich jene umfangreiche schottische Pockenstatistik erledigt werden, die ganze dreizehn Seiten des Vogt'schen Buches (Seite 195—208) umfasst, ohne aber die Frage um einen Schritt vorwärts zu bringen. Was soll denn auch über den Schutz oder Nichtschutz der Impfung bewiesen werden können aus Daten, die, gleich den soeben citirten, weiter nichts als die Altersstatistik der in Schottland Verstorbenen enthalten, höchstens noch die Unterscheidung nach Inseln, Land und Stadt? Alle Schlüsse, die Vogt aus diesen Tabellen zieht und ziehen kann (siehe Seite 197), mögen für die Statistik der Altersklassen sehr werthvoll sein, sind aber für die Frage der Schutzkraft ganz gleichgültig: So z. B., auf welches Alter die Maxima der Sterblichkeit fallen? wie diese Maxima auf den Inseln und in den Städten wechseln, etc.? Die Behauptung, dass die gesunde Lebensweise einen *grösseren*, "einen weit sichereren Erfolg erziele, als alle sanitätspolizeilichen Schablonen und willkürlichen Vexationen" (d. h. als die Impfung), wird zwar wiederholt: aber eine Wiederholung einer leeren Behauptung ist noch kein Beweis.[2]

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[1] Nach den bei Vogt selbst (Seite 200) enthaltenen Angaben starben in den Inselbezirken *jährlich:*

im Alter von 0—1 Jahr	1½ Personen,
" " " 1—2 "	$\frac{1}{10}$ "
" " " 2—3 "	$\frac{1}{10}$ "
" " " 3—4 "	Niemand!
" " " 4—5 "	$\frac{1}{10}$ Personen! u. s. f.

[2] Die Mortalitätsziffern der drei ersten Altersperioden sollen "ein Vexirbild für den Impfdogmatiker darstellen", weil sich nämlich die Frage aufdrängen soll: Wo steckt der Nutzen der Impfung? Die Antwort hierauf und die Nachweisung des Irrthums bei Vogt findet sich bei *Lotz*, Seite 74.

Wenn man aus der schottischen Statistik einen Schluss über die Schutzkraft der Impfung ziehen will, bleibt nur der eine, von uns Vogt gegenüber wiederholt eingeschlagene Weg übrig, dass man sich nämlich frage: wie stand es in Schottland um die Blatternsterblichkeit vor und wie nach Einführung der Impfung? Diesem so nahe liegenden historischen Beweise geht Vogt hier wieder aus dem Wege. Lotz hat denselben versucht. Man sehe hier das Ergebniss für die Kinderbevölkerung (Seite 74):

Es starben von je 100,000 Kindern im Alter von

	0—6 Monaten:	6—12 Monaten:
1855—1864 (vor Einführung der Impfung)	310	341
1865—1873 (nach " " ")	174	49

Da Kinder in den ersten sechs Monaten selten geimpft werden, ist auch der Einfluss der Impfung kein so grosser; *aber noch immer rettete dieselbe die Hälfte der sonst an Pocken Verstorbenen.*

Seite 77 finden sich dann bei Lotz ähnliche Rechnungen für höhere Altersklassen, die alle überaus günstig für die Impftheorie sind. Ebendaselbst finden sich Berichtigungen mehrfacher in Vogt's Berechnungen vorgefallener Irrthümer.

Es möge schliesslich auch noch erwähnt sein, dass in den grossen, von Proletariat wimmelnden, Städten Schottlands mit 1¼ Millionen Einwohnern, im Laufe des Jahres 1885 nur 8 (!) Pockentodesfälle vorkamen, wovon auf Glasgow 1, auf Greenock 1, auf Pittsburg 1 Fall kommt, von dem einen in Greenock Verstorbenen es aber constatirt wurde, dass er auf dem Schiffe "Buenos Ayres" von Montreal aus krank eingeschleppt wurde. Freilich bleibt es noch immer möglich, dass von der nur 170,000 Seelen zählenden Inselbevölkerung während dieses Jahres gar Keiner starb. Fällt es nun Jemand bei, diese Mortalität von 0 mit jener von 8 Fällen zu vergleichen, so ergibt dies begreiflicherweise noch immer eine grosse, und zwar unendlich grosse Differenz zu Ungunsten der Städte, eine Differenz, von der Vogt sagen könnte, dass, "soweit wir nur irgend statistische Angaben besitzen, sich zwischen Geimpften und Ungeimpften nirgends auch nur annähernd eine so grosse ergibt". Man ersieht hieraus eben, wohin es führt, wenn man Extreme zum Maasse wählt.

London (Seite 136—140): Es starben daselbst 1877 im Ganzen 2544 Menschen, also auf 100,000 je 72. Die verschiedenen Distrikte waren in verschiedenem Maasse mitgenommen: in Lewisham starben nur 3 Menschen, in St. Giles 2, in Strand *Einer;* hingegen in Hampstead 352, in Hackney 552. Warum das so kam, das wüsste nur Der zu sagen, der die Entstehungsbedingungen des unbekannten Epidemiekeims kennte. Was hat aber dies mit der Frage zu schaffen, ob dieser Keim geimpften oder ungeimpften Personen mehr anhaben könne? Wir werden übrigens noch Gelegenheit haben, auf den wohlthätigen Einfluss, den die Impfung auf die Einschränkung der Londoner Pockenepidemie ausgeübt, zurückzukommen. Wenn aber Vogt zu dem famosen *einen* Verstorbenen des Strand greift und die Mortalität dieses beinahe immunen Bezirkes als jenen normalen Zustand auswählt, der zum Maasse für den Zustand aller anderen Bezirke der Weltstadt dienen soll, so ist das nicht blosse Geschmackssache, sondern einfach unerlaubt. Wäre der eine Kranke am Strand mit dem Leben davongekommen, so hätte die Auswahl des exceptionell immunen Strand wieder zu lauter unendlichen Werthen geführt! Diese Ergebnisse nennt Vogt dann "einzig in ihrer Art" — nicht mit Unrecht!

Wir schliessen hier an, was Vogt später über *England* anführt:

Pocken und Impfung in England und Wales, 1858—1875 (Seite 208—227). Durch lange Tabellen soll hier bewiesen werden, dass die Pockensterblichkeit seit Einführung des Impfzwangsgesetzes nicht nur nicht gemildert, sondern gesteigert wurde. So starben z. B. von je einer Million Einwohner:

IN ENGLAND:

von 1860—1867 (vor dem Impfzwange) 183
" 1868—1874 (nach " ") **315**

IN LONDON:

von 1857—1867 285
hingegen von 1868—1878 .....**446**

"Für das Dogma," sagt Vogt (Seite 209) ". . . . . bietet diese Tabelle den niederschlagenden Beweis, dass während der acht, auf die Erlassung des strengen Zwangsgesetzes folgenden Jahre durchschnittlich 315 Todesfälle per Jahr auf je eine Million Einwohner kamen, während die acht vorher verlaufenen Jahre deren nur 183 zählten."

Bevor wir auf eine Würdigung dieser Daten eingehen, wollen wir, zur Wahrung des principiellen Standpunktes, wieder hervorheben, dass der ganze historische Beweis eigentlich nur ein indirekter ist, dass trotz Einführung der Impfung, also trotz der hiermit beabsichtigten Schwächung der *Disposition* des Individuums, doch die *Keime* der Epidemie gegenwärtig kräftiger sein könnten, als vordem, dass also die Impfung die Geimpften schützen und trotzdem in der nachvaccinatorischen Zeit die Epidemieen stärker sein können, als vor Jenner. Wir wollen ferner hervorheben, dass der historische Beweis, auf ein einziges Land beschränkt, noch mehr an Bedeutung verliert. Derselbe ist uns bisher freilich noch in allen Fällen geglückt: in allen Ländern, auf deren Epidemieen Vogt sich berufen, ist es uns gelungen, zu zeigen, dass die postvaccinatorische Zeit viel milder war, als die prävaccinatorische. Es könnte aber sehr wohl geschehen, dass in dem einen oder dem anderen Lande ein solcher Beweis sich ausnahmsweise nicht herstellen liesse, ohne dass hierdurch die Hypothese des Impfschutzes als sinnloses Götzenbild über den Haufen geworfen werden dürfte, ohne dass die Impfgegner — wie Vogt meint — "hierdurch das Recht erwirkten, das gesetzliche Verbot der Impfung zu verlangen."

Diese Vorbehalte sollen übrigens nur zur Wahrung des principiellen Standpunktes dienen, und durchaus nicht dazu, etwaige ungünstige Ergebnisse des auf England angewendeten historischen Beweises im Vorhinein abzuschwächen. Wenn man nämlich einen Blick auf die im Anhange des vierten Kapitels mitgetheilte Statistik der englischen Pockentodesfälle seit 1838 wirft, wird man finden, dass diese auch für England — soweit dies auf dem Wege des historischen Beweises überhaupt möglich — *den glänzendsten Beweis für die wohlthuende Wirkung des Impfens abgeben.*

Die englische Impfgesetzgebung bietet nämlich folgende Phasen:

1841 gesetzliche Einführung der Impfung (ohne besondere Bedeutung),
1853 Einführung des Impfzwanges,
1867 neues, strenges Impfgesetz,
1871 neueste Verschärfung des Impfzwanges.

Mit Hinzurechnung der Periode vor 1841, wo die Impfung nur privatim geübt wurde, haben wir solcherart im Laufe dieses Jahrhunderts in England fünf Perioden zu unterscheiden, deren jede folgende eine strengere Handhabung der Impfung repräsentirt.

Auf Grundlage der Jahresberichte des Registrar General — die mit dem Jahre 1838 ihren Anfang nehmen — lässt sich nun sowohl für England, als für London folgender, mit der grösseren Verbreitung der Vaccination parallel gehender Rückgang der Blattern-todesfälle constatiren.

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[1] Auf Seite 217 wird diese Untersuchung auch nach dem Geschlecht durchgeführt: indem aber dort die Todesfälle für 1861—1870 in je *einer* Ziffer angegeben werden, während doch der Markstein der Zwangseinführung auf das Jahr 1867 fällt, lässt sich diese — ohnehin nicht bedeutsame — Tabelle für unsere Absicht gar nicht verwerthen.

Es starben von je 100,000 Einwohnern jährlich an Pocken :

	In ganz England:		In London:	
VOR EINFÜHRUNG DES IMPFZWANGES:				
I. Periode 1838—1841 (Privatimpfungen) nur vier Jahre!	68	31.	93	40.
II. " 1842—1853 (gesetzliche Einführung der Impfung)	29		37	
NACH EINFÜHRUNG DES IMPFZWANGES:				
III. Periode 1854—1867 (Impfzwang).....	15	18.	28	32.
IV. " 1868—1871 " (nur 4 Jahre!)	32		75	
V. " 1872—1885 (verschärfter Impfzwang)	13		25	

Wer könnte solchen Zahlen gegenüber den wohlthätigen Einfluss der Impfung läugnen ?

Die Schutzpockenstatistik Englands wird übrigens durch die ganz eigenthümlichen Verhältnisse Londons in fühlbarer Weise beeinflusst. Wenn man weiss, dass Grossstädte überhaupt der Fortpflanzung jenes geheimnissvollen Blatternkeims so fruchtbaren Boden bieten, wird man nicht verwundert sein, die volkreichste Stadt der Welt den Pockenepidemieen in starkem Maasse ausgesetzt zu sehen. Trennt man deshalb die Londoner Verhältnisse von jenen der Provinz, so findet man, dass auf 100,000 Lebende entfielen Pockentodesfälle :

	In London :	In der Provinz:
Vor der gesetzlichen Einführung der Impfung (1838 und 1839).....	124.69	76.92
Nach dem neuen Impfzwangsgesetze (von 1868 bis auf 1885).....	**34.32**	**13.58**

wie dies aus nachfolgender Tabelle erhellt :

POCKENSTERBLICHKEIT IN ENGLAND, 1838–1885, MIT UNTERSCHEIDUNG VON LONDON UND DER PROVINZ.

(*Nach den Ausweisen des Registrar General.*)

PERIODE.	ENGLAND.			u. z. a) PROVINZ.			b) LONDON.		
	Durchschnittliche Bevölkerung.	Pockentodesfälle im Jahres-Durchschnitt.	Von je 100,000 Lebenden.	Durchschnittliche Bevölkerung.	Pockentodesfälle im Jahres-Durchschnitt.	Von je 100,000 Lebenden.	Durchschnittliche Bevölkerung.	Pockentodesfälle im Jahres-Durchschnitt.	Von je 100,000 Lebenden.
1838–39	15,400,977	12,699	82.46	13,616,517	10,474	76.92	1,784,460	2,225	124 69
1840–49	16,641,282	5,882	35.35	14,586,791	4,967	34.05	2,054,491	915	44.54
1850–59	18,725,598	4,394	23.47	16,199,122	3,719	22.96	2,526,476	675	26.72
1860–67	20,766,893	3,863	18.60	17,838,986	2,938	16.47	2,927,907	925	31.59
1838–67	18,353,530	5,213	28.40	15,926,802	4,288	26.92	2,426,728	925	38.12
SEIT EINFÜHRUNG DES IMPFZWANGES.									
1868–69	22,086,006	1,738	7.87	18,932,272	1,302	6.88	3,153,734	437	13.82
1870–79	23,903,973	5,894	24.66	20,445,104	4,290	20.98	3,458,869	1,604	46.37
1880–85	26,598,686	1,847	6.94	22,672,814	980	4.32	3,925,872	867	22.08
1868–85	24,600,215	4,083	16.60	21,019,582	2,854	13.58	3,580,633	1,229	34.32

Um uns schliesslich eine Vorstellung jener Verhältnisse zu bieten, die in der prävaccinatorischen Zeit des vorigen Jahrhunderts herrschten, lassen wir hier noch nachstehende Angaben *Porter's* ("Progress of the Nation," die eine Decade 1760-'69 nach Moreau de Jonnès' "Statistique de la Grande-Bretagne," Vol. II, S. 359) folgen:

POCKENTODESFÄLLE IN LONDON VON 1701—1836.

Bevölkerung:[1]	Pockentodesfälle:	Also auf 100,000 Einwohner:
1701—1710.....688,250	jährlich im Durchschnitt 1372	199.3
1711—1720.....716,750	" " " 2123	296.2
1721—1730.....745,250	" " " 2257	302.9
1731—1740.....773,750	" " " 1978	255.6
1741—1750.....802,250	" " " 2002	249.5
1751—1760.....830,750	" " " 1957	235.6
1760—1769.....859,250	" " " 2443	284.3
1770—1779.....887,750	" " " 2204	248.3
1780—1789.....916,250	" " " 1712	186.8
1790—1799.....944,750	" " " 1768	187.1
Durchschnitt des Jahrhunderts.....		244.6

In London starben also im vorigen Jahrhundert jährlich durchschnittlich von je 100,000 Menschen 244 an Blattern, seit 1854 aber nur 32! Kann eine grossartigere Rechtfertigung einer hygiënischen Maassregel gefordert werden? Freilich kann man, wenn bloss die 32 Verstorbenen der letzten Jahre vorgeführt und dem gegenüber Länder mit noch geringerer Blatternsterblichkeit citirt werden, oder wenn einzelne ganz ausnahmsweise günstige Jahre der prävaccinatorischen Zeit herausgesucht werden, scheinbar die wahren Folgen der Schutzpockenimpfung verdecken und den Effekt hervorbringen, als ob jene 32 Todesfälle auf 100,000 Einwohner eigentlich noch immer recht bedenklich wären.[2] Auf solche Weise brachte es Vogt denn auch zu Wege, aus eben dieser so günstigen englischen Statistik Waffen gegen die Wirksamkeit der Impfung zu schmieden: er wählt eben einerseits nur jene acht Jahre, welche dem Impfgesetz von 1867 unmittelbar vorausgehen und welche überaus günstige sind, während in die acht nachfolgenden Jahre die arge Epidemie der siebenziger Jahre fällt: was Wunder, dass die letzte Periode grössere Sterbeziffern aufweist! Das sind eben die Effekte, welche man in der Statistik mit Hilfe der tendentiösen Auswahl zu Stande bringen kann.

CHEMNITZ (Seite 141—150): Gegenüber der ausgezeichneten, in ihrer Art einzigen, Arbeit *Flinzer's* — der nämlich eine Conscription des Impfzustandes der Chemnitzer Bevölkerung durchzusetzen wusste — macht Vogt mit Recht die Bemerkung, wie bedauerlich es sei, dass dieselbe nicht auch die Altersklassen in Betracht gezogen habe. (Die Chemnitzer Conscription unterscheidet nämlich nur zwischen Unter- und Ueberfünfzehnjährigen.) Flinzer kommt nun in seiner Arbeit zu Resultaten, welche entschieden zu Gunsten der Impfung sprechen: je grösser nämlich in einer Strasse die Anzahl der Ungeimpften war, um so häufiger waren dort die Pocken und umgekehrt. Vogt will dies nicht gelten lassen: ihm scheint, viel mehr als der Impfzustand, die

[1] Nach Moreau de Jonnès (a. a. V., II., Bd. S. 452) betrug die Bevölkerung von London im Jahre 1700 674,000. Im Jahre 1801 wurden gelegentlich der ersten Zählung 960,000, also in 101 Jahren ein Zuwachs von 286,000 gefunden. Wir nehmen daher für jedes Jahr eine Zunahme von 2850 Seelen an.

[2] Wir lassen hier noch — stets nach Porter — die Blatterntodesfälle für die weiteren Decaden, — welche schon in die Zeit der Schutzpockenimpfung fallen, — bis zur Einführung der Berichte des Registrar General (1838) folgen: 1800–'9: 1374; 1810–'19: 833; 1820–'29: 715; 1830–'36: 610. Der Einfluss der Impfung ist unverkennbar, namentlich wenn man die starke Zunahme Londons von 960,000 auf 1,800,000, — also das Doppelte! — in Betracht zieht.

Dichtigkeit der Wohnverhältnisse ausschlaggebend gewesen zu sein, und zwar entwirft er, um dies zu beweisen, folgenden Auszug aus den Tabellen Flinzer's. Indem nämlich die dichtest und die wenigst dicht bewohnten Strassen einander gegenüber gestellt werden, ergibt sich, dass auf 1000 Einwohner entfallen:

a) IN DEN DICHTEST BEWOHNTEN STRASSEN:

17 Wohnhäuser,	869 Geimpfte,	104 Pockenkranke,	9 Pockentodte.
21 "	880 "	78 "	6 "

b) IN DEN WENIGST DICHT BEWOHNTEN STRASSEN:

33 Wohnhäuser,	931 Geimpfte,	26 Pockenkranke,	2 Pockentodte.
53 "	939 "	18 "	1 "

Wenn Vogt hieraus folgert, dass die Wohnverhältnisse von günstigem Einfluss waren, so ist das ganz richtig; aber ebenso richtig ist doch, dass auch der Impfzustand von Einfluss war, da man sieht, dass in dem Maasse, als der Procentsatz der Geimpften steigt, auch die Anzahl der Pockenerkrankungen und -Todesfälle abnimmt. Will man den Einfluss dieser zwei concurrirenden Factoren getrennt erkennen, so müsste man jede Cathegorie des einen Gesichtspunktes (z. B. dichtest bewohnte Strassen) nach den verschiedenen Cathegorien des zweiten Gesichtspunktes in secundäre Gruppen auftheilen (also im vorigen Beispiel in vier Gruppen, nämlich 869, 880, 931 und 939% von Geimpften,) und dann untersuchen, *a*) wie sich die Pockensterblichkeit ein und derselben Dichtigkeitsgruppen je nach dem wechselnden Procentsatze der Geimpften verändert; *b*) wie innerhalb ein und desselben Procentsatzes von Geimpften nach den Abstufungen der Wohnverhältnisse. Da Vogt auf eine derartige Untersuchung nicht einging, ist er auch nicht in der Lage und hat auch kein Recht, den Einfluss des einen Factors auf Kosten des anderen hervorzuheben. Die Motivirung aber, mit welcher dies geschieht, ist eine bei Vogt wiederholt wiederkehrende, weshalb es gut sein wird, uns mit derselben endgültig abzufinden.

Es heisst nämlich Seite 148: "Die Tabelle zeigt, dass in allen Colonnen keine Differenz so gross auftritt als diejenige in der *Wohnungsdichtigkeit:* auf das Doppelte der Wohnungsdichtigkeit kommt das Vierfache von Pockenerkrankungen und auf das Dreifache der Ersteren fällt nahezu das Sechsfache der Letzteren . . . . Es geht mit Sicherheit aus der Tabelle hervor, dass diese erste und direkte Ursache (d. i. der unbekannte Pockenkeim) weit mehr mit der Wohnungsdichtigkeit, als mit der Impfung zusammenhängt. Die schwerstbefallene Strassengruppe hatte 87 pCt. Geimpfte, während die pockenärmste Gruppe davon 94 pCt. zählte: 87 zu 94 gibt aber ein Verhältniss, welches gegenüber dem oberwähnten ganz verschwindet."

In dieser Motivirung liegt ein ganz eigenthümliches, so viel mir erinnerlich, auch von Anderen bereits supponirtes Princip verborgen, als ob nämlich bei gleitenden Vergleichungen die Scalen parallel gleiten müssten. Wir verstehen unter gleitenden Vergleichungen, wenn man zur Erkenntniss eines Causalzusammenhanges die Ursache stärker oder schwächer auftreten lässt, um dann zu sehen, ob auch die Wirkung sich in derselben Richtung ändere. Die falsche Supposition liegt nun darin, dass man erwartet, die Scala der Wirkungen müsse in *eben demselben Verhältnisse* zunehmen, als die Scala der Ursachen. Das ist aber durchaus nicht nöthig und wird nur in den seltensten Fällen möglich sein. Es ist z. B. erwiesen, dass der Wohlstand die Lebensdauer verlängere. Um nun diesen Satz durch die angegebene Methode zu bestärken, wird man eine Wohlstandsscala aufstellen, etwa den Einkommen von 1000, 2000, 3000, 4000, 5000 Francs entsprechend. Wenn man nun auch findet, dass Leute mit 2000 Francs um zwei Jahre länger leben als die von Almosen lebenden, so wäre es doch falsch, zu erwarten, dass Leute mit 3000 Francs um 3, und Leute mit 5000 Francs um 5 Jahre länger leben müssten. Die Scala der *Ursachen* kann in diesem Falle ins Unendliche gesteigert werden; es gibt Leute mit 1 Million und mit 10 Millionen Francs Revenuen: die Scala der *Wirkungen* hat aber eine Grenze. So z. B. in unserem Exempel, dass das

menschliche Leben keine 10,000 oder 100,000 Jahre währen kann. Man darf also bei Gleitung der Ursachen keine parallele Gleitung der Wirkungen erwarten. Eben deshalb ist es aber ganz unrichtig, wenn Vogt sich darauf beruft, dass die Scala der Geimpften die Latitude von 94 bis 87 pCt. aufweise, während sowohl jene der Wohnverhältnisse, als die der Pockentodesfälle, in einem viel grösseren Maassstabe gleite. Demselben logischen Fehler begegnen wir, wenn Vogt aus den Differenzen in der Zahl der jährlichen Impfungen auf die Differenzen in dem Ausbrechen der Epidemieen schliessen will. Ganz nahe berühren sich hiemit jene "falschen Parallelismen", die sowohl Vogt, wie auch Impffreunde (z. B. Brunner) zwischen Impfungsziffern und Todesfällen zu ziehen lieben.

Nord-Amerika (Seite 150): Es besteht daselbst kein Impfzwang; trotzdem kamen im Jahre 1870 nur 4507 Pockentodesfälle vor. Es darf aber hierbei nicht übersehen werden, dass die Vereinigten Staaten keine Mortalitätsstatistik besitzen. Während die europäischen statistischen Bureaus Tag für Tag die Todesursachen verzeichnen und sie am Jahresschlusse nur summiren, beschränkt sich die Mortalitätsstatistik der Vereinigten Staaten — einige wenige Grossstädte und noch wenigere Staaten ausgenommen — auf jene Daten, welche der Volkszählungsagent gelegentlich des Census für das verflossene Jahr *nach mündlichen* Informationen (!) zusammentragen kann. Es folgt hierauf bei Vogt eine Tabelle über die Blatternfälle in New York während der letzten 25 Jahre; da dieselbe keinerlei Bezug auf den Impfzustand nimmt, ist diese für unsere Frage irrelevant.

c) Pocken und Impfschutz in den verschiedenen Lebensaltern.

Wir sind Vogt bisher in seinen statistischen Bemühungen Schritt für Schritt gefolgt; wir haben seine Quellen nachgeschlagen, haben seine Rechnungen nachgerechnet und sind zu dem gewiss bemerkenswerthen Ergebnisse gekommen, dass bisher in dem voluminösen, an Ziffern und Thatsachen leider zu reichen Buche, sich *nicht ein einziges* Datum finden lässt, das zu Ungunsten der Impfung spräche, ja dass im Gegentheil fast alle zur Bekämpfung der Impftheorie ausgesuchten statistischen Beobachtungen in eine Verherrlichung derselben ausliefen. Es erübrigt uns nun nur noch die Prüfung eines Abschnittes, jenes, der von den Einflüssen des Lebensalters auf Pocken und Impfschutz handelt.

Versuchen wir, zuerst für uns in's Reine zu kommen, was eigentlich durch eine derartige Untersuchung erreicht werden solle. Es können sich nur zwei Fälle ergeben: entweder verhalten sich die Pocken, bez. die Sterblichkeit der Geimpften in den verschiedenen Lebensaltern so wie die übrigen Krankheiten, oder sie verhalten sich verschieden. Ob nun das Eine oder das Andere eintrifft, was kann denn hieraus über die Existenz oder Nichtexistenz der Schutzkraft gefolgert werden? Vogt hebt es nun, mit Löhnert, als eine sehr bemerkenswerthe Errungenschaft der Impfgegner hervor, entdeckt zu haben, dass die Pockensterblichkeit Geimpfter in den verschiedenen Lebensaltern genau dieselben Schwankungen zeige, wie die sonstige Sterblichkeit. Und wenn auch? Angenommen, die Pockensterblichkeit repräsentirte eine stabile Erhöhung der gewöhnlichen Sterblichkeitsrate; sie fungirte gleich einem ständigen Steuerzuschlage. Gelingt es nun Jemand, diesen Zuschlag zu beseitigen, ist sein Verdienst deshalb ein geringeres, weil dieser Zuschlag nicht ein variabler, sondern ein ständiger gewesen?

Für die Frage über die Existenz des Impfschutzes, d. h. darüber, ob die Impfung schützt oder nicht, hat also die Untersuchung des Einflusses des Alters keine fundamentale Bedeutung — namentlich, nachdem wir schon wissen, dass die Impfung wirklich und in allen Altersklassen schützt. Die Probleme, die sich Vogt diesbezüglich auf Seite 162 stellt, haben deshalb für uns kein besonderes Interesse, selbst für den Fall nicht, als Vogt die Antwort auf diese Fragen in einer, nach dem Stande der Vaccinationsstatistik, verlässlichen Weise bieten würde.

Nun tritt aber der Umstand hinzu, dass dies durchaus nicht der Fall ist, dass im Gegentheil dieser Theil des Buches die partie honteuse der ganzen Arbeit bildet. Wir gehören nicht zu Jenen, welche den Werth einer statistischen Arbeit nach der Präcision der mechanischen Rechenarbeit schätzen, welche eine in ihren Anlagen und ihren Gesichtspunkten anerkennenswerthe Leistung, einiger irriger Additionen oder Divisionen wegen für werthlos erachteten. Was aber in diesem Theile der Vogt'schen Arbeit geboten wird, übersteigt denn doch das Maass des Erlaubten. So, wenn ein Autor seitenlange Beobachtungen über die Moral der Leipziger Sterblichkeitsstatistik anstellt, welche Reflectionen sich aber dann alle als hinfällig herausstellen, weil die betreffende Statistik sich gar nicht auf die Blattern bezieht;[1] oder, wenn aus dem Vergleiche mit den Verhältnissen Besançons vor der Impfæra, gegen andere Städte nach der Impfæra, spaltenlange Beweise über die günstige Sterblichkeit des nichtimpfenden Besançons gezogen werden, während eben das Gegentheil der Fall ist, indem die Ziffern von Besançon gerade um das *achtfache* zu niedrig angegeben sind;[2] wenn ebenso bei einem ähnlichen Vergleiche mit Genf sich herausstellt, dass alle Genfer Ziffern um das Drei- bis Dreissigfache zu niedrig lauten;[3] wenn man schliesslich bei jedem Schritte die impfgegnerischen Behauptungen nur durch jene sonderbaren, ganze Seiten füllenden "Pockentafelberechnungen" Vogt's legitimirt sieht, von denen aber bewiesen ist, dass sie irrig sind, und von denen deshalb Vogt selbst, in seiner neuen Arbeit, endgiltig abgekommen ist.[4]

Der ganze Abschnitt macht solcherweise mehr den Eindruck eines ungeprüften Brouillons von Notizen, Rechnungsversuchen und Gedankenspänen, die besser ungedruckt geblieben wären und die eine eingehende Kritik umsoweniger lohnen als—wie erwähnt—selbst für den Fall, als der Inhalt ein geordneter wäre, wir aus denselben keine Erleuchtung über die Frage der Geschütztheit Geimpfter erwarten können.[5]

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[1] Weshalb auch in Vogt's "Alter und neuer Impfglaube" ersucht wird, die ganze Abhandlung als "non arrivè" zu betrachten.

[2] Siehe die kritischen Bemerkungen von *Lotz*, Seite 70.

[3] Siehe wieder Lotz, Seite 5.

[4] Solche Pockentafelberechnungen werden aufgestellt: Seite 165 für Besançon; Seite 168 für Paris, Wien, Stockholm, Chemnitz, Berlin, Budapest, Genf; Seite 171 für Brixen, Chemnitz, Berlin, Leipzig, Dinaburg; Seite 172 für Paris, Nürnberg, Zürich; dieselben füllen auch die Seiten 175, 176, 181 (England), 182 (Elberfeld) und 183 (Stockholm).

[5] Zum Ueberflusse enthält dieser Abschnitt auch noch eine ganz unvermittelte Abschweifung über die Pockenmortalität der französischen Armee. Es werden daselbst die grossen Unterschiede hervorgehoben, die sich in den drei Theilen der französischen Armee (in Frankreich, in Algier, algierisches Corps) hinsichtlich der Pockenerkrankung ergaben. Vogt findet nun, dass bei der Armee in Frankreich, welche die meisten vollkommen und unvollkommen Geschützten enthielt, die meisten Pockenerkrankungen vorfielen, während hingegen das algierische Corps, welches "weniger im Dienste Vaccinirte und Revaccinirte" enthielt, am wenigsten an Pocken litt, dass also hierdurch die Impftheorie schmählich desavouirt sei. Die betreffenden Quellen stehen uns leider nicht zu Gebote: wir wollen also annehmen, dass in den Berechnungen kein Irrthum untergelaufen sei. Wir haben aber aus den eigenen Angaben Vogt's vor Allem zu bemerken, dass in dem algierischen Corps im Ganzen 44,000 Mann, in Frankreich aber 283,000 dienten, dass also die 1½% Pockenkrankenfälle des Corps im Ganzen nur etwa 40 Fälle repräsentiren konnten, die Todesfälle aber nur etwa *fünf* betragen mochten! Die Zahlen sind also wieder, zum Theile sogar in bedenklichem Maasse, zu klein. Abgesehen hiervon, befleisst sich aber Vogt einer auffällig künstlichen Unterscheidung zwischen "mit und ohne Erfolg Geimpften,"—eine Unterscheidung, die er sonst perhorrescirt, auch nirgends anwendet—ferner der "ganz Geschützten," d. i. "der im Dienste Vaccinirten und Revaccinirten," und der "theilweise Geschützten," d. i. der früher Geimpften. *Merkwürdigerweise vergisst er aber hierbei der allermeist Geschützten, nämlich der Gepockten, ganz!* Wohin sind diese zu zählen?

5. IMPFSCHÄDEN.

In den restlichen wenigen Seiten seiner Schrift behandelt Vogt die Impfschäden und trägt mehrere Fälle von durch Impfung verursachter Syphilis zusammen.

Die Möglichkeit solcher Impfschäden ist heute anerkannt: es fragt sich nur um die Bedeutung dieser Fälle, um das *Maass* des durch dieselben verursachten Schadens. Vogt ist natürlich nicht in der Lage, eine Abschätzung dieses Schadens zu bieten, weil die bisherigen Methoden der Vaccinationsstatistik diese Möglichkeit nicht bieten. So weit es uns möglich war, auf diese wichtige Frage mit Hilfe unserer neueren Methode zu antworten, haben wir aber gefunden, dass das Maass dieser Schädigungen verschwindend klein, beinahe Null ist, brauchen uns also auch durch diese Seite der impfgegnerischen Argumentation in dem Vertrauen zum Impfschutze nicht wankend machen zu lassen.

So wären wir denn bis an das Ende dieser reichhaltigsten Streitschrift des Impfkampfes gelangt. Redlich uns bemühend, in den Gedankengang derselben einzugehen, Schritt für Schritt jedem statistischen Beweise nachtretend und denselben bis an seine Quellen verfolgend, können wir diese mühsame Inquirirung mit dem, für die Impfvertheidigung immerhin erfreulichen Ergebnisse abschliessen, dass in dem ganzen angehäuften, in seiner Massenhaftigkeit erdrückenden, in seiner Verschiedenartigkeit ermüdenden, Material auch nicht ein Stein auf dem anderen bleiben, auch *nicht ein einziger* der gegen den Impfschutz vorgebrachten Beweise als stichhaltig anerkannt werden konnte.

Diese Vergesslichkeit scheint keine ganz zufällige zu sein, denn hätte Vogt auch diese zu den Geschützten gerechnet, so hätte sich der Impfzustand der drei Armeen folgendermaassen ergeben:

	in Frankreich,	in Algier,	algierisches Corps,
Anzahl der Gepockten.....	3.4 %	3.4 %	**40.9 %**
" " jüngst Geimpften.....	1.2 %	0.5 %	0.5 %
" " Revaccinirten	46.7 %	17.8 %	10.8 %
Zusammen: Geschützte.....	51.3 %	21.7 %	52.2 %

Es ergibt sich solcherart, dass die Anzahl der Geschützten bei der Armee in Frankreich und dem algierischen Corps gleich gross war, und dass die französische Armee in Algier die wenigsten Geschützten zählte. Bei der Letzteren traten nun auch mehr Pockenfälle ein, als bei dem unter demselben Clima, denselben hygiënischen Bedingungen lebenden algierischen Corps. Gesondert von diesen zwei afrikanischen Corps stand nun die Hauptarmee in Europa, dieselbe, welche der verheerenden Epidemie, die in Frankreich in den siebenziger Jahren herrschte, sowie den in jeder Beziehung mörderischen Einflüssen des deutsch-französischen Krieges und der Kriegsgefangenschaft ausgesetzt war. Dass bei einer solchen Häufung und Verstärkung der epidemischen Keime, dieselben mehr Menschen krank machten, als bei den in Afrika ruhenden Truppen, das braucht uns wahrlich nicht Wunder zu nehmen. Falls in Algier Krieg ausgebrochen wäre, würde dies gewiss auch die Sterblichkeit des dortigen Corps erhöht haben. Die Vergleichungen Vogt's werden durch diese theils übersehenen, theils absichtlich ignorirten Umstände bedeutend abgeschwächt. Eine viel richtigere Aufstellung wäre es doch, zu fragen, wie viel von den Geimpften, wie viel von den Nichtgeimpften der französischen Armee in *Europa* den Blattern erlagen? Wir verfügen, wie gesagt, über die diesbezüglichen Quellenwerke nicht, wissen also auch nicht, wie die Antwort hierauf lauten werde: nach den bisherigen Erfahrungen steht es aber für uns ausser allem Zweifel, dass auch im französischen Heere die Geimpften zum Mindesten dreifach besser davonkommen mochten. Es wäre dies wieder ein Fall, der Zeugniss dafür ablegte, ob man denn wirklich mit der Statistik alles Beliebige beweisen könne? Was uns betrifft, sehen wir dem uns unbekannten Ergebnisse dieser statistischen Herausforderung mit der grössten Ruhe und Zuversicht entgegen.

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DELEGATE: Prof. LÉON LE FORT, Paris, France.

FIRST DAY.

The Section met on Monday, September 5th, at 3 P. M., at the Congregational Church, corner Tenth and G streets. Each day thereafter, throughout the sessions of the Congress, the Section met at 11 A. M. in morning session, and at 3 P. M. in afternoon session. The following is the opening address of the President of the Section :—

GENTLEMEN OF THE SECTION OF GENERAL SURGERY :—

It is my pleasant duty to greet you to this hall set apart for our portion of the work of the International Medical Congress, and in behalf of my American colleagues I extend a special and cordial welcome to those of you who have come from foreign countries to participate in the discussions of this Section. I welcome you to our beautiful Capital city, the admiration of all who visit it. I assure you it is our most earnest desire that your visit may prove both pleasant and profitable. I hail this happy occasion for fraternal intercourse, and trust the friendships formed under such happy auspices may continue to grow in interest, and enhance our mutual estimation. I recognize among you many who bear names highly honored and respected in the countries from which you come, and whose distinction has made them familiar as household words in our own.

The ranks of foreign and domestic workers in the field of surgery have contributed this representation to promote the advancement of surgical knowledge and skill; but great as that object may be, it is even subordinate to the general advantage to accrue from the acquaintance of individual members, and the personal interchange of thought on the topics of our daily labors and study. This Congress is a deliberative assemblage of experts from all parts of the world, called to pass under review the state of the various departments of the science and art of healing, as presented at nearly the close of the nineteenth century of Christian civilization. Its object is a part of that great mission of peace and good will to men. The progress we have met to illustrate and cultivate is that of man's skill in the abrogation of disease and suffering. The victories we claim are those won by wresting the innermost secrets from nature, by patient research and brilliant experiment, for the relief of the human race from the physical accidents and ills to which it is subject. The achievements of surgery constitute a notable and noble chapter of this great record of beneficence.

Above all others which have preceded it, this may be claimed as the age of mental activity and progress in exact and scientific knowledge. Our heritage in this respect has come to us weighed down with the accumulation of all the centuries, from which we reserve the sound and proven parts, and after adding our own collection, transmit the whole for the benefit of future generations. Rich as we may esteem ourselves at the present time, it is not perhaps given us to conceive the measureless accretion of the time to come. The surgical profession was never so active as now, never so prolific in great and beneficent results, never so successful in

venturous exploits. Masterly intellects on both sides of the Atlantic are leading in this great work, and broadening and strengthening year by year the scientific foundations and buttresses of the art. Under this inspiration, the whole field of surgery is undergoing thorough and complete revision. Surgical pathology was never more zealously or successfully cultivated. Therapeutic surgery is making rapid advances, and rendering more sure the work of the knife. Operative surgery is adding one brilliant success to another, and commands the admiration of the world to a degree never before attained. In whatever direction the surgeon may look the prospect is gratifying, and he is animated to strive for still wider achievements. From this elevated standpoint, we have come to measure the extent of the progress made, and to estimate fairly the intrinsic value of the advance as shown by its practical and demonstrated utility.

In no department of modern surgery have more marked and brilliant results been attained than in that pertaining to the abdominal cavity, which will be the first subject to be presented and discussed in this Section. It has been but a short time —a time even in the personal recollection of many in the hearing of my voice—since the abdominal cavity was to the surgeon a *terra incognita*—a forbidden field, which the knife and the ligature dared not enter. Even when an accidental wound invaded the precincts of this region, it was sacred from the surgeon's interference, and it was the teaching to close it hermetically in the shortest possible time, and trust to nature for the consequences. The growth of large and uncouth tumors in the peritoneal cavity of females was a fact of appalling frequency, from which no relief was hoped. Many a poor woman was left to bear through life a grievous weight of suffering from which a miserable death would only free her. Over many of the handmaidens of God surgery permitted a curse to hang more bitter than that from which Sarah of old prayed the deliverance to be found only in the grave. In such a presence the hand of science was paralyzed, its skill was as naught, and the surgeon turned his face in despair from her whom it were better had a millstone been hanged about her neck; his ear was pierced by the helpless and hopeless plaint of her who was doomed to constant pain and premature death, and in all the resources of his art no remedy was to be found. At last, in the deep forest of the new world—in the backwoods of Kentucky—the genius of Ephraim McDowell flashed upon the darkness which had so long enthralled such subjects of suffering, and it was given to his skillful hand to lift the weight from the beauteous girdle of maternity, and loose her who had been bound down by the spirit of infirmity for long wretched years, and to constitute himself the apostle of a new faith in surgical science whose songs of ransom will be forever sung in praise of his name. From far among the hills of Scotland, the hand of Bell, who of all men bore alone a torch in the unexplored region of ovarian pathology, first pointed the pathway which even he had not the nerve to tread; from that torch dropped a spark which fell into the fervid soul of McDowell. It there found both the courage and the skill to develop it into the beacon now erected on the dark and treacherous shores of ovarian disease, which has since illumed the way and conducted many a sorrow-laden argosy to the port of health. McDowell's first subject may justly rank among the world's sublimest heroines. Linked inseparably with his great fame will be the name of Mrs. Crawford—a courageous woman whose heart and brain were equal to the task of encountering a risk before unmatched in the annals of woman's faith and endurance. She mounted with a willing martyr's tread the operating table of the intrepid surgeon, and stepping down whole, she became the instrumentality of redemption from untold agony of her suffering sisters in all ages to come. Conscious that she was the first to undergo the dreadful ordeal,

she did not quail. The scimitar's edge bridged the fearful gorge beneath, which no foot had ever crossed. It looked like the bottomless pit of despair. No angel of anæsthesia then spread her wings to offer the lethal cup to the quivering victim. Every writhing nerve was torture to the steadfast soul who subjected herself to the perilous undertaking. She did not murmur or draw back. She knew her brave surgeon had no guide or lamp—that no one had preceded him; yet she trusted his skill to lead her to the triumphant goal; and out into the glorious paradise of health he conveyed her, from the valley of the shadow of death. Neither the pen of history nor the lyre of song has ever told a nobler story than of this true-hearted matron of our western wilds; and the records of surgical science embalm no triumph of its art greater than McDowell's, nor one fraught with such treasures of beneficence to our kind. The monuments of marble or of brass which have been raised to sages and warriors will crumble to dust, but grateful human hearts will hold a monument to the names of Ephraim McDowell and Mary Crawford, to last as long as hearts shall beat.

It would appear that this crowning success in abdominal surgery should have at once taken deep and firm hold on the surgical mind, and led to important results long ere it did so. The history of science teaches, however, that important discoveries in her domain are doomed to mistrust and doubt, and sometimes to the ignobler passions. Thus it was with Harvey's great discovery of the action of the heart and the circulation of the blood—that crowning achievement which lies at the foundation of modern physiology. Notwithstanding Dr. McDowell had performed his operation in thirteen cases with an unprecedented degree of success, only three of his countrymen during his lifetime had the courage to follow in his footsteps. Nathan Smith, of Connecticut, Alban Goldsmith, of Kentucky and David L. Rogers, of New York, constitute the illustrious trio. The first attempt at regular ovariotomy in Great Britain, since so successfully and gloriously illustrated, was made by Mr. John Lizars, of Edinburgh, a pupil of John Bell, in 1825. McDowell's success encouraged him to the undertaking. He published a monograph detailing his own cases and embracing a full report of those of McDowell, which to that date were not generally known in Great Britain. For a number of years following this publication, ovariotomy was in a state of desuetude. The great merits of its successful revival is unquestionably due to the able and persistent labors of Charles Clay, of Manchester, England, Frederick Bird, of London, the Atlees, of Pennsylvania, and Dr. Dunlap, of Ohio. Their great success placed the operation on a firm and immutable basis. It is now recognized as one of the established and necessary operations of surgery, and is practiced in every country where civilization has carried the resources of scientific medicine. It is an acknowledged benefaction to the human race. In the hands of different surgeons, it is computed that to this date it has added more than fifty thousand years to the span of woman's life, and has restored thousands of wretched sufferers to health and happiness, whose fate else would have been protracted misery and longed-for death.

Under the success achieved in ovariotomy in recent years, the field of abdominal surgery has been largely extended. No part of the peritoneal cavity is now prohibited territory. Laparotomy is frequently resorted to for diagnostic and therapeutic purposes. The uterus and its appendages, the kidney and the spleen, the stomach and pancreas, the large and small intestines and the gall bladder have all been brought in subjection to the surgeon's knife, and made to pay tribute to his skill. Wounds of the abdomen involving the peritoneum have been enlarged, bleeding vessels ligatured, injured viscera sutured, the cavity toiletted and closed. Even in peritonitis it is proposed that a laparotomy shall be made, the inflammatory products discharged, and the

cavity thoroughly washed and drained. What the fate of some of these operations may be—whether they will stand the crucial test and be ultimately adopted as legitimate surgical procedures—is an open question. This address is not the proper place to discuss such questions, but there is reason to apprehend that the value and safety of abdominal section and exploration for other purposes than the removal of ovarian tumors may be somewhat exaggerated; yet antiseptic surgery has so efficiently aided the knife, and under the inspiration of its great leader is still making such progress, that we may not despair of hope that much yet questioned may become assured and permanent acquisition to our art.

The advance in operative surgery inaugurated by McDowell and his colaborers in the abdominal cavity has had a wider range of application than that region, and under its spirit and success surgeons have been emboldened, with the support of antiseptic measures, to venture into other regions until recently regarded as beyond the pale of legitimate interference. The brain, one of the most intricate structures, both anatomically and physiologically, with its sensitive and delicate texture, securely protected as it is by its tutamina, has become one of the most fertile and promising fields for operative effort. The surgeon now boldly passes through these safeguards, not only to lift or to extract impinging fragments of bone, to arrest bleeding from injured vessels and to give exit to inflammatory effusions which may be doing damage to the frail structure of the organ, but also to remove foreign bodies which may have been driven into its substance and to extirpate tumors which may have developed therein.

Brain surgery is indeed claiming a very large attention, and a degree of progress on safe and conservative lines is being attained, which in the near future will add crowns of triumph to our art. I am gratified to state that one of the papers to be presented to this Section will be from the hand of a distinguished foreign writer, and will have reference to this branch of surgery.

Since the meeting of the last International Medical Congress, the general field of operative skill has been greatly enlarged, and the noble workmen in our art are confidently and aggressively entering upon surgical procedures which our fathers would have shrunk from as impossible of curative results. But I must desist from further remarks and introduce you to the business of the day.

Thanking you for the attention and courtesy you have paid to this brief opening, I now declare the Section of General Surgery of the Ninth International Medical Congress to be opened for its regular business.

Dr. Charles T. Parkes, of Chicago, read the following :—

A REVIEW OF SOME FACTS CONNECTED WITH GUNSHOT WOUNDS OF THE ABDOMEN, AND PRACTICAL DEDUCTIONS THEREFROM.

UNE REVUE DE QUELQUES FAITS LIÉS AUX COUPS DE FEU DE L'ABDOMEN, ET DES DÉDUCTIONS PRATICABLES DE CELA.

ÜBERSICHT EINIGER MIT SCHUSSWUNDEN DES UNTERLEIBES VERBUNDENEN THATSACHEN UND PRAKTISCHE AUSZÜGE DARAUS.

The object of this contribution to the subject of gunshot wounds of the abdomen is to pass in review such facts as are at our disposal in its consideration, to make such deductions from these as their limited number will afford, and to offer some suggestions based on personal experience.

Probably no question submitted to the consideration of surgeons has ever arrested the attention of the profession more promptly than the general question of surgical interference in penetrating gunshot wounds of the abdominal cavity, and it is at once remarkable, and to the honor of the profession, that the obvious deductions have been as promptly applied.

Up to 1885 the whole number of recorded operations for gunshot wounds of the abdomen, that I have been able to find, is six; by this is meant cases in which the surgeon has deliberately sought out the wounded intestines and repaired the damage inflicted, in accordance with surgical principles. Surely, a small number, in view of the wars which have gone by, contributing and bringing under the surgeon's care great numbers of these injuries under consideration, and the many individuals shot through the abdomen in the brawls of civil life, also placed under medical supervision.

Up to 1885 the profession had not looked the real question square in the face; surgeons held uncertain opinions, with the large majority opposed to any interference whatever, and, as might be expected under such complicating conditions, the results were not brilliant. Operations previous to three years ago were the exception; the magnitude and importance of the subject seemed not to be realized.

Now, I would venture the assertion that there are few modern surgeons who, confronted with a bullet wound of the abdominal walls and not able to convince themselves that the ball had not effected a penetration, but would explore the cavity.

When, in the winter of 1884, I reflected on the necessity of systematically and experimentally studying this subject, I did not anticipate that in so short a time such a radical change would take place in the method of treating such cases, which previously had been relegated to cure by opium, rest and hopes in Providence.

The results of my observations were published in the *Journal of the American Medical Association* in 1885; they were the observations and outgrowing deductions from a series of experiments systematically carried out during the previous winter, for the purpose of throwing light upon the pathology and treatment of these injuries, and of recording the clinical facts attending shot wounds of these organs.

Since the publication of my address to the American Medical Association in 1885, thirty-six cases of operative interference in gunshot wounds of the abdomen have been recorded, with nine recoveries following opening the abdomen, suturing the wounded intestines and treating other complicating injuries.

Sir William MacCormac, in the Annual Oration delivered by him May 2, 1887, before the Medical Society of London, has collected from all sources thirty cases. To these must be added one case reported by Prof. McGraw, of Detroit, of double perforation of the ascending colon, exposed by enlarging the surface wound and suturing the intestinal perforations; recovery following. Another by Dr. J. B. Murphy, of Chicago, of shot wound of the liver, in which the abdomen was opened, blood clots removed, and the wounds sutured; recovery following. Also a death, reported by Dr. J. B. Murphy, from post-peritoneal hemorrhage; post-mortem showing the intestinal wounds to have been safely sutured. One other case of recovery is reported by Dr. J. J. Skelly, of Potomac, Ill., and two fatal cases coming under my own care, to which further reference will be made in this paper; in all, thirty-six cases with nine recoveries.

A reference to the extended reports of these cases, or to the tables of Sir William MacCormac, shows plainly that there has been no selection of favorable conditions; that the operations have been done under a great diversity of surroundings, without special assistants, and in many cases with injuries the fatality of which it seemed impossible to overcome.

These results furnish the greatest encouragement for further trials in saving the lives of persons so certainly condemned to death unless relieved by operation, when suffering from the wounds under consideration.

Every case, whether of recovery or death, following operation should be published in full, so that our experience may be increased, the nature and character of these wounds better understood, and definite rules of procedure elucidated. We might in this way be able to set aside those cases which, from the special character of the wounds, will necessarily prove fatal; with our present limited knowledge of reliable symptoms all is uncertain with an unopened abdomen. Gradually we may be able to positively recognize those cases which possess "a faint hope of relief," to be followed by a good percentage of recoveries after operation. Where to draw the line and what to do when operation is decided upon, and how best to carry out the necessary manipulations, are the questions which the future must settle.

Judging from the valuable papers of Drs. Bull, Dennis and Bryant, of New York city; Dr. Tremaine, of Buffalo; Dr. Nancrede, of Philadelphia; Dr. Senn, of Milwaukee; Dr. Marcy, of Boston; Surgeon-general Hamilton, of Washington; Sir William MacCormac and others; and from the expression of opinion published as coming from the surgeons present at the last meeting of the American Surgical Association, it is certainly just to claim that the belief that surgical interference in proper cases is the accepted course to pursue, is rapidly being adopted by the profession at large.

To me, this is a great victory gained. The method of procedure has been tried, and, notwithstanding the crudest of data to build upon—the deficiencies in practical experience in man—with propositions already made, not thoroughly tested, or perhaps only superficially studied; with the dimmest of light for a guide, the results have been remarkably good.

What inferences are justifiable from an external gunshot wound of the abdominal walls?

A single wound of the abdominal walls, in so far as it affords any inference at all from its being single, furnishes a hope that no penetration of the peritoneal cavity has taken place, but it is merely a hope. The resistance of the walls and viscera, though considerable in every case, varies greatly. Consequently a bullet, although not having momentum enough to make an exit, may have force enough to do much damage among the viscera; or its momentum may be so slight, and its direction so oblique, as to cause it to remain between the planes of the abdominal walls.

Even the existence of an entrance and exit wound, widely separated, is not always a proof of injury to the viscera. Observations have shown, both on man and the lower animals, that a bullet may enter the abdominal walls at one point and reappear at another at a considerable distance from the first, and yet not enter the abdominal cavity.

If penetration be present with only a wound of entrance it suggests that the firearm used might have possessed moderate penetrating force, that the velocity of the bullet may not have been extreme, and that the damage done by it is very likely amenable to treatment with fair prospects of relief.

If there exists a continuous track of tenderness, especially if accompanied with slight redness, from the wound of entrance for some distance over the abdominal surface, it is fair to infer that the missile has wormed itself between the layers of the abdominal walls, and that penetration does not exist. This was plainly shown in a case seen in 1886 and reported by myself in a paper read in New York that year.

The peculiar appearance presented by the edges of the wound, and its size, when carefully studied, will furnish pretty reliable information of the size of the bullet, and its direction of impact, both items of considerable importance in estimating the possible damage.

Bullets from firearms of large calibre are the most destructive to the opposing tissues, and have the maximum penetrating force. A large bullet-hole argues a large bullet, certainty of penetration, and large destruction of tissues and organs.

Powder-marks on the clothing or body prove a close body shot, and hence greater probability of complete penetration of the abdominal cavity, with wounds to the viscera; and this is true, no matter what may be the calibre of the firearm used.

If the edges of the skin perforation are equally stained throughout, and clean cut, the fact suggests that the bullet struck perpendicularly to the surface upon which the wound is found. Again, if these edges are unequally stained, if unequally ragged, or if the surrounding surface shows a stain, or abrasion, or discoloration, leading to some portion of the edge of the perforation, all these facts suggest valuable information as to the probable course of the missile in its transit through the abdomen, and the conclusion is justifiable that the impact was not perpendicular to the surface, and, of course, in proportion as the course of impact departs from the perpendicular, the greater is the probability that penetration of the cavity has not occurred.

Naturally, one of the most important items of estimation is to determine the probable course of the injuring body. To this end information as definite as possible must be obtained as to the direction in which the shot came and the distance from which it was fired; both facts having great bearing on the organs wounded and the damage done them. It is no easy matter, even with very complete data to guide one, to feel certain as to the direction of the missile inside the cavity when there is only one wound. A great many cases will furnish no corroborative information; the surgeon will be compelled to depend upon the signs belonging to the wound itself.

If in doubt as to penetration, the wound should be enlarged by an incision directly through the skin perforation in some chosen direction. By carefully following the track of discoloration left in the tissues by the bullet, not only the fact of penetration or non-penetration will be positively determined, but its directness or obliquity through the abdominal walls will furnish positive information as to the course of transit of the entering body. With the usual precautions this incision will not increase the patient's danger, even if central section becomes necessary; it throws valuable light upon subsequent requirements, makes clear the fact of penetration or non-penetration, and, in some situations, may enable the operator to repair all the damage done.

The presence of a wound of entrance and exit, produced by the firearms and missiles of the present day, especially if the shot is delivered in close proximity to the body, with scarcely an exception possible, indicates injury to all the fixed organs lying in the estimated line drawn between the two external wounds made by the missiles. Moreover, it is highly probable that the small intestines are also damaged, although these latter wounds may be found some distance away from the line of the ball, their changed position being dependent upon the extreme mobility of the viscera at the time of the receipt of the wound, and from the movements of the body subsequent to the passage of the bullet, or other causes.

The great majority of double wounds tell positively of complete and direct perforation, and damage, more or less severe, to every organ in their path; there seems scarcely any probability of deviation from their course caused by the resistance of the soft tissues of the body.

Whether wounds in organs (as contended by Prof. McGraw) found some distance away from the line of transit of a bullet are to be explained by the elasticity and mobility of the tissues—their constant change of form by inherent contraction enabling them to get in or out of the way—or by subsequent changes induced by weight of the bullet or movements of the body, or, as contended by myself, are sometimes dependent upon an erratic course of the latter from deviations in its line of flight caused by deflections therefrom through impingement on tissues of different powers of resistance or elasticity, is a matter that must be settled by an appeal to physical laws through experimentation; it will never be settled by assertions or assumptions. I am fully convinced that the time does come in the "life" of a flying bullet when its veloc-

ity and power of penetration bear such a relation to the power of resistance of the different tissues in the abdominal walls and contents, that the softest of these, touched in a certain way, will deflect its course. In no other way than through this supposition have I been able to explain the character and kind of damage I have seen done by a bullet in its transit through the body.

My conclusions and deductions on the course of a bullet are based mainly upon the results of experimentation, during which the animal was profoundly anæsthetized, and, consequently, muscular contraction and movement abolished. If the ball deviated at all from a straight line, there was nothing else to cause the deviation but the soft tissues in its track.

The situation of the wounds will, of course, call attention to the likelihood of damage inflicted upon the organs in the probable course of the bullet. The severity of the injury and gravity of prognosis is surely greatly enhanced if the movable viscera are wounded. It is much less if only fixed organs are hit. In both, the absolutely necessary manipulations by the surgeon, required for the repair thereof, will be suggested, and due preparation to meet all indications can be provided for. There is no opportunity to hunt up necessary appliances after the operation is begun.

An antero-posterior shot, below the level of the umbilicus and well toward the lateral surfaces of the body, will be very likely to miss the small intestines entirely and expend its damage on the large bowel, as in Prof. McGraw's case. The same kind of wound high on the lateral surfaces may pass into or through the liver without injuring the intestines, or the spleen alone, if the entrance is on the left side.

If the wound is so situated that the bullet enters the abdomen through the diaphragm, adding injury of abdominal viscera to that of the contents of the chest, the surgeon's help will probably be of little use.

A wound of entrance and exit, or an entrance wound alone, showing perforation of the ball from side to side through the cavity, means the worst of injuries, and suggests the need of the greatest care in staying the hemorrhage, repair of intestines and toilet of the cavity.

Antero-posterior perforation, if complete, can only fail to wound the small intestines when situated well on the outskirts of the surface of the abdomen; seemingly, there can be no exception to this proposition, save in those extremely rare instances in which the perforating body traverses the cavity without injuring the contents.

Penetration through the posterior walls of the cavity, if complete, with likelihood of laceration of important fixed organs, argues an injury of the most severe character —one in which the surgeon's art will be of no avail in the majority of instances. The exceptions in which the severity will not prove unsurmountable, will be transit through the space between the lower end of the kidney and the crest of the ileum and in wounds occupying the outskirts of the entire posterior surface. If the penetration be incomplete, as can, in the majority of instances, be determined by enlarging the bullet wound, the injuries are by no means necessarily fatal, and do not require any other surgical interference than enlargement of the wound and proper dressing. Many instances are recorded of recovery from posterior penetration of the large and fixed viscera of the abdomen without any surgical operation whatever.

What collateral evidence influences the formation of a diagnosis?

The peculiarities of the individual injured constitute so important an element in the development of collateral manifestations, that all such testimony should be subjected to the most rigorous search; in fact, much value cannot be attached to subjective manifestations. It is not necessary to state to you that one person may be prostrated and literally frightened to death by the sound of a firearm or the "swish" of a bullet, while another will continue his course or perform his usual duties after he has been

injured, and can only with difficulty be persuaded that he has been shot. Between these two extremes all gradations present themselves.

There are other phenomena, independent of personal peculiarities, which contribute to the formation of the surgeon's opinion. Among these may be mentioned: Tympanitic resonance, unusual dullness on percussion, the presence of fecal matter or any of the normal secretions or contents of the different viscera in any of the external wounds, blood in the stools or urine, or egesta from the stomach, paralysis of any kind, persistent nausea and vomiting, and the general condition designated shock.

Allow me to briefly refer to the probable significance of these symptoms when present.

Unusual and rapidly-forming tympanites would suggest the escape of the intestinal gases into the peritoneal cavity through a perforation, and if found in a region of normal dullness, as in the liver region, it is considered good corroborative testimony in favor of intestinal rupture, by some authorities.

Circumscribed dullness on percussion, with localized bulging in the abdomen in the neighborhood of the wounds, or in the most dependent region of the cavity, argues the possible presence of blood accumulation from wound of a large vessel, and consequent penetration of the abdomen.

The rare but possible phenomenon of fecal matter appearing in the external wounds renders the demonstration of perforation of the alimentary canal absolute. That such extrusion does occur as an early symptom after wounds made by large bullets finds illustration in the case to be reported by myself in this paper.

The presence of blood in the urine, in connection with the situation of the external opening, demonstrates wound to the kidney, ureter or bladder; the two former adding greatly to the gravity of the prognosis, and, certainly, in so far as its presence influences opinion at all, each condition would favor the necessity for operative procedure.

Paralysis of any part of the body below the level of wounds in the abdomen necessarily complicates matters very much, rendering it very probable that the ball has not only injured the viscera in its course, but has also done irreparable damage to the spinal cord or important nerves.

"Shock" cannot be relied on as a positive indication of the presence or absence of perforation of the viscera. Cases with many perforations have presented no evidence of shock whatever. Its presence is rather an indication of some special nervous condition of the patient, of some injury to nervous structure, or, perhaps, more often than any other condition, it indicates the laceration of some large blood vessel, with free bleeding. The last, a condition of itself requiring abdominal section for its relief quite as surely as the rupture of the sac of tubal pregnancy, and proving quite as fatal if the operation is not done.

It is to me a source of disappointment to be compelled to put the presence of "shock" among the doubtful signs of perforation, for I was at one time fully convinced that its presence surely meant bowel wound, and I am still of the belief that when present the probability of such injury is very great.

Absence of pulsation in either of the femoral arteries will call attention to injury of the iliac vessels, and will, when present, give a second point with which to estimate the course of the bullet.

In three cases of penetrating wounds seen by me all had persistent nausea and vomiting present. Other reported cases have shown similar symptoms. It is also a common symptom with ruptured intestine from other causes, hence I deem it proper to claim that its import as a symptom be borne in mind.

What symptoms make it probable that the issue in any case will be fatal, whether operated upon or not?

It seems quite proper to say that the majority of cases of through and through perforations of the abdominal cavity, with injury to both fixed and movable viscera and blood vessels, will prove fatal in spite of the best efforts to save them. Still, it would not take much time or thought to bring to mind instances of individual experience, or cases of record, in which the injuries done to abdominal viscera, and the shock incidental to a prolonged operation performed in recognized procedures for the relief of abdominal tumor, have been quite as severe as could be produced by a bullet in transit through the cavity, and yet the patient has survived. So it becomes a difficult matter to decide when to decline operative interference. Cases of recovery have followed surgical care of the wounds when many perforations of the intestines* were found, and in which solid viscera† have been traversed by the bullet; many cases have perished, in which, after death, examination showed the simplest injury to repair, and indicated the probability of speedy recovery had the abdomen been opened at once and the wounds treated. One is almost tempted to say that all cases are entitled to the chance of life offered through operative procedure. It is hazardous to predict a fatal issue. However, if the abdominal wound is complicated with a severe injury of the spinal cord or bad wound of the solid viscera, or so great a time has elapsed as to allow of extensive extravasation and infiltration, with consequent virulent inflammation, the probability is that the issue will be fatal.

In application I will present the following cases:—

Mr. J. F. shot himself in two places, in rapid succession, with a 32-calibre revolver. I saw him four hours after the injury was produced, and found two bullet wounds four inches to the left of the median line, on the same line with each other, and one and one-half inches apart; the lower wound was even with the umbilicus. One bullet had gone through the body; its exit posteriorly was just below the last rib and close to the outer edge of the erector muscle. There had been, and still was, considerable hemorrhage going on from the posterior wound. He had eaten a hearty dinner just previous to the shooting. The patient was moderately collapsed, pulse very fast and countenance pale. By the time he was anæsthetized, and necessary preparations were made, darkness had come on, and the operation was done with the light from a single gas jet.

The two bullet wounds were joined by an incision, and the fact of penetration demonstrated. An opening was then made in the median line, four inches long, through the abdominal walls. Considerable blood was found in the peritoneal cavity; this was removed and the wounded intestines sought for. These were easily found, and the perforations quickly closed with a straight needle, carrying No. 1 silk thread. Five perforations were found and secured. On examining the posterior peritoneal surface, a bullet perforation was found in it directly over the body of the left kidney. On passing the finger through it, the kidney was found to be perforated by the bullet. The hemorrhage from the wound was at this time very slight. During this period operative procedures had to be discontinued several times to prevent the patient from choking during his attempts to evacuate the stomach, as he was vomiting large masses of meat and other food. The kidney was not removed. The wounds were closed, dressed antiseptically and the patient put to bed. He rallied fairly well in a few hours, and seemed to progress nicely for twenty-four hours, when he began rather suddenly to fail rapidly, and died in collapse. There had been considerable bleeding from the posterior wound, and the patient died from hemorrhage from the wound in the kidney. It is to be regretted that the organ was not removed. The other wounds were comparatively simple in character and easily secured. There was but slight extravasation, and the cavity was left quite clean.

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* Hamilton, Bull. † Murphy.

P. J., aged 45 years, was shot while walking in the street on the night of July 4th. He was seen by me at the Presbyterian Hospital sixteen hours after the injury was inflicted. I found a large-sized bullet wound in the right iliac region, slightly below and two inches inside of the right anterior superior spine. The surface had been rendered aseptic and the patient anæsthetized before I examined him. The edges of the wound were more ragged and more deeply discolored on the outer and upper portion, showing that the missile struck obliquely to the surface of the abdomen, and that its direction was from the right and above. Considerable fecal matter was found in the wound. The man held his right thigh semiflexed, even when fairly anæsthetized. The right half of the abdomen and the upper portion of the thigh and buttock were fiery red in color, the margin of the erythematous blush being well marked. The respiration was entirely thoracic, the abdominal walls hard and motionless. An incision was at once made in the mid-line after emptying the bladder of a pint of urine. As soon as the peritoneum was opened large quantities of a stinking ichorous serum poured out, bringing with it fecal matter and small pieces of potato undigested. This material was washed away by a free flow of mild boric acid solution. The wounded intestine was then sought, and after drawing out about six inches of badly-inflamed tube, it was reached. It was a large perforation of the small intestine, but was secured by the continuous suture without difficulty. All the folds of the bowel in the iliac fossa and pelvis were examined, but no other openings found. A question of perforation of the bladder was raised, but a thorough examination with finger failed to find any. To further prove the uninjured condition of the bladder it was distended with warm milk, but no leakage was noticed, and the milk was allowed to flow away through the catheter. The external bullet wound was enlarged and its track followed by the finger. The missile barely entered the abdominal cavity at the fold between the abdominal walls and iliac fossa, and just outside of the femoral vessels, and was then lost in the soft parts of the thigh on the posterior surface. It evidently gouged out the convexity of the knuckle of intestines lying in its course.

After thorough cleansing, the wounds were all closed and drainage left in the abdominal cavity. It was now noticed that the patient had abdominal respiration and straightened his right leg. The operation was done in the presence of Profs. Gunn, Etheridge and Merriman, and Drs. Talbot, Mellish, Ward and others. All expressed their belief that no other incision than that through the mid-line would have enabled the surgeon to as easily and positively repair the injury and cleanse the cavity. The man died sixteen hours after the operation. With such extensive extravasation and virulent peritonitis as was found, no other result could be anticipated. With an early operation I believe the recovery would have been prompt in a case with so simple a wound and the absence of any complication. The case certainly points to the necessity of prompt relief in these injuries. The post-mortem confirmed all the facts ascertained during the operation, and I have present a section of the bowel showing the wound and the condition of the suture used to close it.

One can fairly believe that the abdominal incision adds but little to the patient's danger, and if there be any, it is quite offset by the benefits accruing from a perfect knowledge of the wounded person's true condition, as is exemplified in the following case :—

Dr. John I. Skelly, of Pontiac, Ill., reports, in the July number of *The Annals of Surgery*, a case of penetrating shot wound of the abdomen. The cavity was opened by median section, no injury to the intestines was found, the bleeding was controlled, the peritoneal wounds sutured, and the patient recovered. The injury was done by a bullet from a 32-calibre revolver. Great shock was present in this case, although no important viscera were wounded. Dr. Skelly refers especially to the great confidence in recovery expressed by the patient, when assured that the intestines were not injured.

What technical measures are best in the treatment of bullet wounds of the intestines, mesentery, stomach, kidneys, spleen, liver and bladder?

It is yet my firm conviction that, in the great majority of cases, the incision in the mid-line will allow the most room for all the manipulations absolutely necessary on the surgeon's part, and yet be conducive of rapidity of action. It will furnish the surest way of following the course of the bullet, and thereby enhance the certainty of securing all injured viscera in all cases of through-and-through perforation, especially if the course of the ball is transverse, oblique or median. It will furnish the best way of reaching all parts of the cavity through which to insure perfect toilet of the peritoneum. Prof. McGraw's case shows there may be exceptions to what seems should be the general rule. Here, as elsewhere, each case has its own indications, and must be managed according to them.

It seems proper for me to refer to a few conditions found in the wound of the intestine itself, dependent upon the character of the ball producing it, previous to passing in review the means to be adopted for its closure.

The character of the wound in the bowel depends greatly upon the size and shape of the bullet producing it, and much, also, upon the velocity of the missile. Round bullets moving rapidly make a clean-cut, rather small wound, with the minimum amount of bruising, so that they are comparatively easy to close. Rapidly-moving conical bullets do much more damage than the round; still, even with these the great velocity makes the injury less severe than might be expected from such terribly destructive agents. The extent of bruising is greater, the edges of the perforation are more ragged; still, if they strike the tube fairly in the lateral surface, many such wounds can be safely managed without resection. The greatest amount of damage, in my experience, is done by the rather slowly-moving missile, be it either round or conical. These tear, mash and lacerate the tissues instead of making a clean punch, like the swift ones.

However made, the large proportion of perforations in the bowel will be well secured, and quite rapidly, by means of the continuous stitch, applied so as to invert the edges of the wound toward the lumen of the tube, by entering the needle a safe distance away from the margin of the wound, and sewing over and over until all of it is covered with the opposite surfaces of peritoneum, held together by the stitches.

As the result of experimental research, the following statement was made by myself in 1885, in an address before the American Medical Association, written on this subject: "This way (that is, the use of the continuous suture) of treating the bullet openings in the bowel is susceptible of much wider application than would appear possible at first glance. I am quite well satisfied that it will take the place of excision in not a few cases of quite severe injury. The torn edges of the wound can be turned in, and peritoneal surfaces fastened together, even in large wounds, with perfect confidence in the result of safe and secure adhesion following."

This statement has been borne out absolutely in my own experience since then in the human being, and I believe it is the experience of all other operators. In no instance, in any of the reported cases submitted to surgical treatment since then, do I remember that the operator has been called upon to make a section of the bowel. All seem to have trusted to the continuous suture over the inverted wound. The recoveries are a positive evidence of its success, and none of the fatal cases show a failure to secure the bowel wound by its use.

It apparently makes no difference whether the wound in the bowel is closed parallel to the course of the tube or transversely or obliquely; the result is the same, provided the stitches are securely taken. Of course, the most easy and most rapid method of procedure is the best to be adopted, and of this the operator must be the judge.

With a through-and-through penetration of the cavity we may expect to find clean

perforations and the opening of minimum size in the intestine ; with a single entrance wound, arguing diminished velocity of the bullet, the tube openings will, very likely, be ragged, bruised and difficult to repair without sacrifice of intestines.

I show you, on this platter, an actual representation of the damage done to the intestines of a dog by a small 22-calibre bullet. Its force was apparently entirely expended in this injury to the bowel, and that, as can be seen, is well near total destruction.

If the perforations found in the bowel are situated fairly away from the mesenteric surface, little difficulty will be found in carrying out the manipulations necessary for their closure.

Still, in cases in which many perforations of the tube are close together, the intervening portions between the wounds have their vitality so greatly impaired by contusion that complete resection of the implicated portion of intestine will be required.

When the ball opening is directly at the mesenteric junction, repeated instances of imperfect union, followed by extravasation, have occurred to me in experimentation. This kind of injury requires exceptional care in the application of the sutures, so that they include something more than the peritoneal covering, and do not include the larger blood vessels entering the coats in this position.

When the injury is so extensive as to positively require resection of the wounded portion, my experience, from experimental inquiry, was positively in favor of two methods of procedure. 1st, Cases in which the mesenteric border could not be saved were most successfully treated by making the section in healthy bowel tissue, and removing the injured portion, with a triangular piece of the mesentery—the base of the triangle representing the length of intestine removed. The first sutures are best introduced at the mesenteric border of the divided ends of the intestines, because this plan furnishes more room in which to make sure of the engagement of sufficient tissue in the loop of the suture to make a fast and secure hold, than there would be if the other portions of the circumference were united before reaching this border. Failure to get good union and to avoid extravasation followed in every case in which this plan was not adopted.

Neither in man nor animal have I found it necessary to introduce more than one row of sutures, either in the repair of single perforations or in complete resection, provided the hold of the suture included about one-third of an inch of peritoneum, with underlying muscular coat, and the sutures were placed about one-eighth of an inch apart.

In no instance, in my experience, except when drawn too tightly, have the sutures failed to perfectly close the opening, so that at the end of twelve hours sufficient plastic adhesion had taken place to resist powerful hydrostatic pressure, and that, too, in cases in which there were thirteen perforations in eighteen inches of intestine.

That method which safely accomplishes the object of surgical interference in the quickest possible time, and with the least possible disturbance of the viscera locally or in general, is the best one to adopt ; saving of time alone is of vital importance to the patient.

The edges of the divided mesentery should be sutured and all raw surfaces covered with peritoneum by means of stitches, with very fine catgut or silk, in order to avoid leaving any secreting surfaces free in the peritoneal cavity.

2d, Cases in which the mesenteric or nutritive border can be saved. Here is an illustration of a plan which I have successfully adopted experimentally : The wounded part is cleanly cut out, leaving the uninjured mesenteric portion. From this the mucous membrane is stripped, and the muscular coat, with its peritoneal covering, drawn downward in a loop. This loop is closed with stitches and the bowel circumference remaining fastened as in complete resection. This method produces no flexure of the

bowel and does not interfere with the free flow of blood in the vessels coming from the mesentery.

The most reliable and safest clamps for use in holding the bowel during the manipulations of making a resection were found in experimentation to be the fingers of an assistant, and further experience has not changed the results of that observation; they can do the least damage and produce the least amount of shock, and will prove an intelligent aid to the operator.

The wounds of the large intestines can be rapidly, and usually easily, repaired by means of the continuous stitch, on account of their large calibre and comparatively thick walls. Here, if anywhere, the wounded bowel can be reached through an enlargement of the external bullet opening, as has been successfully done in one case by Prof. McGraw, of Detroit; but this is only possible, in my opinion, in cases in which the shot is a direct antero-posterior one, over the course of the large intestines, and does not touch any small intestines. It seems impossible to me so easily to find the injured parts, or so rapidly repair them, or to carry out so successfully proper cleansing of the cavity, through any other incision than the median section, in oblique or through-and-through penetration, in any transverse plane of the abdominal cavity. My belief is based upon trials on the cadaver and living animals and wounded men. No other incision, to my mind, gives such perfect command of the entire cavity.

In one instance an incision extending over the entire length of the lateral surface of the abdomen and fully to the middle of Poupart's ligament failed to enable the operator to find the vessel from which the fatal hemorrhage came. The track of the bullet could be traced to the opposite side of the cavity, but the intestine could not be drawn through this lateral incision so as to properly explore the course of the ball. There was no wound of exit. It seems very probable that the median section, by bisecting the bullet's course, and allowing easier access to the cavity, would have made this case, as it will others, simpler to manage, at least.

It is a matter of record in surgical experience, that the wounds confined to the large intestines have often been recovered without surgical interference; still, it is certainly probable that the number of recoveries will be increased and rapidity of restoration to health more surely provided for by closing the wound in the intestine and cleansing the cavity at once, and without adding materially to the patient's danger.

In animals, and probably likewise in man, a perforation of the great omentum is followed sometimes by a universal extravasation of blood throughout the meshes of the mass, producing a condition that requires ablation of the greater portion, after proper ligation. The end of the stump left after separation can be covered by sewing adjoining surfaces of peritoneum over it. All slits or openings in the mesentery should be carefully closed with the continuous suture, so as to avoid contaminating secretion into the peritoneal cavity.

The wound in perforations of the stomach is occasionally difficult to find, but when found no difficulty is met with in applying the means of closure. The continuous suture has not failed to securely fasten them, and in every instance they have been followed by speedy recovery, unless they were complicated by severe injuries to other viscera.

There are on record quite a number of cases of penetration or perforation of the liver alone, followed by recovery. If in doubt, with a posterior wound of entrance, enlargement thereof, with antiseptic care and dressing, would be justifiable. With an anterior wound, the course pursued by Dr. Murphy, of Chicago, in a case reported by him, was followed by recovery. Median section was made, the cavity cleaned of blood, and the wounds on the surface of the liver drawn together with catgut sutures. In my experiments, wounds of the liver were managed in the same way, and did well if the sutures were deeply placed.

Wounds of the spleen bleed freely and are difficult to manage with sutures, on account of the brittle nature of the spleen tissue; still, sutures rather deeply placed will hold the edges of the perforation in apposition. If badly lacerated, the many reported cases of recovery after complete removal of the spleen for injury rather indicate that extirpation is the best means of treatment in such injuries.

Perforation and wounds of the kidney, from the character of the organ and the profuse hemorrhage from its torn surface, from the danger of urinary infiltration and decomposition, seem impossible to manage without extirpation, especially if injured by an anterior wound of entrance. In one case of my own a complete perforation of the kidney was found. It was decided to leave the kidney. The patient did well for about twenty-four hours, and then succumbed to a profuse hemorrhage from the wounded organ. It seems the chances would have been better with it out.

If the kidney is wounded, with posterior opening only, and enlargement thereof shows the injury to be confined to this organ alone, the cavity of the abdomen is not perforated, and recovery is possible, either with or without removal of the organ.

Perforation or damage done to either the liver, spleen or kidneys accompanying similar injury to the small intestines greatly increases the gravity of the case, and probably very few cases will recover, whatever is done for their relief.

Sir William MacCormac has positively demonstrated the success following abdominal section in ruptures of the bladder, in order to securely suture the wound. It is proper to infer that bullet perforation of the viscus can be quite as easily secured in the same way.

The results of the experience of Varick, of New Jersey, and Wylie, of New York, should always be borne in mind. They have demonstrated that hot water introduced into the peritoneal cavity accomplishes three purposes of great moment—relief of shock, arrest or abatement of hemorrhage, and cleansing of the cavity.

I think carbolized silk, of fine size, is the best material to use for the bowel suture, simply because perfectly reliable catgut cannot always be obtained, and the risk is too great if there be the least likelihood of any strand giving way. No doubt, well-prepared catgut may answer every purpose, but the silk never fails to do the work required of it satisfactorily.

If asked what are the points most likely to be neglected or slighted in such an undertaking as giving surgical relief to a case of perforating gunshot wound of the abdominal viscera, my attention would be drawn to the items leading to failure in experimentation, and to the conditions mentioned as found in the repeated unsuccessful cases in man. Among these would come first the paramount necessity of searching out and securing all bleeding vessels dependent upon the danger of immediate or secondary hemorrhage. Surgeon-general Hamilton tells us that his successful case passed through a period of extreme danger, in the last days of his illness, from the formation of a blood tumor. Murphy, of Chicago, reports a case lost from post-peritoneal bleeding. And in this case post-mortem showed all the intestinal wounds thoroughly closed and water-tight. I have reported a case in which the immediate cause of death was kidney hemorrhage.

It is, no doubt, a hazardous ordeal to put a patient through, to examine the intestines from one end to the other, in order to be well satisfied that no perforation had been overlooked; yet it is far more hazardous (in fact, the result will be surely fatal) to leave an opening in the small intestines untreated. In some of the reported cases wide-open bullet wounds have been found, with their surrounding fecal extravasation and contaminated blood.

It is to me extremely doubtful if all the wounded parts will be found in an estimated transverse plane drawn through the demonstrated track of the bullet, especially if the missile implicates the ever-gliding and moving small intestines. I am not prepared to believe that a supposed probability as to the seat of injured parts should take

the place of a regular, carefully-made and satisfactory search for the wounds, and yet I would very carefully avoid practicing or advising any procedure that might unnecessarily add to the shock already present. We do not know all that it is best to do yet, and still, we do know that failure to close all the wounds means death to the patient, and some risk must be taken to avoid so great a hazard.

It needs no argument or demonstration to prove the harm resulting from tight suturing. It has been my experience to see, in animals, the edges of several wounds slough away to the extent of the bowel tissue included in the sutures, followed by extravasation, making a failure out of a case that, otherwise, gave good promise of being a success. The temptation is great to be over-sure of good union. In my experience, peritoneal surfaces need only to be laid in contact with each other and kept quiet for a few hours in order that adhesion may occur. The paralyzed condition of the bowel at the seat of wound, from the injury in itself, favors this desirable quiet.

Dr. NICHOLAS SENN then read the following paper :—

AN EXPERIMENTAL CONTRIBUTION TO INTESTINAL SURGERY, WITH SPECIAL REFERENCE TO THE TREATMENT OF INTESTINAL OBSTRUCTION.

UNE CONTRIBUTION EXPERIMENTALE À LA CHIRURGIE DES INTESTINS AVEC RAPPORT SPECIAL AU TRAITEMENT DE L'OBSTRUCTION DES INTESTINS.

EIN EXPERIMENTALER BEITRAG ZUR CHIRURGIE DER EINGEWEIDE MIT BESONDERER BEZIEHUNG ZUR BEHANDLUNG DER VERSTOPFUNG DER EINGEWEIDE.

BY N. SENN, M. D., PH.D.,

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The most important, and, at the same time, the most popular topic for discussion among surgeons of the present day, is intestinal surgery. The current medical literature is teeming with reports of cases, and at the meetings of almost every medical and surgical society, large or small, this subject comes up for discussion, and occupies a liberal and conspicuous place in their printed transactions. The unusual activity which has been manifested in all parts of the civilized world in the development of this, one of the most modern and aggressive departments of abdominal surgery, is sufficient evidence that the subject is comparatively new, and, as yet, imperfectly understood. A study of the literature of intestinal surgery must convince every unprejudiced mind that here, as in many other difficult problems in surgery, the positive knowledge which we have acquired rests almost exclusively on the results obtained by experimental research. Gunshot wounds of the abdominal cavity have been made the object of careful and patient experimentation by a number of enthusiastic surgeons, and the results obtained have laid the foundation for a rational method of treatment of these injuries, which has been eagerly accepted by all modern aggressive and progressive surgeons. The practical results which have been obtained thus far in the hands of a number of surgeons have been the means of saving a number of lives which, by the old conservative method of treatment, would have been doomed to inevitable death from hemorrhage or septic peritonitis. The numerous valuable practical suggestions for treatment of gunshot injuries of the intestines, the direct outcome of experiments on animals, as well as the remarkable recoveries following gunshot

wounds of the abdomen treated by laparotomy, have so firmly convinced the profession of the necessity of resorting to operative measures in such cases that few surgeons could be found at the present day who would be willing to trust to conservative treatment any case where positive, or only probable, evidences pointed toward the existence of a visceral injury of any portion of the intestine. While a decided advance has been made in the treatment of injuries of the intestinal tract, the operative treatment of intestinal obstruction still constitutes one of the darkest and most unsatisfactory chapters in the wide domain of intestinal surgery. The obscurity and uncertainty which cling to this subject are due to the difficulties which often surround an accurate diagnosis. At the same time we have every reason to believe that the appalling mortality which has so far attended the surgical treatment of intestinal obstruction is mainly due to late operations, and not infrequently to a faulty technique in the removal of the cause of the obstruction, and in the restoration of the continuity of the intestinal canal. An accurate anatomical or pathological diagnosis in such cases during life is often difficult, if not impossible, and when as a *dernier ressort* laparotomy is performed, and the surgeon is confronted by an unexpected condition of things, he is often in doubt as to what course to pursue, and frequently ends the operation by establishing an artificial anus. No one who has been forced to resort to this measure has left his patient with a feeling of satisfaction, as he must have been sadly impressed with the fact that at best he has only been instrumental in relieving the urgent symptoms of the obstruction, while he has failed to remove its cause, and, consequently, also in restoring the continuity of the intestinal canal. A patient with an artificial anus is indeed an object of commiseration, as experience has sufficiently demonstrated how difficult it is, in many instances, to close the abnormal outlet even after the cause of obstruction is subsequently removed, or corrected spontaneously without exposing him a second time to the risks of life incident to another abdominal section. If the causes which have led to the obstruction are of a permanent character, all attempts at closing the fistulous opening will, of course, prove worse than useless, and the patient is condemned to suffer from this loathsome condition the balance of his or her lifetime without a hope of ultimate relief. I believe I can safely make the statement, without fear of contradiction, that most of these unfortunate patients would prefer death itself to such a life of misery. The ideal of an operation for intestinal obstruction embraces the fulfillment of two principal indications:—

(1) The removal or rendering harmless the cause of obstruction.

(2) The immediate restoration of the continuity of the intestinal canal.

To meet the first indication the cause of obstruction must be found, its nature determined, and, whenever advisable or practicable, it is removed, a step in the operation which may be very easy, or may demand a most formidable and serious undertaking, more especially in cases where the pathological conditions which have given rise to the obstruction are of such a nature as to constitute in themselves an imminent or remote source of danger, as, for instance, malignant disease or gangrene of the bowel from constriction. In all cases of inoperable conditions the cause of obstruction is rendered harmless, as far as obstruction is concerned, by establishing an anastomosis between the bowel above and below the obstruction, by an operation which will be described further on. Immediate restoration of the continuity of the intestinal canal should be secured in the operative treatment of all cases of intestinal obstruction, with the exception of inoperable cases of carcinoma of the rectum, but is most urgently indicated in cases of obstruction in the upper portion of the small intestines and the colon, as the formation of an artificial anus in the former locality would prove a direct source of danger from marasmus, by excluding too large a surface for intestinal digestion and absorption, while in the latter situation the cure of a fecal fistula only too often proves an opprobrium of surgery. A careful perusal of the literature on the

treatment of intestinal obstruction proves only too plainly the imperfection of this branch of surgery. The rules laid down in our text-books are often given with so much hesitation, reserve and ambiguity, that it becomes impossible to apply them in practice. Opinions are so widely at variance that every surgeon finally acts upon the impulse of the moment and adopts a method which he deems appropriate for his case. It can be said that no uniformity of action exists, consequently the statistics which have been produced so far are of but little value from a practical standpoint. A rational and successful surgical treatment of intestinal obstruction, like other abdominal operations, can only be established upon a basis founded upon the results obtained by experimental investigation. In view of this fact, it is astonishing that so little has been accomplished in this direction. I am convinced that accurate work of this kind will render essential information in the diagnosis of the obscure causes of obstruction and will point out more clearly the indications for operative interference, while improved methods of operation will have to be studied exclusively in this manner. During the last eighteen months I have made 150 operations on animals, for the purpose of studying the effects of the principal varieties of intestinal obstruction which were produced artificially; at the same time I have attempted to establish a number of new operations for the relief of certain forms of intestinal obstruction where it is impossible or inadvisable to remove the local conditions which gave rise to the obstruction. One of the greatest dangers in all operations for intestinal obstruction is the length of time required to perform the ordinary operations, hence it has been my object to simplify the operations, and thus, by shortening the time, diminish the danger from shock. All patients requiring an operation for intestinal obstruction are invariably in a condition not well adapted for prolonged operations which necessitate the opening of the peritoneal cavity and exposure of its contents to the cooling influences of the atmospheric air. An operation which can be completed in twenty minutes must certainly prove less disastrous to the patient than one requiring from one to two hours. A prolonged operation on the intestines is attended by two great risks: (1) Immediate, due to shock; (2) Remote, from prolonged exposure to infection. Both of these dangers are diminished in proportion to the shortening of the time consumed in the operation, which is made possible by resorting to simpler measures, provided they are equally safe and efficient.

GENERAL REMARKS ON EXPERIMENTS.

With few exceptions, the experiments detailed in this paper were made at the Milwaukee County Hospital, located at Wauwatosa, six miles from Milwaukee. I wish, on this occasion, to return my thanks to Dr. M. E, Connel, Superintendent of the Hospital, and his assistants, as well as to Dr. William Mackie, of this city, for valuable services rendered in my experimental work. As the main object of these experiments was not to show a favorable statistic, but more for the purpose of studying the effect of different forms of intestinal obstruction and to establish new principles of treatment, the animals were not submitted to any special treatment before or after the operation; the diet was not restricted and no internal medicines were given. I pursued this course so as to bring the intestinal canal in the most unfavorable conditions for operative interference, so as to expose the operations to the severest test. Ether was used exclusively as an anæsthetic. The abdomen was shaved thoroughly, washed with soap and warm water, and disinfected with a 1-1000 solution of corrosive sublimate or a two-and-a-half per cent. solution of carbolic acid. For the sponges the same solution of carbolic acid, or a weaker solution of corrosive sublimate, was used. The abdomen was covered by several layers of aseptic gauze, with a slit in the centre. Whenever division or incision of the bowel was made, fecal extravasation was guarded against by compressing the bowel on each side, by compressors made for this special

purpose, or by constriction with an elastic rubber band. Experience showed that the latter method was preferable, as it proved less injurious to the tisues of the bowel and afforded greater security against extravasation, while at the same time it proved less disastrous to the circulation between the points of compression. The rubber bands for this purpose should be about an eighth of an inch in width, rendered properly aseptic by prolonged immersion in a five per cent. solution of carbolic acid, and can be readily applied by perforating the mesentery with an ordinary hæmostatic forceps at a point not supplied with visible blood vessels, and tied in a loop with sufficient firmness to obstruct the lumen of the bowel. Elastic constriction, practiced in this manner, prevents all possibility of extravasation, and does not interfere with the free manipulations of the operator, as is the case with clamps or the hands of an assistant, while the degree of compression that is necessary exerts no injurious effects on the vessels and tissues at the seat of constriction. Drainage was never resorted to, and the abdominal wound was always closed by deep interrupted sutures, including the peritoneum. In all cases where partial or complete exventration was made necessary, the bowels were kept covered with warm gauze compressed. In all cases where complete exventration became necessary, and where the bowels remained out of the abdomen for half an hour or more, a certain degree of shock was always noticed, and a number of animals died within a few hours after the operation, death being referable directly to this cause. For an external dressing we used iodoform ointment, applied directly over the wound, and a compress of cotton retained by a bandage, and a jacket made of coarse cloth. As a rule, the sutures were removed at the end of six days, when the wound was usually found healed by primary union.

I. ARTIFICIAL INTESTINAL OBSTRUCTION.

In imitation of the more common forms of intestinal obstruction in the human subject, due to congenital malformations or pathological conditions, the following kinds of obstruction were produced on animals: (1) stenosis; (2) flexion; (3) volvulus; (4) invagination. It is a noteworthy fact that, even in cases where the obstruction was complete from the beginning, vomiting was moderate, and in some instances entirely absent. As vomiting constitutes one of the earliest and most conspicuous and persistent symptoms in most cases of intestinal obstruction in man, we can only explain its lesser intensity or complete absence in animals from the circumstance that animals suffering from this condition, as a rule, refuse all food and drink. As a rule, the tympanites was also less marked than in the human subject.

1. STENOSIS.

Circular narrowing of the lumen of the vessel was produced by excision of a semilunar piece of the intestinal wall and double suturing of the wound in a direction parallel to the intestine; and (2) circular constriction with bands of aseptic gauze.

(*A*) PARTIAL ENTERECTOMY.

EXPERIMENT I.—Dog, weight 39 lbs. A semilunar portion, embracing half the circumference of the bowel, removed from the convex surface, two inches above the ileo-cæcal valve. Wound closed in a longitudinal direction by Czerny-Lembert suture. The first two weeks the discharges from the bowels were fluid and dark in color, subsequently normal in color and consistence. Animal killed 36 days after operation. Body well nourished; abdominal wound indicated by a firm linear cicatrix. Omentum adherent at point of operation; lumen of bowel at point of operation reduced one-half in size; lumen of bowel above and below the contraction equal in size, showing that

the stenosis had not furnished an obstacle to the passage of intestinal contents. A few of the sutures remained attached, their free ends floating in the bowel.

EXPERIMENT II.—Large, full-grown cat. The same operation was performed on the concave side of the bowel about the middle of the ileum, a semilunar piece of the wall of the intestine with the corresponding mesentery being removed and the wound closed in a similar manner, which diminished the diameter of the lumen of the bowel to about one-eighth of an inch. It was noticed during the operation that the convex surface of the bowel, over an area corresponding to the partial excision, presented a cyanosed appearance. The animal died on the fourth day after operation, and the whole segment of the sutured bowel was found gangrenous, but no fluid in the abdominal cavity.

EXPERIMENT III.—Adult, large cat. In this case a segment of the ileum was emptied of its contents, and before cutting away a semilunar piece from the convex surface, a back-stitch continuous suture was applied on the inner margin of the proposed line of incision, which left about one-third of the lumen of the bowel. After excision of the semilunar piece the margins of the cut surface were turned inward and covered with serous surface by a continuous catgut suture. Several small passages occurred after the operation, but the animal died on the fourth day with symptoms of intestinal obstruction. The visceral wound was found healed, but the lumen had become so narrow from the inflammatory swelling of the tunics of the bowel, that it was entirely inadequate for the passage of intestinal contents, and as a result of this obstruction the bowel had become considerably dilated above the point of operation.

Remarks.—These experiments illustrate conclusively that in wounds of the convex side of the intestines, where from the nature of the injury transverse suturing is impossible, longitudinal approximation and suturing can be safely done, provided at least one-half of the lumen of the bowel can be preserved. If the stenosis is carried beyond this point there is great danger that the inflammatory swelling following the operation will still further narrow the tube and lead to the most serious consequences, due to intestinal obstruction, and place the visceral wound in the most unfavorable condition for the healing process.

Experiment II shows the great danger of interference with the blood supply from the mesentery in longitudinal suturing of wounds on the concave side of the bowel, as such a procedure is invariably followed by gangrene of the corresponding segment of bowel on the convex side.

(*B*) CIRCULAR CONSTRICTION.

The following experiments were made to study the effect of circular constriction upon the circulation of the isolated constricted loop of bowel. In all cases where the constriction was made with a gauze band this was tied with the same degree of firmness, so as to determine whether the same degree of strangulation would produce identical results.

EXPERIMENT IV.—Adult cat. A loop of bowel about the middle of the ileum, six inches in length, was tied with a band of aseptic gauze with sufficient firmness to cause slight congestion, but without interfering with a free arterial supply, as the arteries in the ligated portion continued to pulsate freely. The day after operation a few small fecal discharges stained with blood. The cat died forty-eight hours after the operation; no rise in temperature was observed, and death was evidently caused by collapse from perforation. The loop of bowel showed gangrene on convex side equidistant from the point of strangulation, and a small perforation which had given rise to diffuse septic peritonitis. The whole visceral and parietal peritoneum were uniformly affected and the peritoneal cavity contained a considerable quantity of sero-sanguinolent fluid.

EXPERIMENT V.—Large adult cat. A loop of the ileum of the same length was tied in a similar manner and with same degree of firmness. The animal absolutely refused food until the eighth day. Rise in temperature second and third day. Only one fecal discharge on the second day. Killed eight days after operation. Abdominal wound completely united; no peritonitis. Four inches of bowel below the point of constriction, showing that partial reduction had taken place. The gauze band was found completely covered with adherent omentum, and a thick layer of plastic lymph which formed a complete bridge connecting the intestine above and below the ligature. The ligated portion showed no evidences of defective circulation, and no ulceration underneath the ligature. The obstruction was complete, as no fluid could be forced through the bowel, and in proof that the same condition existed during life, it was found that the bowel above the constriction was considerably dilated, while below the strangulation it was empty and contracted.

EXPERIMENT VI.—Large Maltese cat. A loop of the ileum six inches in length tied in a similar manner. On the third day fæces stained with blood. On the same day the temperature, which had remained nearly normal until this time, rose to 105° F., and on the following day the animal died, having manifested symptoms of perforative peritonitis for twenty-four hours. Abdominal wound united; recent diffuse peritonitis. The abdominal cavity contained several ounces of sero-purulent fluid. Bowel above constriction distended with fluid contents, below the obstruction empty and slightly contracted. The greater portion of strangulated loop was found gangrenous and adherent to adjacent loops of bowel. Perforation had taken place in the middle of the loop on the convex surface, showing that gangrene had taken place first at this point and had extended from here toward the ligature.

EXPERIMENT VII.—Adult dog; weight twenty-six pounds. In this case an opening was made in the mesentery, through which a loop of the small intestine six inches in length was pushed. With sutures this opening was made sufficiently small so that its margins produced slight strangulation. The dog remained perfectly well after the operation, and was killed on the twenty-second day. Abdominal wound completely healed. No signs of peritonitis. On searching for the seat of obstruction it was found that spontaneous reduction had taken place, the site of perforation in the mesentery being indicated by a recent cicatrix.

Remarks.—The post-mortem appearances in these cases demonstrate clearly that the gangrene was not produced by the primary mechanical strangulation, but that it depended upon consecutive pathological changes in the loop or its vessels. In Experiment V, the primary strangulation was fully as great as in the preceding experiment, and yet gangrene did not take place, and we have positive proof that vascular engorgement in the ligated portion was less intense, from the fact that partial reduction took place. In all cases where gangrene resulted, it must not have been from deficient arterial blood supply, but from an obstruction to the return of blood through the veins. If the defective arterial blood supply had been the immediate cause of the gangrene we would have found more constantly gangrene of the entire loop, while every specimen illustrates that gangrene always commenced at a point where the return of venous blood met with the greatest resistance, viz., on the convex surface in the middle portion of the loop. As in cases of hernia, or in any other form of intestinal strangulation, where a firm constricting band surrounds the loop of bowel, the danger of complete strangulation is increased if, by the peristaltic action, additional portions of the intestine are forced through the ring, and the immediate cause of the gangrene is always referable to obstruction to the return of venous blood, which leads rapidly to œdema, complete stasis and moist gangrene in that portion where the venous circulation is most seriously compromised. Violent peristalsis, under such circumstances, always aggravates the existing conditions, and is often the precursor of symptoms of

complete strangulation. In such cases opiates act favorably by arresting peristaltic action, and in so doing may avert gangrene by preventing the causes which otherwise would have led to complete venous stasis.

2. FLEXION.

As many instances are on record where flexion of the bowel constituted the cause of intestinal obstruction, this condition was artificially produced in animals, either by making a partial enterectomy by removing a wedge-shaped piece from one side of the bowel, or by bending the bowel upon itself acutely and fixing it in this position with catgut sutures.

EXPERIMENT VIII.—Dog, weight sixty pounds. A wedge-shaped piece of the wall of the ileum was removed from the concave side with a corresponding portion of the mesenteric attachment, and after arresting the bleeding by tying several vessels with catgut, the wound was closed transversely by two rows of sutures. The excised piece measured one inch at its base, and the apex reached as far as the median line of the bowel. Immediately after excision, the convex portion of the bowel, which had become acutely flexed by uniting the wound, presented a livid, congested appearance, and after tying of the sutures the cyanosis increased. The area of disturbance of the circulation corresponded to the width of the base of the excised portion. About fourteen inches from this place a similar piece was excised from the convex side of the bowel, and the wound closed in the same manner. At this point the flexion was only slight, the mesenteric portion forming the prominence of the curve. On the third day the temperature rose to 105.6° F., and the following day the animal died with symptoms indicative of perforative peritonitis. On opening the abdomen, diffuse, general peritonitis was found, with numerous adhesions. Gangrene and perforation were found on convex side directly opposite the first operation. Second visceral wound closed and lumen of bowel at this point somewhat contracted, but permeable to fluids.

EXPERIMENT IX.—Large, adult cat. Removed from convex side of ileum a triangular piece measuring one inch at its base and the apex reaching a little beyond the middle line of the bowel. Wound closed transversely by Czerny-Lembert sutures. After closure of wound the bowel presented at point of partial resection an obtuse angle, the apex being formed by the mesenteric portion. The stools were bloody the second day after operation. The animal remained in excellent condition until it was killed, forty-three days after the operation. Adhesions of loops of small intestines to abdominal wound and of omentum and adjacent intestines at point of operation. The extent of flexion was found somewhat diminished, yet the concavity on convex side of bowel was well marked. Size of bowel above and below the operation was equal, showing that the flexion had not acted as cause of obstruction. On opening the bowel a pouch-like bulging was found on the mesenteric side, which appeared to compensate for the narrowing caused by the artificial stenosis. Two of the deep sutures still remained attached to the inner surface of the bowel.

EXPERIMENT X.—Adult, large cat. In this case a loop of the middle portion of the ileum, four inches in length, was acutely flexed in such a manner that the peritoneal surfaces of the convex side were brought in contact, and in this position the bowel was fixed by a number of fine catgut sutures. No symptoms pointing toward intestinal obstruction were observed, and the animal was killed sixteen days after the operation. Wound was found completely united, and no signs of peritonitis. The angle of flexion had somewhat diminished, but otherwise the bowel adherent in position left after operation. The bowel presented no dilatation above nor contraction below the flexion, showing that complete permeability of the canal at the point of flexion was quickly restored.

Remarks.—The partial excision on concave side of bowel in Experiment VIII illustrates the danger of suturing wounds in this locality, where the blood supply from the mesentery is likewise impaired, as gangrene of the remaining portion of the bowel is almost certain to take place. In all wounds on this side of the bowel more than half an inch in length, there is also another great danger which attends transverse suturing, viz., stenosis, which may become the cause of intestinal obstruction. As the small intestines naturally describe quite a strong curve, with the concavity on the mesenteric side, closure of a wound involving this portion of the bowel gives rise to acute flexion which, at least, during the process of healing, must cause more or less obstruction until, by yielding of the opposite portion of the intestinal wall, an adequate dilatation of the caliber of the tube has taken place. A considerable portion of the wall on the convex side of the bowel can be removed and sutured transversely until the bowel has been transformed into a straight tube, and a wound an inch in length will make only a slight flexion which furnishes no serious mechanical obstacle to the passage of the intestinal contents. In this connection the question arises, Does simple flexion, even if acute, without diminution of the lumen of the bowel, give rise to symptoms of obstruction? I have made numerous flexions when performing operations for establishing intestinal anastomosis, and in most instances satisfied myself, by examination of the specimens, that fluids passed them without great difficulty. If the bowel at the point of flexion remains free certain portions of its wall will yield to pressure of the fluid intestinal contents, and gradually the lumen of the bowel will become restored. If, on the other hand, the entire circumference of the bowel at the point of flexion has become fixed and immovable by inflammatory adhesions or other pathological products a compensating dilatation becomes impossible and the flexion becomes a direct and serious cause of obstruction.

3. VOLVULUS.

This condition, only another form of flexion, was experimentally produced by rotating a loop of intestine one and a half or two times around its axis and retaining it in this position by a number of fine sutures which were applied in places at the base of the volvulus, where fixation was most required.

EXPERIMENT XI.—Dog, weight 12 lbs. A loop of the ileum eight inches in length was brought out through a small incision, and the two tubes turned around their axis twice and the twist maintained by two catgut sutures. The constriction was sufficiently firm to cause considerable venous engorgement in the twisted loop. The dog manifested no unpleasant symptoms after the operation. The specimen was not obtained, as after a few days the dog ran away.

EXPERIMENT XII.—Medium-sized adult cat. In this case the volvulus was made by twisting a loop of the ileum about four inches in length twice around its axis, and retained in this position by a number of fine silk sutures. Vomited several times during the first day. The first three days in taking the temperature in the rectum the thermometer when taken out was bloody. The first two days the temperature was normal, followed by an increase to 104.6° F. and 103.2° F. the two succeeding days, when it again became normal. No constipation; appetite good throughout the whole time. Animal killed 22 days after operation. Abdominal wound completely united; no peritonitis. Volvulus remains as after operation, with the exception that where the bowel had been flattened by the twisting, it had, at least partially, resumed its tubular form. Serous surfaces, where approximated, had become firmly adherent at point of constriction; size of bowel considerably diminished. The twisted loop contained liquid fæces. Connecting the specimen with the faucet of a hydrant, water could be forced through, but on increasing the force of the current the peritoneum ruptured extensively in a longitudinal direction to point of partial obstruction.

Remarks.—These experiments are interesting, inasmuch as the primary constriction produced in making and maintaining the volvulus, which was sufficient to cause venous engorgement in the twisted loop, must have been only of short duration, the disappearance of the effects of constriction being undoubtedly due to the gradual yielding of the sutured parts, while the faulty axis of the twisted loop was maintained by the sutures; the circulation improved and remained in a sufficiently vigorous condition to adequately nourish the most distant portions of the volvulus. While it was found difficult to force fluid through the specimen of a volvulus, during life propulsion of the intestinal contents by peristaltic action was carried on in a satisfactory manner, as the bowel above the volvulus was not dilated, and contained no abnormal amount of fluid, and the animal manifested no symptoms indicative of intestinal obstruction.

4. INVAGINATION.

The most frequent, and, from a surgical standpoint, the most important form of intestinal obstruction is invagination. Leichtenstern and Leubuscher have made careful experimental studies to explain the mechanism and pathological conditions which give rise to this kind of intestinal obstruction, but in the following experiments this part of the subject was ignored, and the invaginations were made by direct manipulation. It was found impossible to make an invagination at any point as long as the bowel was in a condition of contraction, consequently it was always found necessary to wait until the peristaltic wave had passed by, or to cause relaxation by firm pressure continued for several minutes. Usually, it was found easy to produce an invagination of the bowel when in a state of relaxation, by indenting one side of the bowel, and pushing the pouch forward with a blunt instrument until the entire lumen of the intestine had passed into the section of the bowel below. After this was accomplished, further invagination was readily effected by manipulation consisting in pushing gently the intussusceptum and intussuscipiens in opposite directions. After I had learned by experience that disinvagination frequently takes place spontaneously, I resorted sometimes to suturing of the intussusceptum to the neck of the intussuscipiens for the purpose of maintaining the invagination. But even this expedient did not always succeed in retaining the malposition, as spontaneous reduction was observed in several of these cases.

EXPERIMENT XIII.—Adult cat. The lower portion of the ileum and the cæcum and upper portion of the colon were drawn forward into an incision through the linea alba, and five inches of the ileum were pushed into the colon through the ileo-cæcal valve, when the parts were replaced and the abdominal wound closed. For six days the animal had a temperature from 102.6° to 105° F. and suffered from tenesmus. The stools contained mucus and blood. After the sixth day the symptoms due to invagination subsided, and were replaced by symptoms of peritonitis. The animal was killed twenty-two days after operation. Great emaciation; abdominal wound completely united; diffuse purulent peritonitis. The disease had evidently commenced in the ileo-cæcal region, as at this point the pathological changes were found most advanced. Complete spontaneous reduction of the invagination; colon greatly distended and intensely congested.

EXPERIMENT XIV.—Large, adult cat. Invagination was made in the lower part of the ileum. Length of intussusceptum three inches. For nine days the scanty fecal discharges contained mucus and at times blood. On ninth day the temperature registered 105° F.; absolute refusal of food, and only occasional vomiting; death on the twenty-third day after invagination. Abdominal wound healed; small ventral hernia; no peritonitis. Apparently the greater portion of the intussusceptum had disappeared by sloughing, and the subsequent healing process had produced an acute flexion at the

neck of the intussuscipiens. Firm adhesions between peritoneal surfaces in the concavity of the flexion nearly an inch in length. Above this point the intestine enormously dilated and distended with fluid contents. Below the obstruction the bowel was found contracted and empty. Water could not be forced through the obstruction from either direction. On slitting open the bowel in a longitudinal direction it was found that the lumen at the point of flexion was contracted to such an extent that only a fine probe could be passed. On the concave side of the flexion the mucous membrane presented a prominence marked by a number of longitudinal ridges. These folds had undoubtedly acted like valves in completely preventing the passage of intestinal contents, and later of the injection of water. Death in this case resulted from intestinal obstruction caused by cicatricial contraction after the sloughing of the invaginated portion of the bowel.

EXPERIMENT XV.—Adult cat. Two inches of the ileum were invaginated into the colon and fixed by two fine silk sutures at the neck of the intussuscipiens. For two days after the invagination the stools were scanty and contained mucus and blood. On the third day the abdominal cavity was reopened by an incision along the outer border of the right rectus muscle, and the invaginated bowel drawn forward into the wound. No peritonitis. The bowel at point of operation was very vascular, and the neck of the intussuscipiens covered with plastic exudation. The sutures were removed and the rectum and colon distended with water for the purpose of effecting reduction. As soon as the colon had become thoroughly distended the adhesions gave way with an audible noise, and complete reduction followed in such a manner that the portion last invaginated was first reduced. After reduction had been accomplished the injection was continued, to test the competency of the ileo-cæcal valve. As soon as the cæcum was well distended the fluid passed readily through the valve into small intestines, showing that the valve had been rendered incompetent by the invagination. The force required to overcome the adhesions in the reduction of the invagination was sufficient to rupture the peritoneal covering of the large intestines in three different places, the rents always taking place parallel to the bowel. The animal died on the following day with symptoms of diffuse peritonitis.

EXPERIMENT XVI.—Ascending invagination in a cat. A few inches above the ileo-cæcal region the ileum was invaginated in an upward direction to the extent of two inches. At the time the invagination was made the intussuscipiens contracted firmly, in consequence of this a tear occurred in its peritoneal covering in a direction parallel to the bowel. The stools were few and scanty. On the fourth day the animal died of perforative peritonitis. Abdominal wound not united, but the peritoneal wound closed by omental adhesion. Spontaneous reduction of half an inch of the invagination had taken place. Reduction by traction was found impossible on account of firm adhesions about the neck of the invagination. Recent diffuse peritonitis caused by two perforations, one at the neck of the intussusceptum on mesenteric side, and the other a little to one side of this one, and on proximal side of bowel. The perforation resulted from beginning of sloughing of the invaginated portion of the bowel. About two inches above the invagination the bowel was acutely flexed toward the mesenteric side by recent firm adhesions. Flexion was undoubtedly caused by circumscribed plastic peritonitis and increased peristalsis.

EXPERIMENT XVII.—Large, adult cat. Descending invagination of ileum to the extent of two inches in the upper portion of this part of the bowel. Second and third days the scanty discharges from the bowel bloody. Temperature from the second day after operation varied between 103.4° F. to 105.4° F. Death from perforative peritonitis on the seventh day after invagination. Abdominal wound united. Recent diffuse peritonitis from a perforation at the neck of the invagination on mesenteric side. Gangrene of intussusceptum and partial separation which has again caused a sharp flexion

of the bowel at the neck of the invagination. Above the seat of obstruction the bowel dilated and distended with fluid contents, below empty and contracted.

EXPERIMENT XVIII.—Young cat. Invagination of ileum into ascending colon to the extent of three inches. For a week after operation frequent tenesmus, followed by mucous discharges mixed with blood. The temperature during this time varied from 102.6° to 105° F. After this the animal improved and was in good condition when killed, fourteen days after operation. Abdominal wound united. No omental adhesions or peritonitis. Firm union between the serous surfaces. No dilatation of bowel above seat of obstruction. Intussusceptum not gangrenous, its lumen about the size of an ordinary lead pencil. It was found impossible to reduce the invagination by traction or by forcible injection of fluid from below. When the traction was increased, the peritoneal surface of the neck of the intussuscipiens ruptured in a longitudinal direction.

EXPERIMENT XIX.—Large, adult cat. Six inches of the ileum were invaginated into the colon. Frequent bloody discharges until the third day, when the abdomen was reopened and the neck of the intussuscipiens exposed to sight, so as to observe directly the mechanism of disinvagination by rectal injection of water. As soon as the colon was well distended the adhesions at the neck of the intussuscipiens began to give way, and complete reduction followed, as the adhesions gave way under the pressure from below. The abdominal wound was again closed and dressed in the usual manner. The animal recovered completely from this operation, and was killed twenty-four days after the first operation. Abdominal wound well united. In the ileo-cæcal region numerous adhesions around the portion of bowel which had been invaginated and subsequently reduced.

EXPERIMENT XX.—Invagination of colon into colon was commenced about the middle of the bowel and advanced as far as the cæcum. Second day bloody discharges from the bowels. Animal killed five days after operation. External wound united only on peritoneal side. Invagination completely reduced. Localized plastic peritonitis limited to the portion of the bowel which had been invaginated, otherwise peritoneum and intestines in a healthy condition.

EXPERIMENT XXI.—Invagination of colon into colon to the extent of four inches in a cat. The subsequent symptoms only for a short time indicated the existence of invagination, which, after they had subsided, were followed by evidences of peritonitis. Death occurred on the nineteenth day after the invagination. Abdominal wound well united; diffuse purulent peritonitis; under surface of diaphragm covered with plastic lymph. Although sought for, no perforation could be found in the disinvaginated bowel, but, as the peritonitis appeared to have started at the site of operation, it is probable that infection took place through the paretic walls of the disinvaginated bowel.

EXPERIMENT XXII.—Same kind of invagination made in a cat, as in the preceding case. For two days the stools were frequent, scanty, and contained mucus and blood. After this the animal remained in good condition until it was killed, thirty-five days after the invagination. Abdominal cavity showed no trace of inflammation. The invagination was completely reduced and the entire colon presented a normal appearance.

Remarks.—With the exception of Experiment XVI, the invagination was always made in a downward direction. In the case of ascending invagination, gangrene of the intussusceptum and perforation resulted in death from diffuse peritonitis on the fourth day after partial spontaneous reduction had taken place. In Experiments XV and XIX, both cases of ileo-cæcal invagination, complete reduction was effected by distention of the colon with water; in the first case the force required to accomplish this result was sufficient to produce multiple longitudinal lacerations of the peritoneal surface of the distended bowel, which undoubtedly were responsible for the death on the following

day, from diffuse peritonitis; while, in the second case, no such accident occurred, and the animal recovered, although the abdominal wound was reopened for the purpose of observing the mechanism of reduction by this method of procedure. In one case of ileo-cæcal invagination, Experiment XVIII, the intussusceptum remained *in situ* after the invagination, and became so firmly adherent with the intussuscipiens that even in the specimen reduction by traction was found impossible. In this case, although the lumen of the invaginated portion barely permitted the introduction of an ordinary lead pencil, no symptoms of obstruction were manifested during life, and the bowel above the invagination was not found dilated after death. In Experiment XIV the sloughing of the intussusceptum led to cicatricial contraction of the bowel and flexion at site of invagination, conditions which resulted in death from obstruction twenty-three days after invagination. The great danger which attends sloughing of the invaginated portion is circumscribed gangrene and perforation of the intussuscipiens at its neck, and death from perforative peritonitis, as illustrated by Experiments XVI and XVII. Experiment XVI illustrates that ascending invagination, should it occur, is not more likely to be reduced spontaneously than the more common form of descending invagination. These experiments also demonstrate conclusively that the danger attending the invagination increases the higher it is located in the intestinal canal, being greatest when it is situated high up in the tract of the small intestines, and gradually less as the ileo-cæcal region is approached. The ileo-cæcal form is less dangerous, as spontaneous reduction is more likely to take place, and gangrene of the intussusceptum, when it occurs, does so after a longer time after firm adhesions about the neck of the intussuscipiens have formed, a condition which is well adapted to prevent perforation. Of the three invaginations of the colon, Experiments XX, XXI and XXII, complete spontaneous reduction took place in all of them from the first to the fourth day, and in only one of them was the result fatal—in Experiment XXI—where purulent peritonitis, either from infection through the operation wound, or, what is more probable, through the damaged wall of the colon, occurred and was the cause of death on the nineteenth day after the invagination. Experiments XV and XIX prove both the danger and the utility of distention of the colon in cases of ileo-cæcal and colonic invaginations. As a rule, the longer the invagination has existed the firmer the adhesions, and consequently the greater the danger of relying too persistently on this measure in reducing the invagination. In resorting to this expedient in the reduction of an ileo-cæcal invagination it is of the greatest importance to relax the abdominal wall completely by placing the patient fully under the influence of an anæsthetic, and to aid the distending force as much as possible by gravitation the patient should be inverted and the injection should always be made very slowly and with requisite care to prevent rupture of the peritoneal coat by rapid over-distention. When the obstruction is located beyond the ileo-cæcal valve, no reliance can be placed upon the measure, as can be seen from the following experiments made to determine the

PERMEABILITY OF THE ILEO-CÆCAL VALVE.

EXPERIMENT XXIII.—While completely under the influence of ether, an incision was made through the linea alba of a cat sufficiently long to render the ileo-cæcal region readily accessible to sight. An incision was made into the ileum just above the valve, and by gently retracting the margins of the wound, the valve could be distinctly seen. Water was then injected per rectum, and as the cæcum became well distended it could be readily seen that the valve became tense and appeared like a circular curtain preventing effectually the escape of even a drop of fluid into the ileum. The competency of the valve was only overcome by *over-distention* of the cæcum which mechanically separated its margins, which allowed a fine stream of water to escape into the ileum.

The insufficiency of the valve was clearly caused by great distention of the cæcum. That such a degree of distention is attended by no inconsiderable degree of danger was proved by this experiment, as the cat was immediately killed, and on examination of the colon and rectum a number of longitudinal rents of the peritoneal coat were found.

EXPERIMENT XXIV.—In this experiment a cat was fully narcotized with ether and while the body was inverted water was injected, per rectum, in sufficient quantity and adequate force, by means of an elastic syringe, to ascertain the force required to overcome the resistance offered by the ileo-cæcal valve. Great distention of the cæcum could be clearly mapped out by percussion and palpation before any fluid passed into the ileum. As soon as the competency of the valve was overcome the water rushed through the small intestines, and after having traversed the entire alimentary canal, issued from the mouth. About a quart of water was forced through in this manner. The animal was killed and the gastro-intestinal canal carefully examined for injuries. Two longitudinal lacerations of the peritoneal surface of the rectum, over an inch in length, were found on opposite sides.

EXPERIMENT XXV.—This experiment was conducted in the same way as the foregoing, only that the cat was not etherized. More than a quart of water was forced through the entire alimentary canal from anus to mouth. The animal was not killed and lived for eight days, but suffered during the whole time with symptoms of ileo-colitis. A post-mortem examination was not made in this case, although the symptoms manifested during life leave no doubt that they resulted from injuries inflicted by the injection. It will thus be seen that in the three cases where fluid was forced beyond the ileo-cæcal valve, in two of them the post-mortem examination revealed multiple lacerations of the peritoneal coat of the large intestines, while the third animal sickened immediately after the experiment was made, and died from the effects of the injuries inflicted eight days later. The injection of water beyond the ileo-cæcal valve in the treatment of intestinal obstruction must therefore be looked upon in the light of a dangerous expedient and should never be resorted to.

II. ENTERECTOMY.

It still remains an open question to what extent resection of the small intestines can be performed with immunity. It is true that Kœberlé, Kocher and Baum have successfully removed respectively 2.05 m., 160 ctm., and 137 ctm. of the small intestine in the human subject, but while two of the patients do not appear to have suffered any ill effects in consequence of the removal of such a large surface for digestion and absorption, in Baum's case death, which supervened six months after the operation, was attributable clearly to marasmus, brought about by the extensive intestinal resection. As in a number of pathological conditions of the intestinal canal, as multiple strictures, gangrene, and multiple gunshot wounds, where the wounds are large and in close proximity, it may become necessary to resort to extensive resection, it becomes an important matter for the surgeon to know how much of the intestinal tract can be removed without any immediate or remote ill consequences. The immediate danger attending such an operation is the traumatism, which, of course, will be proportionate to the length of the piece of intestine removed, while the remote consequences are due to impairment of the function of digestion and absorption, caused by the shortening of the intestinal canal. With the view of obtaining additional light on these important questions, the following experiments were undertaken :—

EXPERIMENT XXVI.—Dog, weight 22 lbs. Mesentery divided into four portions and tied with catgut, and thirty inches of the ileum from near the ileo-cæcal region upward excised and ends sutured together by Czerny-Lembert's sutures. Abdominal

its lumen should correspond with that of the ileum. The ileum and rectum were then united by Czerny-Lembert sutures. The animal never rallied from the prolonged operation, and died from shock two hours later.

Remarks.—The results of these experiments speak for themselves. In all cases of extensive resections of the small intestines, where the resected portion exceeded one-half of the length of this portion of the intestinal tract, where the animals survived the operation, marasmus followed as a constant result, although the animals consumed large quantities of food. In all of these cases defective digestion and absorption could be directly attributed to a degree of shortening of the digestive canal incompatible with normal digestion and absorption. Only one of these animals (Experiment XXVII) died from shock a few hours after operation. Another death resulted from the trauma in Experiment XXIX, where fatal hemorrhage occurred from one of the mesenteric vessels where the catgut ligature became displaced from the shrinkage of the included mesenteric tissues. When the vessels of the omentum or mesentery are tied *en masse* there is always danger from this source, and to prevent this accident it becomes necessary not to include too much tissue and to tie firmly with fine threads of aseptic silk. After I commenced to tie in this manner I encountered no further difficulty in arresting and preventing hemorrhage in operations requiring incision of these tissues. Although the large artery running parallel with the bowel where the mesentery is attached was excised in every case with the intestine, gangrene and perforation occurred only in Experiment XXVI. The post-mortem appearances after extensive enterectomies indicated that the portion of bowel which remains undergoes compensatory hypertrophy, but as a rule the increased functional activity is not adequate to make up for the great anatomical loss. In all instances where the animal recovered from the operation the discharges from the bowels were frequent, fluid or semi-fluid, and contained undigested food, among other substances free undigested fat, showing that the intestinal secretions play an important rôle in the digestion of fat. As an approximate estimate the statement can be ventured that in dogs and cats the excision of more than one-third of the length of the small intestines is dangerous to life, as it is followed by marasmus, which sooner or later results in death. As all three cases of excision of the colon proved fatal from shock in from two to six hours, it can be safely asserted that this operation is impracticable, and is invariably followed by death from the immediate results of the trauma.

III. PHYSIOLOGICAL EXCLUSION.

As extensive resections of the intestines are always attended by great risks to life from the trauma, I concluded to study the subject of sudden deprivation of the system of a great surface for digestion and absorption by eliminating or diminishing the cause of death from this source by leaving the intestine, but excluding permanently a certain portion from participating in the function of digestion and absorption; in other words, by resorting to physiological exclusion. These experiments were also made to determine the tissue changes which would take place in the bowel thus excluded, and to learn if under such circumstances accumulation of intestinal contents would become a source of danger as had been feared by the older surgeons. The complete interruption of passage of intestinal contents, either by section and closure of the bowel, or by making an intestinal obstruction of some kind, and the restoration of the continuity of the physiologically active portion of the intestinal canal was established by suturing of the proximal end of the high section with the distal end of the lower section, or by implanting the proximal end into the bowel lower down, the intervening portion of the intestinal tract in either case, thus becoming the excluded portion.

EXPERIMENT XXXV.—Large cat, weight nine pounds. Double division of small intestines; upper section made about eight inches below the pylorus, and the lower

three feet lower down; the portion of bowel between these circular sections was closed at both ends, and the continuity of the intestinal canal restored by suturing the open ends in the usual manner. In this way three feet of the small intestines were isolated and completely excluded from the digestive canal. The intervening portion was emptied of its contents as completely as possible before its ends were closed by suturing. The animal died on the fourth day after the operation. A small perforation of the sutured bowel on mesenteric side was found, otherwise the visceral wound was found well united. Excluded portion slightly distended with gas, both ends firmly closed. The perforation had given rise to diffuse peritonitis which was the immediate cause of death.

EXPERIMENT XXXVI.—Dog, weight twelve pounds. The jejunum was divided four feet above the ileo-cæcal region and the distal end closed. Jejuno-colostomy was made by implanting the proximal end into a slit made into the convex side of the ascending colon large enough to correspond to the circumference of the jejunum. The implanted end was fixed in its position by two rows of sutures. The animal never appeared to rally from the effects of the operation and died at the end of the next day. The abdominal cavity was found filled with blood which must have escaped from a mesenteric vessel from which, probably, the catgut ligature had slipped. The excluded portion, that is, that portion intervening between the circular section and the point of implantation, was found quite empty of intestinal contents, but slightly distended with gas. Implanted end perfectly retained by sutures and slight adhesions between serous surfaces had already taken place. Death in this case was the result of secondary hemorrhage.

EXPERIMENT XXXVII.—Dog, weight thirty-five pounds. Divided the ileum just above the ileo-cæcal region and closed both ends of the bowel. Ileo-colostomy was done by making an incision about an inch and a half in length, on concave side of ileum forty-four inches above the division, and a similar slit on convex side of ascending colon, and uniting these wounds by Czerny-Lembert sutures, thus excluding from the intestinal circulation forty-four inches of the bowel. The day after operation the fæces contained blood. During the progress of the case it is frequently noted that the stools were thin, sometimes liquid. Appetite remained good, and animal was well nourished at the time of killing, twenty-five days after operation. Abdominal wound well united. The omentum and a few intestinal loops adherent to inner surface of wound. The excluded portion contracted to more than one-half of its usual size, atrophic, and not nearly as vascular as remaining portion of intestinal canal. The two blind ends adherent to each other and to adjacent loops. The excluded portion contained in its blind end a few sharp fragments of bone. The new opening between the ileum and colon about the capacity of the lumen of the ileum surrounded by a prominent margin of mucous membrane, which somewhat resembles the ileo-cæcal valve, to which still remain attached about ten of the deep sutures. The coats of both bowels at point of approximation thickened by inflammatory exudation.

EXPERIMENT XXXVIII.—Young cat. The ileum was divided about thirty inches above the ileo-cæcal region; the distal end closed and proximal end laterally implanted into the convex side of the transverse colon, where it was fixed by a double row of sutures. Before implantation the continuity of the peritoneal surface was procured by drawing the peritoneum with a fine catgut suture over the denuded space left after detachment of the mesentery. Although the animal partook freely of food progressive marasmus set in, to which the cat succumbed eleven days after operation. Abdominal wound completely healed. Union of implanted ileum with colon perfect. No peritonitis. Excluded portion empty. Bowel above implantation somewhat dilated.

EXPERIMENT XXXIX.—Young, but full-grown cat. Physiological exclusion of two-thirds of the small intestines, and the entire colon by division of the small intestine

at the junction of the upper with the middle third. Closure of distal end, and restoration of continuity of the shortened intestinal tract by making a jejuno-rectostomy. The implantation was made into the upper portion of the rectum at a point opposite the meso-rectum. Previous to section and suturing, the portion of bowel to be excluded was emptied of its contents. Animal died two days after operation. No peritonitis. Slight adhesions between the serous surfaces of rectum and implanted jejunum; excluded portion empty.

EXPERIMENT XL.—The entire ileum was excluded in a cat by dividing the intestine at its junction with the jejunum, closure of distal end and making a jejuno-colostomy by implantation of the proximal end into a slit of the transverse colon at a point opposite the meso-colon. The cat remained in good condition until killed, fifteen days after operation. No vomiting, and movements from bowels normal. Abdominal wound completely closed; no peritonitis; jejunum at point of implantation firmly united. New opening in colon the size of the lumen of the ileum. Excluded portion empty, contracted and anæmic.

EXPERIMENT XLI.—Large mastiff. The small intestine was divided six and a half feet above the ileo-cæcal region, the distal end closed, and the proximal end implanted into an incision of the transverse colon large enough to receive it at a point opposite the meso-colon. Suturing was done exclusively with fine silk. For three weeks the dog appeared quite well, ate well, and the discharges from the bowels were normal. From this time the emaciation, which commenced soon after the operation was done, began to increase rapidly, the animal began to refuse food, and died of marasmus thirty-two days after operation. No peritonitis. Excluded portion empty and reduced one-half in size; the coats of the bowel very much attenuated, and the vessels hardly half the normal size. Only three feet and five inches of the small intestine remained for physiological action. New opening in colon sufficiently large to permit the introduction of the index finger as far as the first joint. On splitting open the colon the point of juncture with the jejunum upon the inner surface is marked by a slight ridge of mucous membrane, which bears a faint resemblance to the ileo-cæcal valve.

Remarks.—For some reason which I am unable to explain satisfactorily, in animals where the same length of intestine was physiologically excluded, as in the resection experiments, the appetite never became so voracious and the remaining portion of intestine did not undergo the same degree of compensatory hypertrophy as in the excision experiments. Theoretically, two explanations might be advanced: firstly, in shortening the intestinal canal by resection an extensive vascular district is cut off by ligation of the mesentery, and it is only reasonable to assume that the circulation in the remaining branches of the mesenteric artery would be increased, and consequently the functional activity of the organs supplied by them augmented; secondly, in cases of physiological exclusion of lateral opposition it is possible that at least some of the fluid contents reached the excluded portion from which a certain amount might still have become absorbed. The exclusion was complete or nearly so, so we must conclude from the post-mortem appearances, that in nearly every instance the excluded portion presented an atrophic, contracted condition, and was only sparingly supplied with blood vessels. From a practical standpoint these experiments teach us that a limited portion of the intestinal canal can be permanently excluded from the processes of digestion and absorption in proper cases by operative measures without incurring any risk of fecal accumulation in the excluded part. These experiments demonstrate also that physiological exclusion of a certain portion of the intestinal tract is a less dangerous operation than excision, and that in certain cases of intestinal obstruction, where excision has been heretofore practiced, it can be resorted to as a substitute for this operation in cases where excision is impracticable, or where the pathological conditions which have caused the obstruction do not in themselves constitute an intrinsic source

of immediate or remote danger to life. The post-mortem appearances of the specimens of these experiments tend to prove that as long as any of the contents of the intestines reach the excluded portion the peristaltic or anti-peristaltic action in that part is effective in forcing it back into the active current of the intestinal circulation.

IV. CIRCULAR ENTERORRHAPHY.

During my experimental work I became convinced that circular enterorrhaphy as it is now commonly performed is attended by three great sources of danger: (1) Perforation at the junction not covered with peritoneum; (2) the length of time required in performing the operation; (3) too many sutures.

To obviate the danger of perforation at the junction of the bowel not covered by serous membrane, I resorted to peritoneal suturing before uniting the bowel, by drawing the peritoneum over the denuded space caused by the limited detachment of the mesentery, by a fine catgut suture applied near the free margin of the bowel, as described before. This requires but little time and secures for the whole circumference of the bowel a peritoneal covering, so that after the bowel has been sutured, the great rule inaugurated by Lembert, "serosa against serosa," has been carried out to perfection. The results showed that this little modification of the ordinary method of suturing yielded more satisfactory results, and should, therefore, be adopted in all cases where circular enterorrhaphy is done with Czerny-Lembert or Lembert's sutures. Time plays an important factor in determining the results of all operations requiring abdominal section, and this is especially true in all operations for intestinal obstruction, as this class of patients is usually greatly exhausted before consent for an operation can be obtained. With a patient exhausted from an acute attack of obstruction of the bowels, it becomes exceedingly important to consume as little time as possible in the operation, as the shock incident to a long operation may in itself determine a fatal result. Even after I had acquired a fair degree of manual dexterity in suturing the bowel, I seldom spent less than an hour in making a circular enterorrhaphy by a double row of sutures.

In opening the abdomen for intestinal obstruction, usually a considerable length of time is spent in finding the obstruction, and when this is found and the patient manifests symptoms of a collapse, a radical operation, which for its performance requires an hour or more, is often abandoned and the operation is finished by making an artificial anus, which, at the present time, must be looked upon as a reproach upon good surgery. The last objection to the Czerny-Lembert method of suturing requires no argument. Any surgeon who hastily transfixes the bowel with a needle from 30 to 40 times in applying the Lembert suture is liable to perforate the whole thickness of its walls once or more, and if silk is used as suturing material the puncture may become the seat of a perforation, and the direct cause of a fatal peritonitis. This is more particularly the case in operating on the bowel in cases of intestinal obstruction, as under such circumstances the walls of the bowel have become greatly attenuated from over-distention, and consequently more liable to become perforated by the needle. But the use of so many sutures, from thirty to forty, as recommended, brings with it another source of danger—gangrene of the inverted margin of the bowel. The second row of sutures applied in such close proximity must materially affect the blood supply to the inverted margin of the bowel, which in some instances must terminate in gangrene. Such a result is the more likely to ensue as the inner surface of the bowel is exposed to all dangers incident to injection from the intestinal canal, in other words, an aseptic condition, for one side of the wound cannot be secured, consequently the gangrene is of a septic character, which is prone to extend beyond the primary cause which produced it.

To obviate some of these dangers I experimented with a modification of Jobert's invagination suture. According to Madelung, the ingenious method of circular sutur-

ing, devised by Jobert, was practiced only in four cases, and two of the patients are known to have recovered. A number of years ago I was forced to resort to resection of a part of the small intestine in a very complicated case of ovariotomy, and resorted to this method, and although the patient died forty-eight hours after the operation, from causes outside of this complication, the bowel was found permeable and quite firmly united, and had the patient lived, I have no doubt the result of the resection and suturing would have been satisfactory. In Jobert's method the invagination sutures must be looked upon as a source of danger, as they were made to traverse the entire thickness of the wall of the bowel, and the material used was silk. It has been claimed that in this method the invaginated portion of the bowel becomes gangrenous, as in cases of invagination from pathological causes. This claim has arisen from a theoretical and not from an experimental standpoint. In cases of invagination the intussusceptum carries with it the mesenteric vessels intact in the form of an arch which, by constriction at the neck of the intussuscipiens, is prone to become strangulated, an event which is followed by œdema and inflammatory swelling of the invaginated portion, which rapidly leads to complete venous stasis and gangrene. In circular suturing by Jobert's method the intussusceptum has no vascular connections with the intussuscipiens. The vascular arch is interrupted and, consequently, the danger arising from venous obstruction is almost completely obviated. My experiments will show that gangrene of the invaginated portion, as a rule, does not occur.

My modification of Jobert's method consists essentially in the use of a thin elastic rubber ring for lining the intussusceptum, to prevent ectropium of the mucous membrane, to protect the mucous membrane of the bowel against injurious pressure from the suture, to keep the lumen of the bowel patent during the inflammatory stage, and to assist in maintaining coaptation of the serous surfaces, and, finally, the substitution of catgut for silk as invagination sutures. My method of proceeding is as follows: The upper end of the bowel, which is to become the intussusceptum, is lined with a soft, pliable rubber ring made of a rubber band, transformed into a ring by fastening the ends together with two catgut sutures. This ring must be the length of the intussusceptum, from one-third to half of an inch; its lower margin is stitched by a continuous catgut suture to the lower end of the bowel, which effectually prevents the bulging of the mucous membrane, a condition which is always difficult to overcome in circular suturing. After the ring is fastened in its place, the end of the bowel presents a tapering appearance which materially facilitates the process of invagination. Two well-prepared, fine, juniper catgut sutures are threaded, each with two needles. The needles are passed from within outward, transfixing the upper portion of the rubber ring and the entire thickness of the wall of the bowel, and always equidistant from each other; the first suture being passed in such a manner that each needle is brought out a short distance from the mesenteric attachment, and the second suture on the opposite convex side of the bowel. During this time an assistant keeps the opposite end of the bowel compressed, to prevent contraction and bulging of the mucous membrane. The needles next are passed through the peritoneal, muscular and connective tissue coats at corresponding points about one-third of an inch from the margins of the opposite end of the bowel, and when all the needles have been passed, an assistant makes equal traction on the four strings, and the operator assists the invagination by turning in the margins of the lower end evenly with a director, and by gently pushing the rubber ring completely into the intussuscipiens. The invagination accurately made, the two catgut sutures are tied only with sufficient firmness to prevent disinvagination should violent peristalsis follow the operation. This is their only function. The invagination itself effects accurate, almost hermetical, sealing of the visceral wound. The intestinal contents pass freely through the lumen of the rubber ring from above downward, and escape from below is impossible as the free end of the intussuscipiens secures accurate

valvular closure. After a few days the rubber ring becomes detached, and by giving way of the catgut sutures is again transformed into a flat band which readily passes off with the discharges through the bowels. The invagination sutures of catgut are gradually removed by substitution on part of the tissues, hence the punctures in the bowel remain closed either by the catgut or by the products of local tissue proliferation; and thus extravasation is prevented. In my first experiments I used three invagination sutures, but found by experience that two are just as efficient in making and retaining the invagination. No superficial or peritoneal sutures were used in any of the cases, sole reliance being placed upon the invagination to maintain approximation and coaptation. The mesenteric attachment, both of the intussusceptum and intussuscipiens, was separated only a few lines, to enable invagination without too much narrowing of the lumen of the intussuscipiens.

EXPERIMENT XLII.—Dog, weight fifteen pounds. Three invagination sutures were used. The ileum was cut completely across at a point about three feet above the ileo-cæcal region. Depth of invagination one inch. For two days after operation a slight rise in temperature; no symptoms of obstruction during the whole time. Animal in good condition when killed, two weeks after operation. Omentum adherent at point of operation as well as an adjacent loop of intestine. Union between intussusceptum and intussuscipiens firm, no signs of gangrene. Narrowest portion of lumen of bowel was large enough to pass the little finger to second joint. An enterolith composed of fragments of wood, bone, etc., in the centre of which the straight rubber band which had been the rubber ring was found, just above the seat of operation. No distention of the bowel above this point. Bowel considerably flexed at seat of invagination; this condition being evidently brought about by inflammatory adhesions.

EXPERIMENT XLIII.—Dog, weight twenty pounds. Section of bowel and invagination with rubber ring the same as in the foregoing experiment. In subsequent history no mention is made of any symptoms of obstruction, but for the last few weeks it was noticed that the dog began to emaciate. He died suddenly eighty-one days after the operation. Diarrhœa was a prominent symptom toward the last. No adhesions and no peritonitis. An enormous enterolith, composed of all kinds of crude material, and again holding in its centre the rubber band, was found just above the invagination. Bowel at this place considerably dilated. Intussusceptum firmly adherent, a false passage, admitting the tip of the little finger, had been made on one side between it and the intussuscipiens. Death in this case was evidently produced by the enterolith. In this, as in the last case, the invagination was made at least an inch in length, and the collection of the crude, indigestible material which the dog must have eaten in large quantities, around the detached rubber ring, gave rise to the formation of the enterolith. The wall of the bowel surrounding the foreign body was not only dilated but greatly thickened. It is a well-known fact that even a moderate degree of stenosis of the bowel in dogs is liable to give rise to the formation of an enterolith, as the crude material which these animals swallow becomes arrested, and by constant accretions of the same kind of material the enterolith forms and continues to increase in size until its presence causes catarrhal inflammation, and, finally, intestinal obstruction.

It is quite possible that the lower end of the intussusceptum in the last case became impermeable during the inflammatory stage, and that the false passage was formed on this account by perforation on one side of the intussusceptum, an accident which was plainly traceable to too deep invagination.

EXPERIMENT XLIV.—Dog, weight forty pounds. This experiment is interesting only from the fact that it shows that it is possible to make a mistake in the direction of the invagination, even after the operator has determined with accuracy which is the ascending and descending end of the gut, and to show the disastrous consequences which must necessarily follow such a technical mistake. The invagination was made in the

usual manner, with rubber rings and three catgut sutures. The animal appeared to be quite ill the day following the operation, and on the next day the thermometer showed a rise in temperature to 104.2° F. On the third day the dog died with well-marked symptoms of perforative peritonitis. Recent peritonitis with some agglutination of intestines. Considerable quantity of sero-sanguinolent fluid in the peritoneal cavity. To my utter astonishment I found that an ascending invagination had been made. Circular gangrene of intussusceptum and complete separation of ends were found. The rubber ring remained *in situ*, still attached to the intussuscipiens by the catgut sutures, which had become somewhat softened. The invagination had decreased considerably by the traction caused by the peristalsis and by the pressure of the intestinal contents from above the obstruction, and the extensive gangrene of the bowel was undoubtedly determined to a great extent by these means.

EXPERIMENT XLV.—As an illustration of another source of danger due to faulty technic, I will relate this experiment. Medium-sized dog. Circular enterorrhaphy was done with the rubber ring two feet above the ileo-cæcal valve. In making the invagination, it was noticed that the ring was too large, as it was seen that it caused too much pressure. Thinking that the parts might adapt themselves to this pressure, the bowel was replaced and the abdominal wound closed. The dog died thirty-six hours after operation. Abdominal wound not united; omentum and intestines adherent to each other and at point of operation. The circumscribed gangrene of the intussuscipiens was evidently entirely due to pressure on part of the rubber ring. The intussuscipiens was much swollen, a condition which materially aggravated the pressure caused by the rubber ring. With the following experiment two new departures were inaugurated, viz.: Instead of three invagination sutures, only two were used, a change which still further shortened the time for performing the operation, and the use of Nothnagel's test in determining the direction in which the invagination should be done. In all of the remaining experiments of circular enterorrhaphy which were made only two catgut sutures were used, which answered the purpose fully as well as when more were used. Until now it was always necessary to find one of the extremities of the small intestines for the purpose of determining which was the afferent and which was the efferent end of the tube, so as to make the invagination in the right direction, a procedure which often required considerable time, and brought additional risk by increasing the shock of the operation and the danger of traumatic infection.

NOTHNAGEL'S TEST.

In experimenting upon animals for the purpose of studying the functions of the intestinal canal in health and disease, Nothnagel made the discovery that when the salts of potash are brought in contact with the serous surface of the bowel, circular constriction takes place, and when the peritoneal surface is touched with a crystal of common salt, ascending peristalsis was produced. The sodic chloride test I applied in 16 cases, and found Nothnagel's observations corroborated in 15 by subsequent anatomical examination. In the remaining case, where a wrong conclusion was drawn, the error might have been due to a faulty observation, or the observation was not continued for a sufficient length of time. If, in the human subject, these observations could be verified, it would be of great practical importance to surgeons in operations on the intestinal canal, whenever it becomes necessary to determine which is the ascending or descending part of the bowel.

EXPERIMENT XLVI.—Dog, weight thirty pounds. Circular section of ileum and immediate enterorrhaphy by invagination with rubber ring and two catgut sutures. Intussusceptum invaginated not more than a quarter of an inch. A few days after the operation, stools mixed with blood; no other unfavorable symptoms. Animal killed,

fourteen days after operation. Wound united firmly. A number of omental and intestinal adhesions. A small abscess in mesentery at point of operation. No obstruction of any kind. On opening the bowel the walls at site of operation were very thick, corresponding to the three intestinal coats, which had become considerably attenuated. The inner surface shows the point of junction of the intussusceptum with the intussuscipiens in the shape of a circular ring of mucous membrane. The most contracted portion is large enough to admit the little finger.

EXPERIMENT XLVII.—Dog, weight fifteen pounds. Section of ileum and circular enterorrhaphy with rubber ring and two catgut sutures. Depth of invagination, one-third of an inch. No unfavorable symptoms after operation. Animal killed after seven days. Wound completely united. Firm union of visceral wound; no gangrene of intussusceptum. Rubber ring retained *in situ* by catgut sutures, which are easily torn. Upper end of rubber ring matted with hair. No obstruction. Lumen of bowel somewhat contracted by a circular ridge of mucous membrane, which indicates the junction of the two invaginated ends of the bowel.

TRANSPLANTATION OF OMENTAL FLAP.

In most all post-mortem examinations of specimens from operations on the intestines, I observed that the omentum was adherent over a greater or less surface at the seat of suturing. I also observed that perforations never occurred wherever this additional protection to the peritoneal cavity had formed. To anticipate nature in protecting the peritoneal cavity in this manner I commenced to transplant an omental flap about an inch in width and sufficiently long to reach around the bowel over the neck of the intussuscipiens, where it was fastened on the mesenteric side by two catgut sutures. The flap was taken either from the margin of the omentum or from its middle, care being taken to take some portions supplied with a vessel of considerable size. Its base was left attached to the omentum; all bleeding points were carefully tied with catgut ligatures. The two catgut stitches used for its fixation were passed twice through the flap, its base and free end, and the mesentery in such a way that, when tied, the direction of the suture corresponded to the course of the mesenteric vessel, so that after tying they would not interfere with the vascular supply of the bowel. When the flap was taken from the middle of the omentum the lateral halves were united with one or two catgut sutures before closing the abdominal wound.

EXPERIMENT XLVII.—Dog, weight forty pounds. Ileum divided eighteen inches above ileo-cæcal region, and the ends united by invagination with rubber ring and two catgut sutures. Transplantation of omental flap, one inch in width around the whole circumference of the bowel over neck of intussuscipiens, fixation with two catgut sutures on mesenteric side. Invagination one-third of an inch in depth. Animal killed two weeks after operation. Abdominal wound perfectly healed. Omental flap firmly adherent to bowel over neck of intussuscipiens. Bowel at seat of operation much thickened; rubber ring gone; lumen of bowel at its most contracted point large enough for the passage of the little finger.

EXPERIMENT XLVIII.—Dog, weight twenty pounds. Complete division of ileum and immediate union of divided ends by invagination with rubber ring and two catgut sutures. Transplantation of omental flap, two inches in width, over the neck of the intussuscipiens. On third day stools mixed with blood. Died on the fifth day. Wound not united; omental flap firmly adherent, except at a small point on mesenteric side, where a minute perforation had taken place from circumscribed gangrene of the intussusceptum. Rubber ring only loosely held by one of the sutures. Lumen in invaginated portion quite narrow, but permeable.

EXPERIMENT XLIX.—Dog, weight fifteen pounds. Complete section of ileum and

union of divided ends by invagination. The rubber ring used was only one-third of an inch wide, while formerly none were used less than half an inch in width. Neck of intussuscipiens protected by an omental flap two inches wide. The dog remained perfectly well and was killed twenty-five days after operation. Abdominal wound completely healed, covered on the inner side by adherent omentum. Rubber ring gone. Lumen of bowel at most contracted point readily admits the little finger. No signs of obstruction. Omental flap adherent throughout.

EXPERIMENT L.—Dog, weight twenty-two pounds. Division of ileum and suturing in usual manner by invagination with rubber ring and two catgut sutures; transplantation of omental flap. The dog remained perfectly well and was killed, twenty-three days after operation. A number of intestinal adhesions produced several flexions. Point of operation four feet above the ileo-cæcal region. Omental flap firmly adherent to bowel throughout. Rubber ring gone. Lumen of bowel in invaginated portion quite large. The invaginated portion atrophic and retracted, so that it appears in the shape of a firm ring, and indicated in the interior by a circular prominence of the mucous membrane. No evidences of obstruction.

EXPERIMENT LI.—Dog, weight fifteen pounds. Complete division of the ileum and reunion of ends by invagination. Transplantation of omental flap, two inches in width, over neck of intussuscipiens, two catgut fixation sutures. Second day after operation stools bloody. After this time all functions normal. Animal killed, forty-four days after operation. Point of operation four feet below the pylorus. The invaginated portion atrophied and retracted to such an extent that the bowel at this point only presents a thickened ring with its lumen only slightly narrowed by a circular ridge of mucous membrane. Omental flap firmly adherent all around and greatly atrophied.

Remarks.—In circular enterorrhaphy, as in cases of intestinal wounds of any kind, the ideal of any operation should be to bring in continuous uninterrupted apposition a large surface of serous membrane, without, at the same time, interfering with the vascular supply of the parts which it is intended to bring together for permanent union by cicatrization. If in employing the Czerny-Lembert sutures more than a few lines of the margins of the bowel is inverted and included between the two rows of sutures, there is great danger of causing primary traumatic stenosis by the projecting circular ring in the lumen of the bowel. The narrowing of the lumen of the bowel must be as great, if not greater, than after invagination. That the second row of sutures has often been the cause of gangrene of the inverted margins of the bowel would not be difficult to prove by many post-mortem records and specimens. By invaginating to the depth of a quarter or third of an inch, accurate coaptation is secured of the corresponding serous surfaces between the intussusceptum and intussuscipiens which is made more secure and effective by the elastic pressure exerted by the rubber ring. This method of coaptation furnishes large peritoneal surfaces for immediate union by cicatrization.

With perhaps one exception all of my experiments have shown that when catgut is used for invagination sutures none of the failures were attributable to their presence. On the inner side of the bowel the rubber ring is drawn against the puncture, and would thus furnish a mechanical protection against the escape of fluids along these minute canals; besides, the swelling of the catgut where it becomes softened by the fluids of the tissues, would most effectually plug the punctures until a permanent plug is furnished by the granulations which in time completely remove the catgut by substitution and close the punctures permanently by a minute cicatrix. One great advantage of the rubber ring consists in its furnishing absolute protection to the bowel against pressure by the invagination sutures during the invagination, and subsequent traction from peristaltic contraction, should the latter cause tension of the sutures, an occurrence which is not likely to arise if the invagination has been properly done. A circular enterorrhaphy as described above can be done in fifteen minutes, which

certainly compares very favorably with any other procedure as far as time is concerned. In the description of a number of the specimens, it has been distinctly stated that injurious results followed the stenosis caused by the invagination, and this might be urged as an argument against the safety and applicability of the operation.

As compared with the human subject, the dog is an unfavorable animal for circular enterorrhaphy by invagination. In the first place, the walls of the bowel are much thicker in proportion to its lumen than in man, a condition which necessarily seriously affects the lumen of the intussusceptum. Again, the dogs were allowed to eat what they desired before and after the operation and the quantity was not limited, consequently a great deal of indigestible substances, often of the coarsest kind, as straw, fragments of wood or bone, hair, etc., found their way into the intestinal canal, and in a number of cases were arrested at the point of narrowing in the bowel, where they gave rise to the formation of an enterolith. In one instance death resulted clearly from intestinal obstruction from such a cause. In men, the coats of the bowel being thinner and the lumen correspondingly larger, invagination is done with greater ease, and the danger from stenosis could hardly come into question, as the fluid contents of the small intestines would pass readily through the rubber tube. Some of the older specimens prove that the traumatic stenosis caused by the invagination gradually diminishes by atrophy of the invaginated portions, which finally only appear as a prominent ridge of mucous membrane on the inner surface of the bowel, the remaining coats having completely or nearly disappeared by retrograde metamorphosis and absorption. In the healing of all wounds, one important condition for an ideal result is rest. The rubber ring in the intussusceptum secures this important condition for the invaginated portion, as the elastic pressure must overcome peristaltic action, and secure for this segment of the bowel as near as possible absolute physiological rest. The danger of stenosis after invagination is greatest as soon as inflammatory swelling makes its appearance, a day or two after the operation, and the rubber ring is again in the right place to prevent any undue swelling by affording a gentle support for the invaginated portion which cannot fail in preventing undue venous engorgement and œdema, which would otherwise follow the invagination. It serves both the purpose of a splint and an elastic bandage. After union of a bowel by invagination with a rubber ring peritoneal sutures are superfluous, as the invagination itself must effectually prevent any escape of intestinal contents by the valvular action of the invaginated portion; at the same time the serous surfaces are kept in permanent and uninterrupted contact by the elastic pressure on part of the rubber ring. Although the experiments have demonstrated the safety of the catgut invagination sutures in operating upon dogs, the same innocuity might not attend operations after intestinal resections for obstruction, as in such cases the coats of the bowels are, almost without exception, very much attenuated, and consequently the danger of extravasation along the needle punctures would be increased.

Very recent trials have satisfied me that invagination after circular restriction can be done with the rubber ring with facility and probably greater safety by dispensing with the invaginating sutures and adopting the following plan: The lower end of the intussusceptum is lined with a soft rubber ring about one-quarter to one-third of an inch in width, and its lumen of sufficient size to afford free transit to the intestinal contents. The lower margin of the ring is stitched to the end of the intussusceptum by a continued fine catgut suture. The ends of the bowel are now brought in contact and fastened together with four catgut sutures which are placed equidistant from each other. Invagination is now made by gently pushing the ends of the bowel in opposite directions, being careful to push the ring sufficiently deep so that its upper margin is grasped by the neck of the intussuscipiens. A few superficial sutures are applied, simply for the purpose of preventing disinvagination; the four catgut sutures act as

invagination sutures, and at the same time prevent ectropium of the mucous membrane of the lower end of the bowel during and after invagination. With proper facilities and good assistance, a circular enterorrhaphy can be made in this manner, without using invagination sutures, in ten minutes, and by using not more than four retention sutures the blood supply to the inverted portions is not impaired, and at the same time the two ends of the bowel have been joined together by a large surface of peritoneum, which is held in accurate contact for rapid union by granulation and cicatrization. The advantages that are derived from covering a sutured intestinal wound by an omental flap are self-evident. The procedure is simply an imitation of nature's process in protecting the peritoneal cavity against perforation and in hastening the healing of the visceral wound. An adherent omentum secures rest for the part to which it has become attached. As the omental flap becomes firmly adherent before definitive healing of the visceral wound has taken place it furnishes additional protection, and in the event of a small perforation it guards against perforative peritonitis by mechanically preventing the entrance of pus into the peritoneal cavity. Should pus reach the omental flap after it has become firmly adherent, it is not very probable that perforation would take place through the two layers of peritoneum furnished by the adherent omental flap, and the subsequent healing of the perforation of the bowel would be most likely to take place. I shall again refer to this subject under the head of "Omental Grafting."

V. INTESTINAL ANASTOMOSIS.

By an intestinal anastomosis we understand a condition of the intestinal canal, where, on account of an obstruction or complete occlusion, the intestinal contents are directed into a segment of the bowel below the seat of obstruction or occlusion, through a fistulous opening between the bowel above and below the seat of partial or complete occlusion. The idea of establishing such a communication between the bowel above and below the seat of obstruction originated with Maisonneuve, who, without testing the new procedure first on animals, operated on two cases, but as the result in each case was fatal he seems to have become discouraged and abandoned the operation, and never published the communication on this subject which he had in preparation. In the Surgical Society of Paris, his proposition met with violent opposition from his contemporaries, who argued that the excluded portion of the intestine would become the seat of fecal accumulation, which, even if the operation were a success, would subsequently destroy the life of the patient. The subject was revived in 1863 by Hacken, who under the directions of Adelmann made some experiments on dogs. For a long time the operation was again completely forgotten until E. Hahn, of Berlin, very recently alluded to it again in commenting on his two cases of excision of the colon, where circular enterorrhaphy could not be performed, and where an artificial anus was established. Both patients recovered from the operation, but all attempts to close the preternatural opening proved futile.

The results of my experiments have shown conclusively that the fear of accumulation of fæces in the excluded portion of the intestine, that is, the intervening portion containing the seat of obstruction and extending on each side so far as the new opening by which the anastomosis has been established, is unfounded. If this objection can be laid aside, it becomes evident that the operation of establishing intestinal anastomosis has a great future, and will soon become an established procedure in the treatment of intestinal obstruction and as a substitute for circular suturing in some forms of injuries of the intestines which require excision. In my first experiments of establishing intestinal anastomosis, I made the operation by making an incision an inch and a half to two inches in length through the convex surface of each bowel, and sutured the wounds together by Czerny-Lembert sutures, the same as in making a circular enterorrhaphy.

The results soon showed that the operation was attended by the same dangers as suturing after circular resection, that is, gangrene of the margins of the bowel and perforation. Dr. M. E. Connel, Superintendent of the Milwaukee County Hospital, suggested the use of perforated plates for making the lateral apposition, in place of suturing. A few crude experiments were made with perforated discs of lead, wood, gutta percha and leather, and the results soon satisfied us of the expediency and greater safety of uniting the intestines in this manner. Although the first experiments were very imperfect and faulty in technique, almost every animal recovered. In the first experiments no needles were used. Around the oval perforation four catgut or silk sutures were tied; a slit was made in the bowel on the convex side, parallel with its axis, and large enough to permit the passage of a plate about an inch in width and two and one-half inches in length. After making the incision and introducing the plate above and below the seat of obstruction, the two wounds were brought into apposition, and the corresponding strings tied together with sufficient firmness to bring the flattened surfaces into accurate coaptation. The threads were cut short and the ends pushed inward out of sight. Experience showed that although the apposition was good a tendency was observed on the part of the margins of the wound to evert, on account of the bulging of the mucous membrane. I consequently modified the operation by arming the lateral threads with a needle with which the margin of the incision about the middle of the wound was transfixed. This proved a step in the right direction, as the lateral sutures completely prevented eversion of the margins of the wound, at the same time they fixed the plates in their position, and, lastly, at once transformed the longitudinal slit into an oval foramen of sufficient size for the free passage of intestinal contents. After many trials of different kinds of materials for the plates, I came to the conclusion that decalcified or partially decalcified bone plates, preserved after the decalcification in pure alcohol, served the best purposes.

DIRECTIONS FOR PREPARING BONE PLATES.

The compact layer of an ox's femur or tibia is cut with a fine saw into oval plates one-fourth of an inch in thickness, two and one-half to three inches in length, and an inch in width. The plates are then decalcified in a ten per cent. solution of hydrochloric acid, changed every twenty-four hours until they have become sufficiently soft that they can be bent in any direction without fracturing. After decalcification, they are washed by letting water flow over them from three to six hours, so as to remove the acid. The plates are then covered with porous paper and compressed between two pieces of tin until they are perfectly dry. If, during the process of drying, the plates are not compressed between two smooth surfaces, they become distorted by warping. The hardened plates are next drilled several times in a straight line in the centre, and the openings enlarged and connected with a file until the perforation is five-eighths of an inch in length and about one-eighth to one-sixth of an inch in width. The sharp margins of the plate and perforation are removed with a file. With a fine drill the four perforations for the sutures are made near the margins of the oblong perforation. One at each end and one at each side. For preservation the plates are kept in absolute alcohol. When the plates are to be used they are washed in a two per cent. solution of carbolic acid, and the threads or sutures attached by threading two fine sewing needles, each with a piece of aseptic silk twenty-four inches in length, which are tied together. The threads are then fastened to the surface of the plate by another thread passing through the perforations in the shape of a loop and fastened at the back. Instead of describing the experiments in their chronological order, I will enumerate them according to the part of the intestine operated upon, commencing with the upper portion of the intestinal tract.

(1) GASTRO-ENTEROSTOMY.

As gastro-enterostomy is an operation which establishes an anastomosis between the stomach and the upper portion of the intestinal canal with exclusion of the duodenum, and sometimes a portion of the jejunum, and is performed in cases of obstruction in the pylorus or duodenum, it comes within the legitimate sphere of this paper. Gastro-enterostomy, as heretofore described and performed, is an operation attended by many difficulties, and requires, even in the hands of an expert, an hour or more for its execution. As this operation is only done in cases greatly debilitated by disease and long suffering, anything which will simplify the technique and shorten the time must be looked upon as an improvement. An operation that can be done in ten minutes instead of an hour or two, and which even furnishes better conditions for the healing of the visceral wounds, must take the place of the more complicated procedure which so far has only been practiced in the hands of the most experienced surgeons.

EXPERIMENT LII.—Dog, weight twenty-five pounds. Incision made through linea alba from xyphoid cartilage to near umbilicus. Omentum pushed to one side and the stomach drawn forward into the wound; near the middle of its anterior surface a longitudinal incision was made, two inches in length, and a perforated gutta-percha plate to which four medium-sized juniper catgut sutures were attached was introduced. The lateral sutures, armed with needles, were passed through the entire thickness of the walls of the stomach half way between the angles of the wound. A similar incision was made into the intestine at the junction of the duodenum with the jejunum; the same kind of a plate introduced and the margins of the wound punctured by the lateral armed sutures, when the two wounds were brought *vis a vis* and the corresponding sutures tied. In tying the sutures the lower lateral suture is tied first, and the threads cut short, next the sutures corresponding to each angle of the wound are tied, and lastly, the upper lateral. The serous surfaces of the stomach and intestine over an area corresponding to the size of the plates, were brought into accurate permanent contact by the tying of the sutures. The stomach was replaced and the abdominal wound closed. The animal was allowed to eat immediately after the operation, and manifested no signs of illness or pain, and was killed seven days after operation. Abdominal wound healed. Omentum adherent to its inner surface. Union between stomach and bowel firm over the entire surface of approximation. Plates detached, the one in the bowel had passed, while the other was found loose in the stomach. The new opening large enough to pass the index finger through.

EXPERIMENT LIII.—Dog, weight fifty pounds. The operation was performed in the same manner as in the previous experiment, but great difficulty was experienced in bringing the stomach forward, as this organ was distended to its utmost with an enormous amount of solid food. Evacuation was effected through the incision, aided by attempts of the animal to vomit, the violent contractions of the stomach forcing the food toward the opening, from where it was removed with fingers and spoon. After the stomach was emptied it was washed out with warm water. For the stomach a bone plate, only partially decalcified, was used, while the approximation plate in the bowel was fully decalcified. The four approximation sutures were of catgut. Several portions of omentum which were soiled during the emptying of the stomach were excised. The abdominal cavity was thoroughly irrigated with warm water before the wound was closed. The animal died the next day, and on opening the abdomen it was ascertained that the immediate cause of death was hemorrhage, as the peritoneal cavity was filled with blood. The bleeding undoubtedly took place from the omentum, by slipping off or loosening of one of the catgut ligatures.

EXPERIMENT LIV.—Medium-sized dog. Operation performed in the same manner with decalcified bone plates and catgut sutures. The first two days the animal had several attacks of vomiting, subsequently showed no signs of suffering. Appetite good

and stools regular. Killed, thirty-four days after operation. Omentum adherent to inner surface of abdominal wound. At point of operation stomach is contracted, so that the organ presents an hour-glass appearance. Interior of the organ contains a large mass of hay and fragments of bone. No signs of plates or sutures. New opening, large enough to pass index finger. Union between stomach and bowel over entire surface of approximation. Water passed in the stomach flows through the pyloric orifice and the new opening in a stream of equal size.

EXPERIMENT LV.—Large bull-dog. Approximation of anterior surface of stomach with bowel by perforated gutta-percha plates, and four catgut sutures. Length of visceral incisions, two inches. The day after operation animal vomited his dinner, subsequently no unfavorable symptoms. Killed, fourteen days after operation. Abdominal wound well united. Omentum adherent to wound, duodenum, liver, and at point of operation. Firm adhesions between stomach and bowel. Water passed into the stomach only passed through the pyloric orifice. On opening the stomach it was found that the wound in the stomach and intestine had completely healed, the site of incisions being marked by a narrow, firm cicatrix. The failure of obtaining an anastomotic opening between the stomach and intestine could only be attributed to one of two causes, viz., either the perforation in the plates were too narrow, or the needles of the lateral sutures included too much tissue. Either cause would bring about approximation of the margins of the wounds and permanent closure of the opening by granulation and cicatrization.

Remarks.—All of the animals recovered, except in Experiment LIII, without any untoward symptoms, although they were allowed to eat immediately after the operation, and the diet was not selected or restricted at any time. In the fatal case, death was caused from complications which had no connection with the gastro-intestinal opening. In all of the specimens examined, the mucous membrane of the stomach and intestine which had been interposed between the approximation plates presented a healthy appearance, showing that the pressure of the plates had exercised no injurious effect on this structure. More recent experience with this operation on animals has revealed the fact that in the stomach a completely decalcified bone plate is digested almost completely in thirty-six to forty-eight hours. It would, therefore, appear advisable to use only partially decalcified bone, which remains for a longer time, so that in case of delayed union the approximation would be maintained for a sufficient length of time. As these animals subjected to the operation recovered promptly, and under the most unfavorable conditions, we have every reason to believe that this operation will be attended by the same favorable results when done for pyloric or duodenal stenosis in man, where a careful preparatory and after treatment cannot fail to facilitate the operation and to improve the conditions for the formation of early adhesions and a speedy definitive healing of the wound. I have no hesitation in recommending it as a substitute for the more time-consuming and less certain operation by the tedious and difficult method of double suturing which is now generally practiced.

(2) JEJUNO-ILEOSTOMY.

In this operation some form of intestinal obstruction is produced, either complete by division of the bowel and closure of both ends, or partial by making a volvulus, invagination, or flexion in the vicinity of the juncture of the jejunum with the ileum, and intestinal anastomosis made by establishing a communication between the bowel above and below the obstruction. Before I made use of the perforated approximation discs this was accomplished by making an incision, an inch and a half or two inches in length, through the convex surface of the bowel above and below the obstruction and uniting the wounds by a double row of sutures. An operation of this kind usually

lasted over an hour, while the rapid operation of coaptation by perforated discs seldom took more than fifteen minutes.

(a) JEJUNO-ILEOSTOMY BY SUTURING.

EXPERIMENT LVI.—Large cat. Invagination of ileum into ileum in a downward direction, and fixation of intussusceptum to neck of intussuscipiens by two fine catgut sutures, to prevent spontaneous reduction. Intestinal anastomosis, by establishing an opening an inch in length, suturing by Czerny-Lembert method. The animal never recovered from the shock of the operation, and died in less than twenty-four hours. Length of intussusceptum two inches, which, after the removal of the sutures, could not be reduced by traction, as the bowel was firmly constricted by the neck of the intussuscipiens and recent adhesions had formed. No peritonitis; suturing found perfect.

EXPERIMENT LVII.—Dog, weight sixty-five pounds. Intestinal obstruction by making several acute flexions in upper portion of ileum, fixation of loops of intestine by fine catgut sutures. Intestinal anastomosis between jejunum and ileum by incision and double suturing. The animal died on third day with symptoms of perforative peritonitis. On close examination, one of the superficial approximation sutures had been passed through the whole thickness of the wall of the bowel, and it was here that perforation had taken place. Recent diffuse general peritonitis.

EXPERIMENT LVIII.—Dog, weight seventeen pounds. Descending invagination of ileum into ileum; length of intussusceptum three inches; fixation by two catgut sutures. Formation of intestinal anastomosis between the bowel above and below the invagination by incision and double suturing. Animal died on third day with symptoms of perforative peritonitis. Abdominal wound not united. Adhesions at point of operation quite firm. Diffuse general peritonitis from a perforation which had been made by a sharp fragment of bone above the new opening. Intussusceptum not gangrenous.

EXPERIMENT LIX.—Dog, weight twenty-three pounds. Intestinal obstruction was made by producing a volvulus in the upper part of the ileum. Restoration of continuity of intestinal canal by making a jejuno-ileostomy by lateral apposition and double suturing. Day after operation intestinal discharges were bloody; after this time normal. Animal in perfect health when killed, sixty-seven days after operation. The volvulus was found in same condition as after operation; the intestinal loop empty, atrophied, and adherent to adjacent loops of intestine. Bowel above seat of obstruction, and as far as the new opening, empty. Intestinal tract above and below the obstruction presents no indications of the presence of an obstruction. New opening oval in shape and as large as the lumen of the bowel at that point.

EXPERIMENT LX.—Large Maltese cat. Intestinal obstruction by making two flexions in ileum about eighteen inches apart, after this portion had been cleared of its contents. Flexions made by doubling the bowel towards its convex side and fixing it in this position by fine catgut sutures. Jejuno-ileostomy by lateral apposition and suturing. Vomiting day after operation; stools scanty; the first few days, and later, complete obstruction; died nineteen days after operation. Wound completely united; no general peritonitis; flexions remained; bowel between them contained a slight amount of fecal matter. Bowel, some distance above the new opening, very much dilated, pointing to obstruction above new opening. On tracing the intestinal canal from above downward this obstruction is seen to consist in acute flexion of the bowel by firm and extensive adhesions. New opening sufficiently large to admit the tip of the index finger, around the margins of which most of the deep sutures remain attached.

EXPERIMENT LXI.—Large cat. Obstruction made by two flexions in the ileum, the apices of which were united by catgut sutures. Intestinal anastomosis made by a

jejuno-ileostomy. For eleven days the animal remained in good condition, when symptoms of perforative peritonitis manifested themselves, and death ensued two days later. External portion of wound not united. Numerous omental and intestinal adhesions. Flexions retained and their apices adherent to each other by firm band of adhesion. Excluded portions above and below the obstruction empty. Two small perforations at point of suturing on anterior surface of bowel; remaining portion of wound firmly united. New opening sufficiently large to admit tip of index finger. Death from perforative peritonitis.

EXPERIMENT LXII.—Large Newfoundland dog. Descending invagination of ileum into ileum to the extent of six inches; fixation of intussusceptum by two catgut sutures. Permeability of intestinal canal restored by making a jejuno-ileostomy; wounds united by a double row of sutures. Intestinal discharges normal throughout. No rise in temperature. General condition as good as before operation when killed, on the twentieth day. Abdominal wound completely united; no peritonitis; omentum adherent at site of operation. Invagination had reduced itself, and its location was marked by an acute flexion, caused by extensive adhesions. No accumulation of intestinal contents in excluded portions. The new opening at least two inches in length, a few of the deep sutures remaining attached to its margins. This opening was partially obstructed by a mass of hair and fragments of bone. On passing a stream of water from above downward, the fluid passed through an opening in the centre of this mass into the lower portion of the ileum, but not through the portion that was invaginated. After this mass was removed, the fluid was found to pass through the portion that was invaginated as well as through the new opening.

(b) JEJUNO-ILEOSTOMY BY PERFORATED APPROXIMATION PLATES.

The many failures which attended jejuno-ileostomy and ileo-ileostomy by lateral apposition and suturing, led to the use of perforated approximation discs. A great contrast was observed in the animals operated upon by these two methods. The operation by suturing required usually more than an hour, and almost all of the animals showed more or less symptoms of shock after its completion, and not a few succumbed to its immediate effects; while the operation by approximation plates could always be finished within twenty minutes, consequently, the animals never suffered seriously from the immediate effects of the operation. The first experiments were made somewhat carelessly and with crude material, and yet it was observed that the healing process progressed more favorably and was accomplished in a shorter time than after suturing. The approximation discs brought into uninterrupted contact large serous surfaces without impairing the vascular supply, at the same time they secured for the parts destined to become united an essential condition for rapid wound healing—rest—by serving the usual purpose of splints.

EXPERIMENT LXIII.—Dog, weight fifteen pounds. Ileum was completely divided at its junction with the jejunum, and both ends of the bowel closed by invagination, and three stitches of the continued suture. An incision was made on convex side of bowel about two inches from the closed ends, and a heavy perforated lead plate to which six catgut sutures were fastened around the oval perforation was introduced into the lumen of the bowel of each closed end, all of the catgut sutures being brought out through the incision. The two wounds were brought opposite each other and the six sutures tied. The serous surfaces of the two intestines over a surface corresponding to the size of the lead discs were thus brought into accurate apposition. The sutures were cut short and the ends buried as deeply as possible. The condition of the animal remained excellent until the time of killing; seventy-five days after operation. Omentum adherent to wound; large intestines distended with normal fæces. Bowel above and below point of operation normal in size and structure. New opening between ileum and

jejunum large enough to admit the little finger to second point. Bowels firmly united by a broad surface. Above the communicating opening a double flexion of the bowel was found which apparently had done no harm.

EXPERIMENT LXIV.—Dog, weight eighteen pounds. Operation done in the same manner as in the last experiment, only that instead of lead the discs were made of sole leather, and the sutures used were linen in place of catgut. For a few days the temperature was higher than normal and appetite diminished. After the fourth day the animal appeared to be in excellent condition, and remained so for three weeks, when the appetite failed and occasional attacks of vomiting set in. These symptoms remained more or less prominent until the time of killing, thirty-nine days after operation. Omentum adherent to abdominal wound ; extensive intestinal adhesions at site of operation; union between intestines perfect. On incising the bowel it was found that the plates had sloughed through and had passed along the distal portion of the bowel, leaving an opening the size of the plates, the margins of which had almost completely cicatrized. The two leather plates still held together by the linen sutures were found three feet lower down in the ileum, where they had become imbedded in a mass of hair, straw and fecal matter, and quite firmly impacted, causing complete obstruction of the bowel. The intestine above the seat of obstruction was enormously dilated, while below the seat of impaction it was empty and contracted. Large intestines likewise empty and contracted. The cause of the illness was evidently due to intestinal obstruction, produced by the impaction of the large enterolith, in the centre of which the leather discs were found.

EXPERIMENT LXV.—Dog, weight ten pounds. In this instance the bowel was divided near the junction of the jejunum with the ileum, both ends closed and its continuity established by incising the convex surface of both ends and approximating the wounds by two perforated bone plates tied together by silk ligatures. The animal died fourteen days after operation. During the last few days symptoms of intestinal obstruction were present. Abdominal wound completely united. Numerous intestinal adhesions at site of operation. Bone plates still *in situ* and firmly fixed. On proximal side perforation of bone plates completely closed by hair and fragments of bone, giving rise to complete intestinal obstruction. The bowel above this point was greatly dilated, while on distal side it was empty and contracted. Adhesions between the two intestinal surfaces included by the bone plates, firm. Intestinal obstruction, by a mechanical arrest of a portion of the intestinal contents above the proximal plate, had caused death before a more efficient communication could be established by sloughing through of the bone plates.

EXPERIMENT LXVI.—Dog, weight thirty pounds. Ileo-ileostomy, by dividing the ileum near its centre, closing both ends, and after incising both ends on convex surface, brought wounds in apposition by perforated plates of cross-grained walnut wood, which were tied together with silk sutures. The dog remained in perfect health and was killed, eighteen days after operation. External wound completely united. Plates had become detached, leaving a communicating opening two inches in length. Blind ends of bowel empty ; no trace of plates could be found.

EXPERIMENT LXVII.—Dog, weight twenty-four pounds. Double ileo-ileostomy. Ileum divided transversely five inches above ileo-cæcal region and both ends closed by invagination, and three stitches of the continued suture. Lower and upper end of bowel were again brought into communication by incision on convex side and lateral apposition of wounds by means of perforated approximation plates of decalcified bone, hardened in alcohol. The plates were fastened together by four silk sutures, all of the threads being brought out of the incision, tied and cut short. Above this point, a loop of the ileum was made by bringing the convex surfaces into apposition after incision at two points, and introducing perforated gutta-percha plates which were retained in

place by four silk sutures. No fever or symptoms of obstruction followed the operation. Animal killed, thirteen days later. External wound firmly united. No evidences of peritonitis or intestinal obstruction. First operation left a communicating opening large enough to admit the little finger; in one of its margins the silk ligatures which had become detached from the plates had imbedded themselves. The decalcified bone plates had disappeared and no trace of them could be found in any portion of the intestinal canal lower down. The second operation was thirty inches higher up. Gutta-percha plates remain *in situ*, although somewhat loosened by the gradual disappearance of the intervening tissues by pressure atrophy. Adhesions between the two surfaces of the bowel firm and extending a little beyond the line of approximation. The perforation in the proximal plate almost completely closed by an accumulation of hair. The entire ileum normal in size and appearance.

EXPERIMENT LXVIII.—Dog, weight fifty-four pounds. Transverse section of ileum thirty inches above ileo-cæcal region and closure of both ends in the usual manner. The two closed ends were overlapped four inches and brought into communication by two longitudinal openings, which were approximated by being buttoned together with a shuttle-shaped button, nearly one and one-half inches in length, the sides being lead plates and the shaft a rubber tube, through which the anastomosis was established at once. As the margins of the intestinal wounds showed a tendency to evert, a fine catgut suture was inserted on each side, embracing only the peritoneal coat. Only for two or three days after the operation did the dog not appear to be well. Killed, twenty-three days after operation. Omentum adherent to abdominal wound, which was firmly united. Omental adhesions to intestines at site of operation. Intestinal anastomosis thirty inches above the ileo-cæcal valve. Proximal blind end of bowel five inches in length, adherent to distal end, considerably dilated and contains fragments of bone and other crude substances. Approximation button *in situ* and quite firmly fixed. A fragment of bone partially fills the lumen of the rubber tube. Coaptated peritoneal surfaces firmly adherent. The obstruction of the communicating tube had given rise to dilatation of the bowel above the point to twice its natural size, while below the seat of partial obstruction the intestine appeared empty and contracted.

EXPERIMENT LXIX.—Small dog. In this experiment the ileo-ileostomy was made by lateral apposition by perforated approximation plates of partially decalcified bone tied together by four catgut sutures. The lateral sutures were passed through the margins of the wound near its border, a modification of the usual procedure, which not only fixed the plates firmly in their places, but also prevented ectropium of the mucous membrane, and ensured free patency of the new opening by retracting the margins of the wound, so that the longitudinal slit is at once transformed into an oval shape. The animal showed no unfavorable symptoms and was killed twenty-nine days after operation. Dog well nourished. External wound united. Omentum adherent to wound and intestines. The proximal blind end of the bowel contained one of the bone plates which showed signs of softening and disintegration. The bone plate in the distal end had been passed with fæces previously. The new opening perfect and sufficiently large to equal in size the lumen of the bowel.

EXPERIMENT LXX.—Dog, weight twelve pounds. Made ileo-ileostomy the same as in the last experiment, using decalcified, perforated bone plates, which were tied together with four catgut sutures, the lateral ones being passed through the margins of the wound. An omental flap was used to cover the sides of the bowel where approximation had been made. This flap was retained by two fine catgut sutures. No unfavorable symptoms. Animal killed, twenty-three days after operation. Omentum adherent to distal blind end. Omental flap in position and firmly adherent. Site of operation fourteen inches above ileo-cæcal region. Both bone plates had disappeared and no trace

of them could be found. Some hair had collected in the blind proximal end. New opening large enough to admit the index finger.

Remarks.—Jejuno-ileostomy and ileo-ileostomy by lateral apposition with decalcified perforated bone plates, in cases of complete obstruction of the bowel artificially produced, is an operation almost devoid of danger. Partially or completely decalcified bone plates, hardened in alcohol, remain firm for a sufficient length of time to answer the purpose of retentive measures, until firm adhesions have formed between the serous surfaces held by them in approximation. Until it was ascertained by experiment that the plates would undergo softening and disintegration in the course of a few days, catgut sutures were used to hold them in place, with the expectation that the plates would become detached and escape with the intestinal contents as soon as the sutures would give way. Experience, however, has shown that aseptic silk threads are preferable to catgut, as they can be tied with greater accuracy, and the knots will never become loosened, while the approximation discs disappear completely, by softening and disintegration, in a few days. Approximation plates of inabsorbable material, as lead, wood, leather, bone and gutta-percha, fastened together by silk or linen sutures, remain *in situ* until the interposed tissues disappear by pressure atrophy, and the opening that results corresponds in size to the dimensions of the plates. In the first experiments the plates were tied together by six sutures, but it was found that four sutures answered the same purpose. As a rule, the plates were about two and one-half inches in length, and their width corresponded to about one-third of the circumference of the bowel. The greatest advantage to be found in the methods of restoring the continuity of the intestinal canal by lateral apposition by approximation discs consists in the fact that the point of contact is always made on the convex surface of the intestines, so that the means resorted to to secure coaptation do not interfere with the blood supply from the mesenteric vessels. As this method requires much less time than any form of circular enterorrhaphy, and has been followed almost without exception by recovery, it recommends itself strongly as a substitute for the latter procedure in many cases where loss of time constitutes an important factor in the issue of the case, or where from other causes circular suturing appears impossible or impracticable.

(3) ILEO-COLOSTOMY.

As the ileo-cæcal region is frequently the seat of intestinal obstruction, it becomes desirable to devise some definite line of operative treatment in cases where the cause of obstruction is not amenable to removal, with a view of establishing the continuity of the intestinal canal, thus avoiding the necessity of resorting to the formation of an artificial anus. To accomplish this object, two distinct methods were followed: (1) Division of the ileum with closure of distal and implantation of proximal end into colon. (2) Division of ileum, closure of both ends and lateral apposition of proximal end with colon, and the formation of an intestinal anastomosis by suturing or approximation discs.

EXPERIMENT LXXI.—Dog, weight thirty-eight pounds. Intestinal anastomosis, by implantation of the ileum into colon. The ileum was divided transversely, just above the ileo-cæcal region, and the distal end closed by invagination and three stitches of the continued suture, and dropped back into the abdominal cavity. A longitudinal incision in size, corresponding to the lumen of the ileum, was made in the ascending colon at a point directly opposite the mesenteric attachment, and the proximal end of the ileum was then fixed in this opening by Czerny-Lembert sutures. Only slight febrile reaction followed the operation. The appetite remained good and the discharges from the bowels were normal. The animal was in excellent condition when killed, thirty-three days after operation. Few circumscribed omental adhesions to

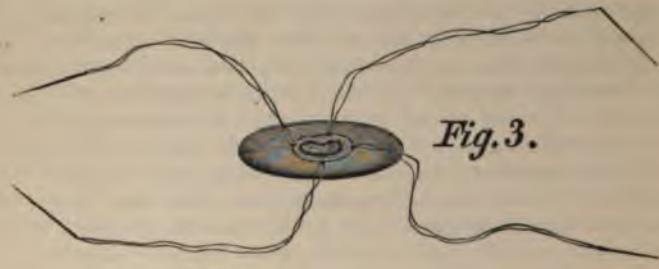
abdominal wound, which was completely closed. Peripheral portion of ileum presents a conical appearance, and was found adherent to, and of the same length as, the appendix vermiformis. Implantation had been done about the middle of the colon. Union at point of suturing perfect, apparently no interruption of continuity of peritoneal surface. The new opening into colon a little smaller than the lumen of the ileum. Around the margins of this opening, which somewhat resembles the ileo-cæcal valve, six of the deep silk sutures remain attached. Above the new opening the colon and cæcum were found empty and somewhat atrophic. Lower portion of the ileum and colon, below the new opening, appear normal in size and structure.

In the remaining experiments the implantation was made by lining the proximal end of the ileum with a narrow flexible rubber ring, which was retained in place by a continued catgut suture, embracing the free margin of the bowel and the lower margin of the rubber ring. The implantation was made by two catgut sutures threaded each by two needles and passed at opposite points from within, outward, through the upper margin of the ring and the entire thickness of the bowel, while the needles were only passed through the serous and muscular coat of the colon. After both sutures were in place, gentle traction upon all of the ends brought the end of the ileum into the incision in the colon, and the walls of the colon were drawn over the end of the ileum to the points where the needles emerged from the ileum, making really a limited invagination. When in proper position, the serous surfaces of the colon and ileum, over a surface corresponding to the width of the rubber ring, were in accurate coaptation after the two sutures were tied. Only in exceptional cases was it found necessary to apply one or two additional superficial coaptation sutures. As in circular enterorrhaphy, so in these cases, the elastic pressure of the rubber ring rendered material assistance in maintaining accurate coaptation, while at the same time it secured rest for the sutured parts, and kept the new opening freely patent for the escape of intestinal contents into the colon. This operation did not require one-fourth of the time consumed in making an implantation by Czerny-Lembert sutures.

EXPERIMENT LXXII.—Dog, weight fifty pounds. Division of ileum eight inches above ileo-cæcal region, distal end closed by invagination, and three stitches of the continued suture. Proximal end lined with rubber ring and implanted into incision of ascending colon by two catgut invagination sutures. The dog did not appear to do well after the operation, and died on the fifth day. Abdominal wound not united. Partial separation of implanted bowel and diffuse septic peritonitis from perforation.

EXPERIMENT LXXIII.—Dog, weight thirty-five pounds. Ileum divided twelve inches above ileo-cæcal region, distal end closed and proximal end lined with flexible rubber ring and implanted into an incision in the transverse colon and retained by two invagination sutures of catgut. An omental flap an inch and a half in width was placed over the junction of the two intestines, and fixed in its place by two catgut sutures. No unfavorable symptoms after operation. Animal when killed, eighteen days later, in excellent condition. Omentum adherent to abdominal wound, which was firmly united. Omental flap adherent all round. Colon above new opening, ten inches in length, completely empty, contracted and atrophic. New opening, oval in outline and as large as the lumen of the ileum.

EXPERIMENT LXXIV.—Dog, weight sixteen pounds. Division of ileum, closure of distal end and implantation of proximal into an incision of the colon by rubber ring and two invagination sutures of catgut. As the inverted portion of the colon showed a tendency to evert, two additional retaining sutures of fine catgut were used, which secured perfect coaptation throughout. An omental flap was laid over the junction of the intestines and fixed in its place by one catgut suture. The dog remained in good condition, appetite unimpaired, and discharges from bowels normal. Killed, thirteen days after operation. Abdominal wound firmly united. Omentum adherent



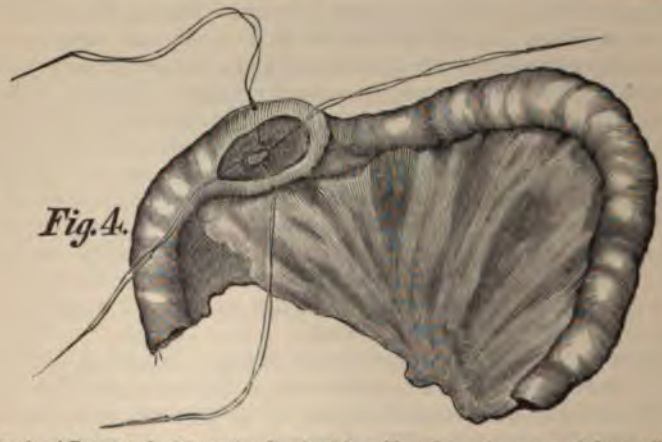


FIG. 1.—Afferent end of intestine lined with rubber-ring sutures passed from within outward. (See p. 454.)
FIG. 2.—Invagination completed and sutures tied. (See p. 454.)

FIGURES 1 and 2 illustrate author's modification of Lembert's Invagination Suture.

FIG. 3.—Decalcified perforated bone plate. (See p. 461.)
FIG. 4.—Plate in intestine above seat of obstruction. (See p. 462.)

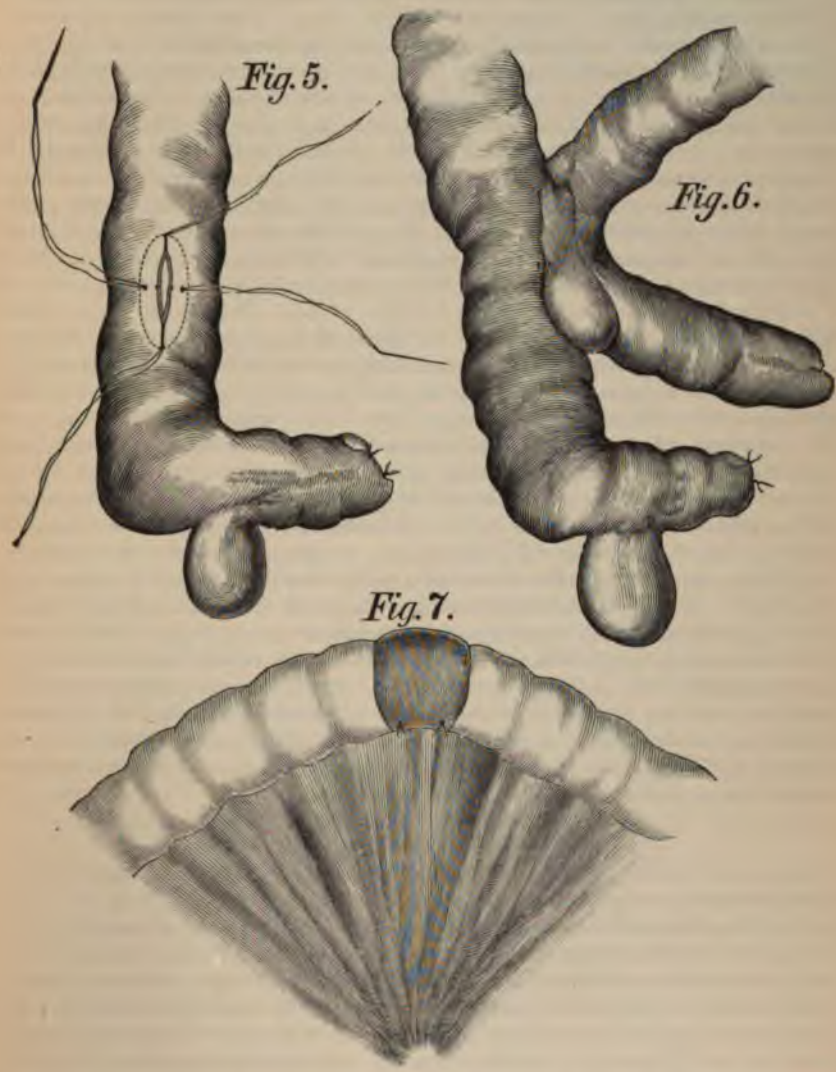


FIG. 5.—Plate in colon below seat of obstruction. Dotted lines represent size of plate. (See page 475.)
FIG. 6.—Approximation of intestines by tying of the sutures. (See page 441.)
FIG. 7.—Omental grafting. (See page 482.)

to wound. A number of adhesions between coils of intestines. Ileum somewhat dilated above the new opening. Omental flap in place and adherent. Union between ileum and colon perfect. A long, sharp fragment of bone was found lodged just above the new opening, its lower end partially occluding its lumen. The dilation of the lower portion of the ileum was evidently due to partial obstruction from the presence of the foreign body in the new opening.

EXPERIMENT LXXV.—Dog, medium size. Section of ileum two feet above the ileo-cæcal region, closure of distal end in the usual manner; implantation of proximal end into colon by rubber ring and two invagination sutures of catgut. No omental flap. Animal remained well and was killed forty-three days after operation. Omentum adherent to abdominal wound. Distal end of ileum conical in shape, the extremity presenting a cup-shaped depression, which was filled with cicatricial material. Omentum adherent at ileo-cæcal region and at site of operation. Union between the bowels perfect, and their serous surfaces appear to be continuous over the line of junction. The new opening from the colon admits the little finger, and is surrounded by a prominent ridge of mucous membrane, which resembled the ileo-cæcal valve.

EXPERIMENT LXXVI.—Dog, weight fourteen pounds. Division of ileum a few inches above the ileo-cæcal valve; distal end closed by invagination and three stitches of continued suture. Implantation of proximal end into colon by rubber ring and two catgut invagination sutures. Over the junction of the two intestines an omental flap was placed, which was retained by a catgut suture. The animal showed no unfavorable symptoms, and was killed twenty-eight days after operation. Omental flap retained and firmly adherent throughout. Point of implantation three inches above cæcum; union between the two intestines firm throughout. New opening corresponds in size to the lumen of the ileum, and is surrounded by a prominent ridge of mucous membrane, which appears to be derived from the invaginated portion of the ileum.

EXPERIMENT LXXVII.—Ileum divided a few inches above ileo-cæcal region, and after closure of distal, the proximal end was implanted into the colon in the usual manner by means of rubber ring and two invagination sutures of catgut. Animal died on the third day after operation. Wound partially united; a considerable quantity of sero-sanguinolent fluid in the abdominal cavity. Ileum almost completely separated from colon, and the portion which had been invaginated showed signs of gangrene. Rubber ring had disappeared; death from perforative peritonitis. In this case we have reason to believe that the rubber ring which was used was too large, and that the gangrene and separation were due to injurious pressure.

(a) ILEO-COLOSTOMY BY LATERAL APPOSITION.

Anastomosis by this method was made after producing an intestinal obstruction of some kind at or near the ileo-cæcal region, and then by bringing the ileum above the seat of obstruction in communication with the colon below the point of obstruction by making an incision an inch and a half to two inches in length in both intestines at a point opposite the mesenteric attachments, and uniting the wounds either by a double row of sutures or perforated decalcified bone discs. The first experiments were all made by suturing, but, as in circular enterorrhaphy, it was found by experience that perforation not infrequently occurred along the track of one of the sutures; in some instances, several days after the operation, at a time when union had taken place by firm adhesions. These unfavorable results led to the use of the perforated approximation discs.

EXPERIMENT LXXVIII.—Dog, weight twenty-five pounds. The ileum was withdrawn from the abdomen through an incision in the linea alba, and having emptied a loop of its contents, acute flexion was made just above the ileo-cæcal region by approximating the serous surfaces of the convex side for an inch and a half by five catgut

sutures. Two longitudinal incisions of equal size were made, one in the ileum six inches above the flexion, and the other in the ascending colon three inches above the cæcum. The visceral wounds were carefully united by Czerny-Lembert sutures, using silk for the deep interrupted sutures, and fine catgut for the superficial continued sutures. No untoward symptoms were observed after the operation; appetite remained unimpaired, and fecal discharges were normal. The dog was killed, thirty-seven days after operation. Animal well nourished. No evidences of peritonitis. Bowel above point of obstruction nearly empty, and somewhat contracted as far as the new opening. Flexion permeable to a stream of water. Slight omental adhesions to bowel at site of operation; union firm throughout. Lumina of non-excluded portion of bowel normal in size above and below the flexion. Serous surfaces at point of junction appear perfect and continuous. On slitting open the colon opposite the new opening, its outlines were seen to be marked by a prominent ridge of mucous membrane, to which a number of the deep sutures remained attached. The opening was large enough to admit the tip of the middle finger. The excluded portion of the colon and the cæcum were somewhat contracted and atrophic, and contained only a very small quantity of fecal matter.

EXPERIMENT LXXIX.—Medium-sized cat. About two inches of the ileum were invaginated into the colon through the ileo-cæcal valve, and the intussusceptum stitched to the neck of the intussuscipiens by two fine catgut sutures. Continuity of the intestinal canal restored by incising the ileum above the obstruction and the ascending colon below the free extremity of the intussusceptum, and uniting the wounds by a double row of sutures. The invagination caused no serious disturbance and the animal remained in good health, and was in excellent condition at the time of killing, 162 days after operation. A number of adhesions between the folds of the intestines near the site of operation. At point of junction of the two intestines the peritoneal surface presented a glistening and continuous surface. New opening an inch and a half in length, oval in outline and located five inches above the ileo-cæcal region. Two inches below the opening the invagination remains in the shape of a circular thickening of the bowel, with a narrowing of its lumen to more than one-half of its normal size. A close inspection of the specimen shows that no gangrene has occurred, but that the intussusceptum has undergone atrophy. A stream of water passing along the ileum in a downward direction escapes through the invaginated portion and through the new opening, the stream from the latter being at least three times larger than the one through the intussusceptum. Excluded portion of ileum and colon empty and very much atrophied and contracted. Below the new opening the colon and rectum contained normal fæces in considerable quantity.

EXPERIMENT LXXX.—Young cat. Ileo-cæcal invagination; length of intussusceptum four inches, and in order to prevent spontaneous disinvagination the bowel was fixed in its position by two fine catgut sutures. Ileo-colostomy below the lower end of the intussusceptum by lateral apposition and suturing. Animal died on the fourth day after operation. Abdominal wound united. Diffuse peritonitis from perforation at site of suturing. Length of intussusceptum reduced from four inches to two inches and a half. It was found impossible to effect reduction by traction, on account of firm adhesions at neck of intussuscipiens. No gangrene.

EXPERIMENT LXXXI.—Adult large dog. Intestinal obstruction was produced by making two sharp flexions near the ileo-cæcal region by folding the bowel on its side and fixing it in this position by fine catgut sutures; the apices of the flexions were sutured together, so as to render the obstruction more complete. Intestinal anastomosis was established by lateral apposition and suturing. Physical condition of dog remained good throughout; appetite and evacuations normal. Killed, thirty-one days after operation. No peritonitis; a number of omental adhesions at point of operation.

Flexions quite sharp, rendering the bowel nearly, if not completely, impermeable at this point. Perfect union between bowels, with some thickening of their walls by inflammatory exudation. New opening oval in shape, an inch and a half in length, a few of the deep sutures still remaining attached to its margins. Excluded portion of bowel empty and somewhat atrophic.

EXPERIMENT LXXXII.—Dog, weight thirteen pounds. Obstruction of the bowels made by an acute flexion four inches above the ileo-cæcal region, retained by four catgut sutures. Intestinal anastomosis by an opening an inch and a half in length, which brings into communication the ileum above the obstruction and the descending colon. The animal showed no untoward symptoms and was killed, forty-one days after operation. A number of intestinal folds agglutinated by adhesions; no evidences of diffuse peritonitis. Where the flexion had been made the loop of intestine is connected by a broad band of adhesion, which gives to the bowel a horseshoe-shaped appearance. Intestine below the seat of flexion contains a small amount of hardened fæces. Colon and cæcum above the new opening nearly empty and greatly contracted. Line of suturing somewhat thickened. New opening oval in outline and about an inch in length, surrounded by a corrugated elevation of mucous membrane. A stream of water, passed through the bowel from above downward, readily escapes through the new opening, while only a small stream can be forced through the flexion.

EXPERIMENT LXXXIII.—Dog, weight twenty-seven pounds. A volvulus was made six inches above the ileo-cæcal region by rotating an empty loop of the intestine once around its axis and fixing it in this position by three catgut sutures. Intestinal anastomosis between the ileum above the volvulus and the descending colon, by lateral apposition and suturing. For four days after the operation, the evacuations from the bowels contained blood; after this time the stools were normal. Dog in excellent condition when killed, thirty-one days after operation. No signs of diffuse peritonitis. The portion of bowel which constitutes the volvulus adherent, contracted and empty. Water can be readily forced through this part of the bowel. Cæcum and colon above new opening empty and contracted. Size of new opening larger than the lumen of the ileum, its margins surrounded by a prominent ridge of mucous membrane, to which a few of the deep sutures still remain attached. In this experiment, nearly the entire colon was excluded, consequently the fecal discharges were quite frequent and fluid or semifluid in consistence.

EXPERIMENT LXXXIV.—Dog, weight seventeen pounds. Two inches of the ileum were invaginated into the cæcum. Ileo-colostomy, by uniting the ileum with the transverse colon by suturing. The animal appeared quite ill after the operation and died on the fifth day, after having manifested well-marked symptoms of perforative peritonitis. Abdominal wound not united. Only partial union between the intestines at point of junction. Diffuse septic peritonitis from perforation.

Remarks.—In at least two experiments, which are not here reported, the animals died a few hours after the operation of shock. In a number of other experiments the operation was followed by more or less shock, but the animals, without receiving any special treatment, rallied after six to twelve hours. The symptoms referable to the immediate effects of the operation were due to the length of time required in applying a double row of sutures in uniting the visceral wounds, a step in the operation which always required from thirty minutes to an hour. These experiments only corroborate the statement previously made that the excluded portion of the intestinal canal, including the obstruction, does not become the seat of fecal accumulation, but undergoes atrophy after free intestinal anastomosis has been established between the intestine above and below the seat of obstruction. Experiments LXVIII and LXIX furnish most striking proof that the danger of gangrene in cases of invagination is greatly diminished by establishing an early intestinal anastomosis, as when this is done the violent peri-

stalsis is promptly arrested by furnishing a new outlet to the intestinal contents; at the same time, the serious consequences resulting from pressure and distention above the obstruction are likewise promptly averted. In cases of intestinal anastomosis, where nearly the entire colon has been excluded, the fluid contents of the small intestines reach the rectum at once, and cause frequent fluid fecal discharges, an occurrence which does not appear to impair the general health of the animal. The new opening should be made of adequate size, so that its lumen will at least correspond to that of the bowel above the obstruction.

(b) ILEO-COLOSTOMY BY PERFORATED APPROXIMATION DISCS.

EXPERIMENT LXXXV.—Dog, weight twenty pounds. The ileum was completely divided three inches above the ileo-cæcal region, both ends closed by invagination and three stitches of the continued suture. A communication was established between the proximal extremity and the colon, by making an incision into the ileum on convex side near the closed end and introducing through this opening a perforated decalcified bone plate. A similar opening was made into the ascending colon, opposite its mesenteric attachment, through which a perforated plate of wood was introduced. To each plate were tied four catgut sutures. The lateral sutures were passed through the margins of the wound. After the plates and sutures were in place the wounds were brought in contact and the four sutures tied, which accurately coaptated the serous surfaces of both bowels over an area corresponding to the size of the plates. The animal remained apparently well for two days, when symptoms of peritonitis set in and death occurred, five days after operation. Diffuse peritonitis. Union at point of operation incomplete, which resulted in a perforation. Discs had disappeared. As the catgut sutures were quite fine, it is more than probable that partial separation of the plates occurred before adhesions had taken place between the serous surfaces of the coaptated bowels, which resulted in perforation and death from diffuse septic peritonitis.

EXPERIMENT LXXXVI.—Dog, weight fifteen pounds. Invagination of colon into colon to the extent of two inches. Intestinal anastomosis, by making an ileo-colostomy by lateral apposition of the ileum to colon below the invagination, using perforated hard rubber plates, which were tied together by four catgut sutures, the lateral sutures being passed through the margins of the wound. After tying the sutures it was found that at one point the margin of the wounds showed a tendency to evert, consequently, a fine catgut suture was passed through the peritoneum only and tied. The animal did not appear bright the day after operation, but subsequently showed no signs of suffering; killed, twenty-four days after operation. Abdominal wound firmly united. Omentum adherent to wound and at point of operation. The invagination was partially reduced. The bowel at this point was curved in the shape of a horseshoe, but permeable to a stream of water. Excluded portion of colon tortuous and atrophic. Cæcum contained a small quantity of fluid fæces. Plates could not be found. New opening sufficiently large for free passage of intestinal contents.

EXPERIMENT LXXXVII.—Dog, weight fifteen pounds. Ileum divided transversely twelve inches above the ileo-cæcal region; both ends closed in the usual manner. Ileum and colon approximated by decalcified perforated bone plates which were tied together by four catgut sutures; the lateral ones transfixed the margins of the wounds. On the second day the evacuations from the bowels contained traces of blood. Animal killed, thirteen days after operation. Abdominal wound completely healed. Omentum adherent to wound. Numerous adhesions between the intestinal folds. Proximal blind end of ileum had been changed into a pouch-like form and contained a mass of hair and fragments of bone. One very sharp spicula of bone had nearly perforated the intestine. New opening corresponds in size to the lumen of the ileum.

Remarks.—The operations of lateral apposition of ileum to colon by perforated

approximation discs have shown that it is unsafe to rely upon catgut as a suturing material, as when fine catgut is used coaptation is not maintained for a sufficient length of time for adhesions to take place, and coarse catgut when tied interferes with accurate approximation, as the knots after tying mechanically separate the serous surfaces. It is advisable to use removable plates and to tie with silk. The results of ileo-colostomy made by approximation discs have not been as favorable as after jejuno-ileostomy or ileo-ileostomy, and in repeating the operation on man it would be indicated, after bringing the intestines in apposition by tying the four sutures, to apply a number of superficial sutures for the purpose of still further guarding against the escape of gas or fluid contents into the peritoneal cavity. The plates, when properly fixed in their places and tied together with sufficient firmness, not only coaptate an extensive area of serous surfaces, but they at the same time secure a perfect rest for the parts which it is intended to unite, until firm adhesions have formed.

(4) ILEO-RECTOSTOMY.

In cases of intestinal obstruction, due to inoperable conditions low down in the colon, it becomes necessary to establish an intestinal anastomosis between the ileum and the rectum in order to avert the necessity of making an artificial anus, in other words, to make an ileo-rectostomy. The operation can be made in the same way as establishing a communication between the ileum and the colon by lateral implantation, by lateral apposition and double suturing, or by lateral apposition by perforated decalcified bone plates. The operation is, however, more difficult because the rectum is not as accessible as the colon, and from the greater vascularity of the gut the incision is more liable to give rise to troublesome hemorrhage. While the slight hemorrhage from an incision into the small intestines and the colon is usually promptly arrested by suturing or compression by the approximation discs, the bleeding from a wound of the upper portion of the rectum not infrequently requires the application of one or more catgut ligatures before it is safe to unite the wounds. During the operation, traction must be made upon the rectum in an upward direction, so as to lift the upper portion of the bowel out of the pelvis. In both of the experiments described below the wounds were united by Czerny-Lembert suture.

EXPERIMENT LXXXVIII.—Dog, weight thirty pounds. Invagination of colon into colon for two inches and suturing of the intussusceptum to neck of intussuscipiens by four fine silk sutures, to prevent spontaneous disinvagination. Ileum incised in a parallel direction for an inch and a half on convex side, and this wound united with a similar incision in the rectum on its anterior surface by a double row of sutures. For the purpose of immobilizing the sutured intestines an additional fine catgut suture was applied above and below the place of suturing, embracing only the peritoneal and muscular coats of the intestines. On the third, fourth, and fifth day the fecal discharges contained blood and mucus. On the sixth day the abdominal wound partially opened, and a considerable quantity of sero-purulent fluid escaped. Death seven days after operation. Abdominal wound not united. Diffuse purulent peritonitis. Numerous intestinal adhesions. Invagination retained; adhesions between intussusceptum and intussuscipiens; no gangrene; perforation at point of operation.

EXPERIMENT LXXXIX.—Cat, weight seven pounds. Ileo-rectostomy by lateral implantation. The ileum was cut across transversely, an inch above the ileo-cæcal valve and the distal end closed by invagination, and three stitches of the continued suture. The proximal end was implanted into a longitudinal incision on the anterior surface of the upper portion of the rectum by Czerny-Lembert suture. With the exception of an occasional slight rise in temperature no serious disturbances were observed during the progress of the case. The evacuation of the contents of the small

intestines directly into the rectum, appeared to increase the peristaltic action of the rectum, as the fecal discharges were fluid and frequent. Animal killed, twenty days after operation. Abdominal wound completely united. No peritonitis. A few folds of the small intestines and the omentum adherent to the wound. Insertion of ileum into rectum in an oblique direction; union at point of junction complete throughout; intestinal coats at this point somewhat thickened. Peritoneal surface smooth and continuous from one bowel to the other. New ileo-rectal opening corresponds in size to the lumen of the ileum; margins of this opening consist of a ridge of mucous membrane, to which a few of the deep sutures remain attached. Excluded portion of large intestine empty and contracted. Rectum contained a small quantity of fluid fæces.

(5) COLO-RECTOSTOMY.

Among the many possibilities in the operative treatment of intestinal obstruction, a condition might be met with where the seat of obstruction might be located low down in the colon, perhaps in the sigmoid flexure, and where it might be impossible or impracticable to remove the cause of obstruction, and where it becomes necessary to restore the continuity of the intestinal canal by establishing a communication between the permeable portion of the colon and the rectum. Such an anastomosis can be made as in ileo-colostomy, by lateral implantation, lateral apposition by perforated approximation plates, or by double suturing. For want of time only one experiment was made, and although the animal died of the immediate effects of the operation, the local conditions at the site of operation, found after death, show that colo-rectostomy in selected cases is not only a justifiable and feasible operation, but whenever it can be done, it is always preferable to the formation of an artificial anus. As the operation by lateral apposition requires much less time than lateral implantation, it should be perferred to the latter procedure, and should be done by perforated approximation discs and a few superficial sutures.

EXPERIMENT XC.—Medium-sized cat. Incision through the linea alba; colon cut transversely in the middle third and the distal portion, and the rectum cleared of its contents by injecting a stream of warm water from the cut end downward, a procedure which could only be well accomplished after forcible dilatation of the sphincter ani muscles. The distal end was closed in the usual manner. The rectum was drawn upward and an incision made into its anterior wall, large enough to correspond with the lumen of the colon. Into this opening the proximal end of the colon was implanted by two rows of sutures. During the latter part of the operation, which lasted over an hour, the animal was seized by convulsions, which continued for several hours, and finally subsided under the administration of whiskey, given hypodermatically. The symptoms of shock, however, continued, and death occurred thirty-six hours after operation. Numerous omental adhesions; closed end of bowel congested; peritoneal surfaces adherent. Colon and rectum at point of implantation adherent.

Remarks.—In cases where the obstruction is located some distance from the rectum, where it would be impossible to approximate the permeable portion of the colon with the rectum, the entire colon must be excluded and the continuity of the intestinal canal restored by ileo-colostomy or ileo-rectostomy. In all cases of intestinal anastomosis, where the communication is made in the lower portion of the colon or the rectum, the sphincters of the anus should be rendered temporarily incompetent by stretching, for the purpose of guarding against over-distention of this part of the bowel during the time required for the healing process between the united intestines.

VI. ADHESION EXPERIMENTS.

In works on abdominal surgery we invariably meet with the assertion that serous surfaces brought into apposition by suturing unite after a few hours. Isolated experiments and the results of post-mortem examinations have given rise to the general belief that serous surfaces, so united, will become firmly adherent in a very short time; but the question concerning the exact time for adhesion to take place, and for the definitive healing to be complete, can only be determined by experiments made for this special purpose. The following experiments were made with a view of ascertaining the exact time which is requisite for adhesions and definitive healing between approximated serous surfaces to take place, and likewise to study the effects of local conditions, which would hasten or retard these processes. It is quite important to make a distinction between the term "adhesion" and "healing." Adhesion precedes the process of definitive healing, but implies simply the presence of an adhesive or cement substance between the serous surfaces, which mechanically agglutinates the parts, while definitive healing includes all the processes which take place during cicatrization. In intestinal surgery this distinction has an important practical bearing, as perforation may take place as long as the serous surfaces are simply held together by adhesions, while such an occurrence is beyond the reach of all possibilities after the approximated surfaces have become united by living, organized tissue. Adhesions between serous surfaces take place by the exudation of plastic lymph, which acts the part of a cement material; while, on the other hand, the process of definitive healing is initiated by cell proliferation from the preëxisting endothelial and connective tissue cells, and the formation of a network of new blood vessels springing from each of the coaptated granulating surfaces. The processes are the same as we observe them within blood vessels during cicatrization after ligature. In suturing an intestinal wound, or in making a circular enterorrhaphy, it has always, heretofore, been deemed necessary not to injure the peritoneum unnecessarily, for fear that such injuries would result deleteriously by interfering with the prompt union between the sutured surfaces. It is a well-known fact in surgery that approximation of intact serous surfaces does not result in the formation of adhesions. When the surgeon desires to secure union between serous surfaces, he resorts to mechanical or chemical irritation for the purpose of inducing a circumscribed plastic peritonitis, which invariably results in adhesions and the obliteration of the serous space. Reasoning from this analogy, I was induced to study the effects of traumatic and chemical irritation in hastening adhesions and cicatrization between opposed serous surfaces. In most of these experiments, the serous surfaces in the different operations were held in contact by perforated approximation plates, and in case artificial means were resorted to to expedite the healing process, the fact is mentioned and the result of such modification noted. The animals operated on were all dogs.

Time, six hours.

EXPERIMENT XCI.—The ileum was divided near its middle, and both ends closed by invagination and the continued suture. Ileo-ileostomy was made at two points, making two openings of communication. No suturing. Parts kept in apposition by perforated decalcified bone plates. To compare the effect of traumatic irritation of the peritoneum in the reparative process with the intact serous surface, the peritoneal surfaces at one point of operation, designated as the upper, were scarified with the point of a needle over an area corresponding to the size of the bone discs, the scratches being made sufficiently deep to penetrate the entire thickness of the peritoneum. The scarifications were made in a longitudinal and transverse direction, mapping out the serous surfaces into small squares. Only slight oozing followed this procedure. The serous surfaces between the plates (No. 1), where no scarification was made, were found slightly adherent by a scanty deposit of plastic lymph. At No. 2, where scari-

fication had been done, the amount of plastic lymph was greater and stained by blood, and the adhesions much firmer.

Time, twelve hours.

EXPERIMENT XCII.—In this experiment the bowel was not interrupted by division, but two adjacent coils of the ileum were united by making an ileo-ileostomy by perforated decalcified bone plates, plates holding the parts perfectly in apposition; a slight tumefaction of the intestinal walls had made the coaptation more secure. Coaptated serous surfaces very vascular, covered with a thin layer of plastic lymph which has agglutinated the folds of the intestine brought in contact.

EXPERIMENT XCIII.—Bowel not divided, but two adjoining loops of the ileum united by making a double ileo-ileostomy by perforated approximation discs, the two communicating openings about six inches apart. At one point of operation, designated as No. 2, serous surfaces freely scarified. At both points the adhesions were perfect throughout, but where a scarification was made they were notably firmer.

EXPERIMENT XCIV.—In this experiment a gastro-enterostomy and an ileo-ileostomy were made at the same time and on the same animal. In both operations the parts were coaptated by perforated decalcified bone plates. Scarification of peritoneal surfaces at both places. The adhesions between the anterior surface of the stomach and upper portion of the jejunum were uniform throughout, over the whole surface, kept in contact by the plates. There was no leakage on distending the stomach and intestine forcibly by water. The adhesions between the folds of the ileum at point of approximation were, if anything, firmer than between stomach and jejunum. The decalcified bone plate in the interior of the stomach was softened more than those in the intestine.

Time, eighteen hours.

EXPERIMENT XCV.—Gastro-enterostomy by perforated decalcified bone plates; communication made between stomach and upper portion of jejunum; no scarification. Agglutination quite firm, so that forcible distention of the stomach and bowel causes no leakage. New opening sufficiently large to admit middle finger, and apparently lined throughout by mucous membrane. Plate in stomach very much softened and on the verge of becoming detached. On forcibly separating the adhesions the serous surfaces are found to be cemented together by a thin layer of plastic lymph, and after scraping this away they appear vascular, rough, as though completely deprived of the endothelial covering.

Time, twenty-four hours.

EXPERIMENT XCVI.—Triple ileo-ileostomy without division of the bowel; the operations were numbered 1, 2, 3, respectively. Coaptation by approximation discs of decalcified bone. Communicating openings about six inches apart. In No. 1, no scarification. No. 2, scarification of one loop only. No. 3, scarification of both serous surfaces. After twenty-four hours, the result was as follows:—

(1) Lymph scanty; adhesions not very firm.
(2) Lymph more plentiful; adhesions firmer.
(3) Lymph more abundant than in No. 2, and mixed with a fine stratum of coagulated blood; adhesions also firmer. The adhesions increase in firmness in the order 1, 2, 3.

EXPERIMENT XCVII.—Double gastro-enterostomy by perforated decalcified bone plates. The communicating openings, one near the pyloric and the other near the cardiac extremity of the stomach, were made between the anterior surface of the stomach and the upper portion of the jejunum. In operation No. 1, near the pylorus, the intact serous surfaces were brought in contact, while in the second operation both the stomach and bowel were scarified. At the post-mortem it was found that the adhesions at both places were of sufficient firmness to prevent leakage under pressure. In No. 2, adhe-

sions firmer, and the inflammatory infiltration more marked than in No. 1. Plates in stomach much softened, but remain *in situ*. Openings lined throughout by mucous membrane, and sufficiently large to admit the index finger.

EXPERIMENT XCVIII.—Ileo-colostomy by lateral apposition and fixation by perforated approximation discs. Lower portion of ileum united with the ascending colon. No scarification; bowels lightly agglutinated throughout by a very thin layer of plastic lymph; adhesions, however, can be easily separated, and where this is done the peritoneal surface appears denuded of endothelial cells, and very vascular, with new vessels along the outer margin of the surface of approximation.

Time, forty-eight hours.

EXPERIMENT XCIX.—Double gastro-enterostomy. The communicating openings were made between the anterior surface of the stomach and the duodenum, and the posterior surface of the stomach and the upper portion of the jejunum. In the posterior operation, the intact serous surfaces were brought in contact, while in the anterior the peritoneal surfaces of the stomach and duodenum were scarified. In both operations, perforated decalcified bone plates were used. Adhesions between posterior surface of stomach and bowel uniform throughout, but easily broken down; the peritoneal surfaces injected and apparently deprived of their endothelial covering. The anterior operation has resulted in the formation of firm adhesions, the products of exudation and tissue proliferation being supplied with new vessels, the circumscribed plastic peritonitis being much more advanced than at the site of the posterior operation.

EXPERIMENT C.—Double ileo-colostomy by perforated approximation plates. The anastomosis between the lower portion of the ileum and the colon just above the cæcum was made without scarification, while in the second operation, about six inches higher up in the colon and ileum, both serous surfaces were freely scarified. Omentum adherent at point of operation. Plates swollen, softened and pliable, but still efficient in maintaining coaptation and fixation. Adhesions at both places quite firm, but more so in the upper portion where scarification had been done.

EXPERIMENT CI.—Ileo-colostomy by approximation discs. The ileum was divided a few inches above the ileo-cæcal region and both ends closed by invagination, and three stitches of the continued suture. An anastomosis was made between the proximal end and the ascending colon by lateral apposition. No scarification. Intestines agglutinated at point of operation, but the adhesions gave way when the bowel was forcibly distended under hydrant pressure.

CHEMICAL IRRITATION OF SEROUS SURFACES.

In these experiments, it was aimed to study the effect of chemical irritation of the peritoneum in the reparative process, after intestinal operations. Iodine has been used for a long time in producing plastic inflammation of serous surfaces, for the purpose of obliterating serous cavities, consequently this substance was used in the first experiments. To study the effects of the diffuse application of tincture of iodine and tincture of iron to the intact peritoneal cavity, the following experiments were made :—

INJECTION OF CHEMICAL IRRITANTS INTO THE PERITONEAL CAVITY.

EXPERIMENT CII.—Medium-sized dog. The needle of a hypodermic syringe was thoroughly disinfected, and a drachm of the tincture of iodine injected into the peritoneal cavity. Immediately after the injection the animal evinced great pain, which, however, appeared to subside after a short time, and subsequently no unfavorable symptoms were observed. Three days after the injection the urine was examined and showed the presence of iodine. Dog killed, nine days after the injection. Circumscribed plastic peritonitis over a space four inches square, corresponding to the point

where the puncture was made. At this place the omentum was much thickened, very vascular and adherent to the parietal peritoneum and the adjoining folds of the intestines.

EXPERIMENT CIII.—Medium-sized dog. A fluidrachm of the tincture of muriate of iron was thrown into the peritoneal cavity by means of a well-disinfected hypodermic syringe. The pain immediately after the injection was intense, and the animal appeared to be very ill two days after the injection, and died with well-marked symptoms of peritonitis on the sixth day. Diffuse plastic peritonitis was found to be the cause of death. The omentum was adherent everywhere, and the intestines were matted together by numerous adhesions. The abdominal cavity contained a considerable quantity of serous fluid.

Remarks.—Both experiments prove that when tincture of iodine and tincture of iron are brought in contact with the peritoneum a plastic inflammation ensues, and it was reasonable to expect that if either of these substances could be applied to the serous surfaces which it was intended to unite, the reparative process would be hastened.

EXPERIMENT CIV.—Triple ileo-ileostomy by perforated decalcified bone plates. Three internal fistulæ were made between two adjacent loops of the ileum, about six inches apart. In operation No. 1, approximation of intact serous surfaces; in operation No. 2, the serous surfaces were painted with tincture of iron over an area corresponding to the size of the plates; in operation No. 3, the serous surfaces over the same extent were brushed with pure tincture of iron. The animal was killed forty-eight hours after operation, and the following conditions were noted: No general peritonitis. All the plates firmly in place coaptating the serous surfaces accurately, the swelling of the tunics of the bowel only serving to enhance their efficiency. At No. 1, adhesions quite firm, flexion of bowel and marked injection of serous surfaces. At No. 2, no adhesions between serous surfaces. The peritoneal surfaces to which the tincture of iron had been applied appeared stained, almost black, and at some points the serous coat was destroyed. At No. 3, peritoneal surfaces stained dark brown; adhesions firm, and an abundance of plastic lymph, even beyond the margins of the plates.

EXPERIMENT CV.—Double ileo-ileostomy by approximation plates and omental grafting. Operation No. 1, approximation of ileum to ileum by perforated decalcified bone plates, serous surfaces intact. Operation No. 2, similar operation six inches higher up, uniting the same loops, but painting the serous surfaces with pure tincture of iodine. Operation 3. Cut off a piece of omentum two inches wide and sufficiently long to encircle the entire bowel. After scarifying the bowel, and the omental graft on one side, the scarified surfaces were brought in contact, and the graft fixed in its place by two fine catgut sutures passed through the mesentery and both ends of the graft. Animal killed, forty-eight hours after operation. All plates firmly in place. At No. 1, adhesions firm. At No. 2, dark-brown discoloration of surface to which the iodine had been applied, agglutination over the whole surface. Under hydrostatic pressure the adhesions first gave way between the two plates where the iodine had been applied, showing conclusively that chemical irritation of serous surfaces does not hasten the adhesive process, while it may, and probably does, expedite the definitive healing. At No. 3, omental graft firmly adherent to the entire circumference of the bowel and beginning vascularization of the graft around its margins.

Remarks.—In all of these experiments the post-mortem examinations showed no evidences of diffuse peritonitis. In most of the cases the inflammatory process was limited to the portion of the bowel interposed between the plates. Without exception the adhesions formed were firmest, and the definitive healing was initiated first where scarification was performed; results which clearly demonstrate the fact that the reparative process between serous surfaces which it is intended to unite is hastened by traumatic irritation. Traumatic irritation, by scarification of the peritoneal surface

with the point of an aseptic needle, is the most potent means to provoke a circumscribed plastic peritonitis, and is followed within a few hours by a copious exudation of plastic lymph, which, like a cement substance, mechanically agglutinates the coaptated serous surfaces. The same measure, by destroying the continuity of the non-vascular layer of the peritoneum, brings at once in contact the vascular network of both sides of the bowel, and opens up a direct route for the new vessels, an important element in the rapid healing of the visceral wounds. Chemical irritants, by destroying the endothelial layer of the peritoneum, rather retard than favor early adhesion and union between the coaptated bowels, and should, therefore, not be resorted to in intestinal surgery with a view to hasten the reparative process.

VII. OMENTAL GRAFTING.

Under the head of circular enterorrhaphy, mention is made of transplantation of omental flaps after uniting the two ends of the bowel by suturing or invagination, with a view of securing an additional safeguard against perforation during the process of repair. A number of experiments are described where the procedure was practiced with satisfactory results. After a few days the omental flaps were found firmly adherent and vascular around the whole circumference of the bowel, constituting a ring of living tissue outside the line of suturing. In all these cases the proximal end of the flap remained in connection with the omentum, and care was taken to cut the flap in such a manner that some vessel of considerable size should furnish the necessary vascular supply. I was well aware that plausible objections could be entered against this method, in that the connecting bridge between the bowel and the omentum might become, subsequently, a cause of intestinal obstruction by making traction upon the bowel, thus causing a flexion, or, by becoming a band of constriction for some loop of intestine. For the purpose of obviating such remote consequences, I resorted to another procedure which I have designated as omental grafting. I was familiar with the fact that implantations of aseptic substances into the peritoneal cavity had frequently been done without any immediate or remote ill effects, and I had every reason to expect that a large, completely detached aseptic omental graft, in an aseptic abdominal cavity, would be well tolerated and would soon become adherent to the subjacent peritoneal surface, and thus afford an additional safeguard against perforation and the disastrous consecutive result, perforative peritonitis, during the time required for the healing of the intestinal wound. In the following experiments the grafts used were from one and a half to two inches in width and of sufficient length to completely encircle the bowel. The free ends were made to project a few lines beyond the mesenteric attachment, and were fixed by two fine catgut sutures, each of which embraced two corresponding angles of the graft and the mesentery. The stitches were made in the direction of the mesenteric vessels, so that in tying, no vessel should be included in the suture. In these experiments dogs were used exclusively.

EXPERIMENT CVI.—Three pieces of omentum two inches wide and sufficiently long to encircle the bowel were completely detached and grafted as follows :—

1. Graft simply laid over the bowel corresponding to the lower portion of the ileum and fastened in its place on the mesenteric side by two fine catgut sutures.
2. Serous surface of bowel about six inches higher up scarified, and graft applied to this surface and fixed in the same manner.
3. About six inches still higher up, bowel treated in the same way, and one of the serous surfaces of the graft also freely scarified.

The graft was scarified on the side which was to be brought in contact with the bowel. Fixation of graft by two catgut sutures on mesenteric side. Animal killed thirty-six hours after operation. All the grafts adherent, slightly contracting the

bowel at the three different places. On separating the adhesions, the subjacent serous surface very vascular and denuded of its endothelial layer. Firmness of adhesions increases in proportion to the extent of scarification done, being least firm at No. 1, firmer at No. 2, and firmest at No. 3, where both coaptated serous surfaces had been scarified. At Nos. 2 and 3 the plastic lymph was freely supplied with new blood vessels. The vascularization was most conspicuous on the mesenteric side.

EXPERIMENT CVII.—Two omental grafts planted around the ileum in the same manner as described above. At No. 1, both the bowel and the inner side of the graft were scarified; at No. 2, only the serous surface of the bowel. Animal killed forty-eight hours after operation. Stump of omentum adherent to abdominal wound and intestines. No peritonitis. At No. 1, graft firmly adherent over the entire extent. A slight extravasation of blood between the graft and the bowel. Beginning vascularization of interposed plastic lymph. At No. 2, also firm adhesions and beginning vascularization of the plastic exudation. Both of the grafts appear to be stained with the coloring material of the blood.

EXPERIMENT CVIII.—Planting of two omental grafts around the ileum, about eight inches apart. At No. 1 both the bowel and one side of the omental graft were scarified. At No. 2 only the serous surface of the bowel was treated in this manner. Animal killed six days after the operation. Both grafts firmly adherent throughout and freely supplied with blood vessels, the largest of the new vessels being seen on the mesenteric side. The omental stump adherent to the portion of bowel between the grafts where a flexion has been made from this cause.

EXPERIMENT CIX.—In this experiment omental grafting was done at two points around the lower portion of the ileum. At one point the serous surfaces were left intact, at the other both the peritoneal surface of the bowel and the omental graft were freely scarified. Animal remained perfectly well and was killed eight days after operation. No signs of peritonitis. Both grafts formed a thin vascular layer around the entire circumference of the bowel and firmly and evenly united throughout. Vascularization was more marked where scarification had been done. On attempting to separate the grafts it was difficult to find and define the line of union between the omentum and the underlying bowel, as the union was very intimate and firm.

Remarks.—In all of these experiments the grafts retained their vitality, and in a few hours became firmly adherent to the intestinal surface with which they had been brought in contact. Scarification of the serous surface has also been found in these experiments, an exceedingly valuable measure in hastening the process of adhesion, granulation and vascularization. By planting grafts side by side, with and without scarification, I was enabled to determine with accuracy the beneficial influence exerted by scarification in favoring the reparative process, and, without a single exception, observed that where scarification was done the adhesions were firmer and vascularization more advanced. The post-mortem examinations appeared to demonstrate that the firmness of the adhesions and the degree of vascularization were in direct proportion to the extent of traumatic irritation of the peritoneum, being always most marked in cases where both the bowel and the under surface of the graft were scarified, and least where intact peritoneal surfaces were brought into apposition. As soon as the omental grafts were cut off from the omentum they were placed in a 1–2000 solution of corrosive sublimate, kept at the temperature of the body, in order to secure for the graft a perfectly aseptic condition, until everything was in readiness for the transfer of the graft to its new location. Before planting the graft it was carefully dried by pressing it between gauze or sponges wrung out of the same solution. The scarifications of the serous surfaces should only be made sufficiently deep to give rise to a very slight oozing, as when hemorrhage is more profuse there is danger of the formation of a clot between the graft and the bowel, which, if it does not ultimately prevent union

between the coaptated surfaces, must necessarily interfere with the formation of early and firm adhesions. Omental grafting cannot fail in becoming an established procedure in many abdominal operations. After suturing a large wound of the stomach or intestines, a strip of omentum should be laid over the wound and fastened in its place by a few catgut sutures. After circular enterorrhaphy the operation should be finished by covering the circular wound by an omental graft about two inches wide, which should be fixed in its place by two catgut sutures passed through both ends of the graft and the mesentery. Omental grafting should also be resorted to in repairing peritoneal defects in visceral injuries of the abdominal organs, and in covering large stumps after ovariotomy or hysterectomy, where the pedicle is treated by the intra-abdominal method. In conclusion, I beg leave to submit the following propositions for your further discussion :—

1. Traumatic stenosis from partial enterectomy and longitudinal suturing of the wound becomes a source of danger from obstruction or perforation in all cases where the lumen of the bowel is reduced more than one-half in size.

2. Longitudinal suturing of wounds on the mesenteric side of the intestine should never be practiced, as such a procedure is invariably followed by gangrene and perforation by intercepting the vascular supply to the portion of bowel which corresponds to the mesenteric defect.

3. The immediate cause of gangrene in circular constriction of a loop of intestine is due to obstruction of the venous circulation, and takes place first, in the majority of cases, at a point most remote from the cause of the obstruction.

4. On the convex surface of the bowel a defect an inch in width, from injury or operation, can be closed by transverse suturing without causing obstruction by flexion. In such cases the stenosis is subsequently corrected by a compensating bulging or dilatation of the mesenteric side of the bowel.

5. Closing a wound of such dimensions on the mesenteric side of the bowel by transverse suturing may give rise to intestinal obstruction by flexion, and to gangrene and perforation by seriously impairing the arterial supply to, and venous return from, the portion of bowel corresponding with the mesenteric defect.

6 Flexion, caused by inflammatory and other extrinsic causes, gives rise to intestinal obstruction only in case the functional capacity of the flexed portion of the bowel has been impaired or suspended by the causes which have produced the flexion, or by subsequent pathological conditions which have occurred independently of the flexion.

7. As in flexion, a volvulus gives rise to symptoms of obstruction when the causes which have given rise to a rotation upon its axis of a loop of bowel have, at the same time, produced an impairment or suspension of peristalsis in the portion of bowel which constitutes the volvulus, or when a diminution or suspension of peristalsis follows in consequence of the degree or extent of the rotation.

8. Accumulation of intestinal contents above the seat of invagination is one of the most important factors which prevents spontaneous reduction, and which determines gangrene of the intussusceptum and perforation of the bowel.

9. Spontaneous disinvagination is not more frequent in ascending than descending invagination.

10. The immediate or direct cause of gangrene of the intussusceptum is obstruction to the return of venous blood by constriction at the neck of the intussuscipiens.

11. Ileo-cæcal invagination, when recent, can frequently be reduced by distention of the colon and rectum with water, but this method of reduction must be practiced with the greatest caution and gentleness, as over-distention of the colon and rectum is productive of multiple longitudinal lacerations of the peritoneal coat—an accident which is followed by the gravest consequences.

12. The competency of the ileo-cæcal valve can only be overcome by over-distention of the cæcum, and is effected by a mechanical separation of the margins of the valve, consequently it is imprudent to attempt the treatment of intestinal obstruction beyond the ileo-cæcal region by injections per rectum.

13. Resection of more than six feet of the small intestine in dogs is uniformly fatal; the cause of death in such cases is always attributable to the immediate effects of the trauma.

14. Resection of more than four feet of the small intestine in dogs is incompatible with normal digestion, absorption and nutrition, and often results in death from marasmus.

15. In cases of extensive intestinal resection, the remaining portion of the intestinal tract undergoes compensatory hypertrophy, which, microscopically, is apparent by thickening of the intestinal coats and increased vascularization.

16. Physiological exclusion of an extensive portion of the intestinal tract does not impair digestion, absorption and nutrition as seriously as the removal of a similar portion by resection.

17. Fecal accumulation does not take place in the excluded portion of the intestinal canal.

18. The excluded portion of the bowel undergoes progressive atrophy.

19. A modification of Jobert's invagination suture—by lining the intussusceptum with a thin, flexible rubber ring, and the substitution of catgut for silk sutures—is preferable to circular enterorrhaphy by the Czerny-Lembert suture.

20. The line of suturing or neck of intussuscipiens should be covered by a flap or graft of omentum in all cases of circular resection, as this procedure furnishes an additional protection against perforation.

21. In circular enterorrhaphy the continuity of the peritoneal surface of the ends of the bowel to be united should be procured, where the mesentery is detached, by uniting the peritoneum with a fine catgut suture before the bowel is sutured, as this modification of the ordinary method furnishes a better security against perforation on the mesenteric side.

22. In cases of complete division of an intestine, if it is deemed advisable not to resort to circular enterorrhaphy, one or both ends of the bowel should be closed by invagination to the depth of an inch, and three stitches of the continued suture embracing only the peritoneal and muscular coats.

23. The formation of a fistulous communication between the bowel above and below the seat of the obstruction should take the place of resection and circular enterorrhaphy in all cases where it is impossible or impracticable to remove the cause of obstruction, or where, after excision, it would be impossible to restore the continuity of the intestinal canal by suturing, or where the pathological conditions which gave rise to the obstruction do not constitute an intrinsic source of danger.

24. The formation of an artificial anus in the treatment of intestinal obstruction should only be practiced in cases where continuity of the intestinal canal cannot be restored by making an intestinal anastomosis.

25. Gastro-enterostomy, jejuno-ileostomy and ileo-ileostomy should always be made by lateral apposition with partially or completely decalcified perforated bone plates.

26. In making an intestinal anastomosis for obstructions in the cæcum or colon, the communication above and below the seat of obstruction can be established by lateral apposition with perforated approximation plates, or by lateral implantation of the ileum into the colon or rectum.

27. An ileo-colostomy or ileo-rectostomy, by approximation with decalcified perforated bone plates or by lateral implantation, should be done in all cases of irreducible

ileo-cæcal invagination where the local signs do not indicate the existence of gangrene or impending perforation.

28. In all cases of impending gangrene or perforation the invaginated portion should be excised, both ends of the bowel permanently closed, and the continuity of the intestinal canal restored by making an ileo-colostomy or ileo-rectostomy.

29. The restoration of the continuity of the intestinal canal by perforated approximation plates, or by lateral implantation, should be resorted to in all cases where circular enterorrhaphy is impossible on account of the difference in size of the lumina of the two ends of the bowel.

30. In cases of multiple gunshot wounds of the intestines involving the lateral or convex side of the bowel, the formation of intestinal anastomosis by perforated decalcified bone plates should be preferred to suturing, as this procedure is equally, if not more, safe, and requires less time.

31. Definitive healing of an intestinal wound is only initiated after the formation of a network of new vessels in the product of tissue proliferation from the approximated serous surfaces.

32. Under favorable circumstances quite firm adhesions are formed between the peritoneal surfaces within six to twelve hours, which effectually resist the pressure from within outward.

33. Scarification of the peritoneum at the seat of coaptation hastens the formation of adhesions and the definitive healing of the intestinal wound.

34. Omental grafts, from one to two inches in width, and sufficiently long to completely encircle the bowel, retain their vitality, become firmly adherent in from twelve to eighteen hours, and are freely supplied with blood vessels in from eighteen to forty-eight hours.

35. Omental transplantation, or omental grafting, should be done in every circular resection, or suturing of large wounds of the stomach or intestines, as this procedure favors healing of the visceral wound and affords an additional protection against perforation.

Dr. John Homans, of Boston, read a paper entitled—

THREE HUNDRED AND EIGHTY-FOUR LAPAROTOMIES FOR VARIOUS DISEASES, WITH TABLES SHOWING THE RESULTS OF THE OPERATIONS AND THE SUBSEQUENT HISTORIES OF THE PATIENTS.

TROIS CENT QUATRE-VINGT-QUATRE LAPAROTOMIES POUR DIVERSES MALADIES AVEC LES TABLES SYNOPTIQUES DES OPERATIONS ET LES BIOGRAPHIES SUBSÉQUENTES DES MALADIES.

DREI HUNDERT UND VIERUNDACHTZIG LAPAROTOMIEN FÜR VERSCHIEDENE KRANKHEITEN, NEBST TABELLEN ÜBER IHRE RESULTATE UND DIE SPÄTERE GESCHICHTE DER PATIENTEN.

GENERAL METHOD PURSUED IN PREPARING FOR LAPAROTOMY—SOME STATISTICAL ACCOUNT OF RESULTS.

The laparotomies enumerated in the accompanying tables have been carefully tabulated, and the preparation of these tables has required much correspondence and hard work. I have looked through the tables and have picked out cases, here and there, which seemed to me to be of special interest, and where the usefulness of a rather minute description of the case and its treatment seemed to require it, I have given a full narration.

In what I have to say I will confine myself wholly to my own experience, without theorizing or quoting authorities. I do not do this in a narrow, egotistical way, but because all of you have read and heard all that I have read or heard, and need not be bored by hearing the statements and theories of others at second hand.

384 Laparotomies. Of these, ovariotomies number 282. Removal of uterine tumors, 27. Simple exploratory laparotomies, 19. Laparotomies and stitching of ovarian cysts to the skin, 15. Removal of uterine appendages for fibro-myoma, 5. Removal of uterine appendages for nervous disorders, 5. Pyosalpinx, 1. Tubo-ovarian, 1. Abdominal abscess, 1. Removal of immense lipomas, 2. Intestinal obstruction, 4. Renal tumor, 3. Perityphlitic abscess, 1.

I have always regarded Sir Spencer Wells' first volume of Cases of Ovariotomy, published about 1865, as the most valuable book for a beginner to study.

In a very humble way, perhaps, this paper and these tables may be of interest to the student and practitioner of abdominal surgery. I have been rather surprised to find that thirty women out of over three hundred, or nearly ten per cent., have ventral hernia.

The general method I have pursued in preparing for a laparotomy is the following:—

I have a sufficient number (say six or eight) earthenware jars, such as we use in New England for holding pickles, each of which will contain six gallons of fluid ; two or more of them are filled with a solution of corrosive sublimate 1–1000 ; new sponges are cleansed of their sand and are put to soak in the bichloride solution, and left there two or more days ; they are then wrung out dry in a wringing machine such as is used in a laundry. The rubber rollers of the wringing machine will dry a sponge almost completely, and the compression squeezes out all the dirt that may remain, and also all the bichloride ; this could not be done as thoroughly with the hands, nor does one wish to keep putting his hands into a mercurial solution. After these sponges have been soaked in this mercuric solution, they are cast into a jar containing a solution of carbolic acid 1–20, and when wanted are taken out of the jar and wrung out again in the wringer and taken in a clean bag to the operation. I have always used carbolic-acid spray, and continue to do so ; though I think it unnecessary, yet I hate to give it up. I use an electric light when necessary.

Of my first five unantiseptic ovariotomies all died. Of my antiseptic ovariotomies 248 have recovered and 34 have died. About one-quarter, probably, of all the fatal cases are to be attributed to some error or carelessness of mine, to some want of cleanliness, or, perhaps, to a slightly suppurating hang-nail or other sore on my hand, or to something that might have been avoided. Perhaps this comes from too much operating within a given time. Deaths for which I am inclined to think I am at fault have occurred generally toward the end of many daily ovariotomies, when I may have been tired or possibly unclean. To balance these fatal cases, of course, many unexpected recoveries have occurred. There is also an element which cannot be estimated beforehand, viz., the viability of the patient (if I may say so); just as it will take many blows to kill one man, any one of which would have killed another less viable man, so, a moderately severe operation will be fatal in a certain case, and a much more severe one will be innocuous in another case. I mean where both patients seem to be equally healthy. This vitality or viability it is impossible to estimate beforehand. There are other causes of death which are unpreventable. Such a one is the case of death from acute mania, No. 63; another one the death from tetanus ; another the death, sixteen days after the operation, when recovery seemed complete, from thrombus in all the pulmonary arteries, No. 275. Let me illustrate this point. No. 117, a simple, uncomplicated case, died, while cases No. 72, where the bladder was cut open, and No. 260, in which a second operation, including removal of the uterus, was done, and No. 50, complicated with heart disease, curvature of the spine and the removal of

not only ovarian but uterine tumors, have recovered. The causes of death have usually been peritonitis and septicæmia. You may call it septic peritonitis or septicæmia, or blood poisoning, but it is fatal, and I am rather skeptical about deaths from intestinal obstruction of a mechanical nature after laparotomy, except as the intestines are paralyzed by peritonitis. There is often a sort of atony of the bowels which is almost equivalent to mechanical obstruction, and which gives rise to great distention and to vomiting, but there is no real strangulation such as you see in hernia, or at least I have never seen such a case.

Two cases in which I wounded the bladder during ovariotomy recovered, and they are both living in good health, two and six years respectively after the operation. In both cases the bladder was sewn up with silk, and in both cases the sutures were left shut up within the abdominal cavity. Of those who recovered, nine have since died of abdominal cancer, a few months or years after recovery, and thirty have ventral hernia. I have heard of fifteen children born to eleven women out of about two hundred heard from. The sexes do not correspond to the ovaries. The patients' ages have varied from twelve to seventy-three years. In size, the weight of the sac and its contents has varied from a pound to one hundred and eleven and a half pounds. Twisted pedicle occurred, I think, six times.

The usual length of my incision is about two inches, except in fat people, or where some difficulty in the operation requires more room. I never leave a clamp on the ovarian pedicle, but always tie and burn the stump and drop it back. I have always used silk sutures, and am careful to include all the abdominal parietes in the suture, particularly the transversalis fascia.

Drainage was used in fifteen cases of ovariotomy, I think, and I have gradually reduced the size of my drainage tubes. In three cases silk sutures have been discharged at long intervals after recovery. I have had one case of tetanus following a simple uncomplicated ovariotomy. Death occurred on the sixth day, the symptoms existing for twenty-four hours before death.

I have had one case of the formation of a stone in the bladder around the dermoid contents of the tumor discharged into the bladder.

Most of my cases have been treated in a private hospital, which is simply an ordinary dwelling house. During convalescence, the patients have found the lifting machine figured in Hamilton's Surgery as Dr. Jenks' Fracture Bed very useful, portable, light and strong. I have found none of the fracture beds, such as those of Crosby and others, of any special service. I use a catheter as little as possible, preferring to have the patients pass their water voluntarily from the very first, if possible.

My greatest number of consecutive recoveries after ovariotomy has been thirty-eight, I think.

Suppurating cysts of the ovary are spoken of and described not unfrequently. I never saw but one, and that had been tapped. I doubt if an ovarian cyst ever suppurates unless there has been at some time a communication with the external air, or with some mucous cavity. But there is a fluid containing fat and sebaceous matter, that to the naked eye looks exactly like laudable pus, and can only be distinguished from it by microscopical examination.

The removal of sessile tumors is accomplished by a sort of knack learned by experience; at least it has been by me. You find the cyst covered with peritoneum and immovable, but if you cut through the outer adherent covering of peritoneum at several points, you will usually strike the familiar-looking wall of the ovarian cyst somewhere, and may succeed in enucleating it and making a sort of pedicle.

I recall two cases of swelling of the parotid gland after ovariotomy. Both recovered quickly, and I have not regarded an enlargement of the parotid during convalescence

as of special importance. I may add that cases of parotitis seen in consultation have recovered, and I have generally given a favorable prognosis.

In regard to hysterectomy, my success has not been great. My cases of removal of uterine fibroid tumors number twenty-seven, with seventeen recoveries and ten deaths. I perform the operation much better than I did years ago, and my later cases have nearly all recovered, but still I am not fond of it and always rather shrink from it. I never do it unless the patient seems in danger of her life from hemorrhage, mechanical pressure, or exhaustion, or else suffers such pain that life is not worth living. I do not know which is the better way of managing the stump, whether intra- or extra-peritoneally. I am pretty sure, however, that with me the extra-peritoneal is safer, although I cannot say that a more skillful operator might not make the intra-peritoneal method safe and reliable.

Encouraged by the recovery of a patient after hysterectomy for fibro-myoma in which the stump had been left in the abdomen, I treated the next case intra-peritoneally, with fatal result. The autopsy showed that the stump was the cause of a quite limited peritonitis, and that if it had been left outside the patient would apparently have been relieved of the source of blood poisoning, and might have recovered. I use a wire écraseur, either Kœberle's or a longer one, for I find that for the compression of the pedicle Kœberle's screw is not long enough, and one must have several of Kœberle's serre-nœuds or else an instrument with a longer screw, and the latter seems to me simpler. I have used drainage several times after removal of fibroid tumors, but it is usually unnecessary. When the clamp and pin come away, there is usually some fluid in the cavity where the stump was, and this has often a very offensive odor like that of a sewer, but it seems of no consequence, as the patient's temperature and pulse are nearly normal. I suppose there is a septic putrescence and a non-septic putrescence; but at the first glance one would suppose that an ounce or so of black, offensive fluid between the intestines and the bladder, and within the peritoneal cavity, would necessarily set up septicæmia or peritonitis.

The smell that I mean is very much like that from macerating bones, and yet there is no septicæmia and the patient recovers, the hole closing rapidly. I am satisfied, on the whole, with the extra-peritoneal treatment of the pedicle by means of a screw serre-nœud, though the intra-peritoneal method is neater. The wire seeks a bed for itself in the part of the tumor where the diameter is smallest, and it is almost impossible to keep it from gravitating to this point. It must be kept off the bladder, of course. I had a recovery, however, in one case where two successive wires broke on being tightened, and in which I found, twenty-four hours afterward, that a piece of the bladder was in the serre-nœud. The opening in the bladder subsequently closed without suture by keeping a catheter in the urethra; so I know that when one is unfortunate enough to have taken a piece out of the apex of the bladder, at least an inch in diameter, he may expect recovery if he will keep a Sims' catheter in the bladder and urethra *all the time*. My patient's fistula closed in six weeks.

Of cases of ovarian tumor, uncomplicated except by adhesion, in which I could not, or thought I could not (for I can get a cyst out now that I could not have removed five or six years ago), remove the cysts, and which I have stitched to the abdominal walls or fastened outside, I have had nine. Of these all have completely recovered. In these cases, the cysts had grown under the peritoneum, and were adherent to the intestines, to the broad ligament or uterus, and in one instance the wall of the cyst and its peritoneal adhesions in the right iliac region were over two inches thick and very dense, and yet the patient recovered, and is perfectly well and hearty to-day. If in these cases you can strike the cyst wall without too much hemorrhage or violence, you can generally remove the cyst. Of cases where I have opened the abdomen and tapped or more or less emptied the cyst, I have had several, nearly all of which died.

These partial operations are generally bad surgery, and a man does them less and less the more he operates. Of cases of collections of pus in the abdominal cavity I have had but one, and that was treated successfully by laparotomy and drainage. Of cases of abscess in the ovary and salpingitis of gonorrhœal origin, I have had one, successfully treated by removal of both tubes and one ovary, the other ovary being so imbedded in the pelvic tissues as to be immovable. The patient recovered. Of removal of large intra-abdominal fatty tumors, sub-peritoneal, I have had two. They each weighed over fifty pounds and were many-lobed. One occurred in a man and the other in a woman, and both were fatal.

In connection with the subject of intestinal obstruction it may not be improper for me to say that I have had one case of an operation for the closure of a Meckel's diverticulum (omphalo-mesenteric remains). In this case, the mucous membrane projecting at the umbilicus was pulled up and cut off, and the skin within the umbilical cicatrix, over a diameter of about a quarter of an inch, was removed, and the denuded surfaces united by silver sutures. The patient was an infant about five months old. The cure was complete. Another case, interesting in connection with this, was a case of laparotomy with intestinal obstruction, caused by Meckel's diverticulum in a young man of twenty-one years. I failed to find the obstructing band, owing to my ignorance at the time of the causation of intestinal obstruction by the diverticulum, and the case resulted fatally. It is described at length by Prof. Fitz, in the *American Journal of the Medical Sciences*, for July, 1884. Once during ovariotomy I removed a portion of a cancerous omentum, and the patient has experienced up to the present time, one and a half years from the date of the operation, no trouble, and has been in perfect health.

I have had one very remarkable case of cure of tubercular peritonitis and dropsy by laparotomy, the patient being now fat and healthy, three years after the operation. Fluid was discharged from the wound for about eight months, and there is now a discharge of about a half a drachm a day from a short sinus.

Of removals of the kidney for sarcoma, cancer or abscess, depending on calculous nephritis, I have had three cases, all of which were fatal. The operations seemingly went off very well, but no urine excepting a few ounces was secreted up to the time of death, about two days in each case. Almost complete suppression had taken place in these cases.

Of the formation of an artificial anus, for cancer, either by lumbar colotomy or anteriorly in the pubic region, I have had five cases, three of which were successful; the life of the patients being rendered comfortable for many months.

I have had many cases of operation for strangulated hernia, which I suppose ought hardly to be mentioned in a paper on laparotomy.

I have successfully removed a fibroid tumor in the abdominal fascia and peritoneum of the right lumbar region, the size of a small placenta, by laparotomy. The patient is now in good health four years after operation. As some of the peritoneum was removed, and its edges could not be brought together, there has been a troublesome rupture. This is the only case of fibroma of the peritoneum I have ever seen.

ILLUSTRATIVE CASES.

CASE IX.—Remarkable as having no pedicle. It was a cyst of the broad ligament, and as the woman coughed, after the peritoneum was opened and the cyst tapped, the cyst was expelled and dropped on the floor without a vessel being tied or any force, except the gentlest assistance, being used. The operation, from the first incision till all the sutures had been tied, was ten minutes. The cyst measured thirty-six inches in circumference.

CASE X.—Acute peritonitis at time of operation, September 29th, 1878. Confined Nov. 16th, 1879, after a normal labor. Child a girl. Ovary remaining the right.

CASES XII and XIII.—Both died subsequently to recovery, of abdominal cancer, and one also with thoracic cancer. Their deaths occurred, one in six months, and one in five months after recovery.

CASE XIX.—Is only of interest as having menstruated for two years irregularly, after the removal of both ovaries.

CASE XXIX.—Cyst of left broad ligament, died of cancer of stomach within a year.

CASE XXXI.—Was confined in October, 1881, fourteen months after ovariotomy. Child a girl. Ovary remaining the right.

CASE XXXIII.—Is remarkable as having had forty ounces of serum removed by aspiration from the left thoracic cavity, on account of acute pleurisy, on the fourth day after ovariotomy.

CASE XXXV.—Died of abdominal cancer nine months after recovery.

CASE L.—Is remarkable on account of her recovery. She had severe cardiac disease, great deformity from curvature of the spine, and at the operation three tumors were removed, a solid tumor of the right ovary, a dermoid tumor of the same, and a fibroid tumor of the uterus. She died three years later, of heart disease.

CASE LVI.—Is remarkable as requiring colotomy, or rectostomy, for cancer of the bowel, Dec. 21st, 1881, seven months after recovery. The artificial anus was made in the pubic region. Her life was very comfortable for many months. She died of abdominal cancer, in November, 1882.

CASE LXI.—Died several months after recovery, from cancer of the stomach.

CASE LXVII.—Is exceptional as having died of acute mania, on the eighth day after ovariotomy. A careful autopsy found everything going on well in the peritoneal cavity, and no recognizable cause for death. I think the case might be disregarded as one of fatal ovariotomy, but I have thought it right to put it in the table as such.

CASE LXXII.—Is an instance of a most fortunate uninterrupted recovery after an incision of the bladder. The convalescence was extraordinarily devoid of fever, the temperature never rising above 99°. A Sims' catheter was kept in the urethra for nine days. The bladder wound was sewed up with a continuous suture of carbolized silk, and was closed tight at the time of the operation. The patient is now, six years after the operation, in perfect health, and has never had a symptom of vesical or other trouble.

CASE LXXVI.—Is remarkable on account of the age of the patient at the time of the operation, and her uninterrupted good health since. She is now hale and hearty at the age of 79.

CASE LXXVII.—In this case a recurrence of the disease, gelatinous cancer, took place, and more or less fluid was discharged after recovery, through the abdominal cicatrix and through the bladder. The tumor was a burst gelatinous dermoid cyst, and weighed 35 pounds. This material was scooped out and sponged out as thoroughly as possible. The patient was much relieved by the operation, and gained flesh and strength. Two years after her recovery I removed a calculus from the bladder, the nucleus being a hair which had probably remained in the peritoneal cavity after the operation, and had found its way into the bladder. She died in 1885, four years after ovariotomy, of general abdominal cancer.

CASE LXXX.—Is a case of the most rapid recurrence of cancer after ovariotomy that has occurred among my cases. There were masses of cancerous-looking nodules in the abdominal parietes, which were cut through in making the incision. The tumor was sarcomatous-looking, and more solid than cystic. It weighed 26 pounds. The operation having been done on March 14th, 1882, the patient died on May 19th, with large, rapidly-growing, soft, sarcomatous tumors in the abdomen. Another remarkable occurrence in the case, and one that will hardly be credited, was the occurrence of two well-formed vaccination vesicles, which appeared spontaneously on the cicatrices of

two successful vaccinations done eight years before. These vesicles ran a normal course. The late Dr. Henry A. Martin was kind enough to confirm my diagnosis, and told me that the occurrence of similar spontaneous vaccine disease had once or twice been recorded. There was no possibility of contagion. The patient was ill, confined to her bed, and had no visitors, and no one in the house had been vaccinated recently. I think the element of contagious inoculation is entirely excluded, and the case may be received as one of spontaneous vaccine disease, appearing in an old cicatrix, and caused in some unexplained way by the patient's condition. It is perhaps unnecessary for me to say that the observation is unique in my experience, and it will be interesting to know if others have observed any similar phenomenon after ovariotomy.

CASE LXXXI.—Recovered rapidly in 1882, and after enjoying four years of health, noticed that she was growing larger. She put herself under my care in 1887, and I made an exploratory incision to remove an adherent tumor of the other (the left) ovary, but I could do nothing, and she died in a few days, of peritonitis.

CASE LXXXII.—Was one of the largest tumors I have removed, the solid and fluid contents removed a few days before, and at the operation weighing 105 pounds. The patient is now, five years later, in robust health.

CASE LXXXIV.—Although the posterior surface and right side of uterus was "peeled" and denuded of its peritoneal covering, and tied and sewn, and burnt, to control hemorrhage, pregnancy has taken place twice, once in 1883 and once in 1885. The first child was a female, and the second a male. The ovary remaining was the right.

CASE LXXXIX.—This lady had a very rapid recovery following ovariotomy, in 1882, the temperature only once rising above the normal. In 1884 she sent for me and I found her very feeble, emaciated, with a large, hard, nodulated tumor filling the pelvis and lower abdominal region, and projecting into and through the cicatrix of the abdominal incision; the inguinal glands were also affected, and there had been hemorrhage from the surface of the tumor. Vomiting was almost constant. Nothing could be done in the way of an operation, and she died in June.

CASE CII.—Was one of suppurating cyst in a woman 37 years old. She had been tapped three weeks before the operation. Her temperature was high, 101° before operation, and rapidly fell to normal. Her recovery was rapid.

CASE CIV.—Was one accompanied by hemorrhage during the operation from two arteries near the aorta in the left lumbar region. Her convalescence was accompanied by high temperature, rising to 104° on the 7th day, and by a severe and constant diarrhœa and more or less dysentery; but she left for home on the 25th day, and has since been remarkably well.

CASE CVII.—One of dermoid tumor, followed by death from cancer, three months after the operation.

CASE CXVIII.—Was one of normal recovery after ovariotomy in a woman 68 years old. This was followed by a return of the disease in the other ovary and a large ventral hernia. In 1887 a tumor of the left ovary, together with the adherent uterus, was removed; the hernia was cured and the patient, now *seventy-three* years old, went home well.

CASE CXXXVIII.—Was confined in August, 1886, three years after ovariotomy; child a female; labor easy and rapid; left ovary remaining.

CASE CXLII.—Recovered after removal of a large tumor of the right ovary in 1883. She has been confined twice since; once in 1884 and once in 1885; both children were females. The left ovary remained.

CASE CXLIV.—Is remarkable for two circumstances. The discharge of a ligature of silk from the wound two years after recovery, and a desire for sexual intercourse since ovariotomy, whereas before the removal of the ovary she had no desire, and, in fact,

disgust. The silk is coarse and strong and is absolutely unchanged, the knot being as perfect as when tied, and the ends and edges as sharply defined as when cut off by the scissors two years before.

CASE CLXIV.—Is one of the cases that died subsequently of abdominal cancer two years after operation.

CASE CLXVII.—Is not properly a death after ovariotomy, as cancer of the omentum, bowels, ovary and bladder was present, and a cancerous mass was removed from the bladder and the bladder opened; but the ovary was removed, and so I have called it a death from ovariotomy.

CASE CLXXII.—One of multilocular cyst of the left ovary. Was confined June 5th, 1886, nineteen months after ovariotomy. Child a male. The right ovary remaining.

CASE CXCIV.—Was a very severe one. The cyst had grown into the broad ligament and had no pedicle. It was forcibly and violently enucleated, and there was much hemorrhage. The pedicle, such as it was, was a part of the uterus, and many ligatures were applied. A drainage tube was used. I expected a fatal result, but the patient reacted well and has since enjoyed good health, with the exception that a sinus has remained in the course of the wound. In June, 1885, fourteen months after the operation, the first suture of silk was discharged; and subsequently five more ligatures have been discharged, the knots being firm and the silk sound. These ligatures were discharged in January, May, October, and December, 1886, and in January, 1887.

CASE CXCV.—Was a successful hysterectomy as well as an ovariotomy.

CASE CXCVI.—Was my first fibroid tumor of the ovary, and had been considered a fibroid tumor of the uterus by myself and other ovariotomists elsewhere.

CASE CXCVIII.—Was one of rapid development of abdominal cancer, with ascites, and death four months after recovery.

CASE CCVIII.—Was the second case of fibroid tumor of the ovary, and with it were connected cysts, but the primary tumor which filled the pelvis was fibroid in its structure. This case and No. CXCVI are the only fibroid tumors of the ovary I ever saw.

CASE CCX.—Is remarkable in many ways. She had been tapped eighteen times. The fluid from the more recent tappings had been ascitic; she was emaciated and feeble, and her abdomen contained much fluid, and at least two hard tumors that felt like fibroids, and were very close to, if not a part of, the uterus. On opening the abdomen in the usual place the abdominal walls seemed more thick and vascular than usual, and yet beneath them could be felt the ascitic fluid. On cutting deeper, a tissue looking like the lining membrane of the bladder was seen. The incision at this point was abandoned and a new one made at the umbilicus; here the normal peritoneum was easily opened. About twenty pounds of ascitic fluid ran out, and when the abdominal cavity was empty of fluid two papillomatous tumors of the ovaries were seen. The right was the larger; both had been burst for some time, and papillomatous material extended beyond the cyst wall of each. After the pedicles had been tied close to the uterus the bladder was inspected. It was found that the wall of the bladder was a part of the anterior abdominal parietes, and that it had been opened for an extent of about four inches. The outer walls of the bladder were sewn together, about twelve interrupted sutures of silk being put in. Care was used so as not to include the mucous membrane. A drainage tube was used in the abdominal cavity, and a catheter was kept in the bladder. On the whole, the operation was completed in a satisfactory and workmanlike manner. This incorporation of the bladder with the abdominal parietes and its extent upward toward the umbilicus is an anatomical anomaly of great awkwardness to the ovariotomist. The convalescence was tedious and accompanied by much pain and some considerable suppuration, but the patient went home after six weeks, and now writes that her health (two years after the operation) is perfect, but that the cicatrix has never entirely closed.

CASE CCXII.—Is remarkable as being the only cyst of the broad ligament that contained papillomatous masses, the ovary not being affected. The wall of the cyst was quite thick, perhaps half an inch thick, whereas the walls of most cysts of the broad ligament are thin and rather delicate.

CASE CCXIII.—Was one of twisted pedicle. The patient was 63 years old. On opening the abdomen the cyst was seen adherent and blackish on the surface. The pedicle (on the left side) was found to be tightly twisted four times, and was rigid and white. The vessels were all plugged and no ligature was required.

CASES CCXV AND CCXVI.—Were both simple cases in healthy young women; both were fatal, and were, it will be seen, consecutive; the same cause of septicism was present in both cases. They occurred during almost daily operating, and their fatal results are to be attributed to some fault of mine, but exactly what I don't know.

CASE CCXVIII.—Was confined November 1st, 1886, thirteen months after her recovery from ovariotomy. The child was a female, the left ovary remaining.

CASE CCXXII.—Was one of rapid recovery in a lady 61 years old. It is remarkable as being the only one I have seen and tried to relieve of intestinal obstruction caused by the operation. I was called in April, 1886 (four months after her recovery), on account of symptoms of obstruction which had existed for several days. On opening the abdomen I found the small intestine adherent at intervals to the cicatrix in the peritoneum; through the openings between the intestine and the abdominal wall several loops of intestine had passed, then had become strangulated and sphacelated, and there was more or less offensive blackish fluid in the abdominal cavity. An artificial anus was made and gave relief, but the patient died in a few days.

CASE CCXXVIII.—Recovered, and went home toward the last of February, 1886. She was delivered of a child, a female, December 13th, 1886, less than ten months after leaving for home, and just ten and a half months after ovariotomy, on January 27th, 1886. The ovary remaining was the left. This is very quick work—ovariotomy, recovery, impregnation and delivery, all within eleven months.

CASE CCXXIX.—Should not be counted among the ovariotomies. The patient was in articulo mortis, and the cyst, a gangrenous one with a twisted pedicle, was only removed because I knew that I should feel at the autopsy as if I ought to have removed the cyst while the patient was alive, even if recovery seemed impossible.

CASE CCXXXVIII.—Was another case of twisted pedicle.

CASE CCXL.—Was a double ovariotomy, with ascites and myxomatous tumors. A piece of omentum, thick and apparently cancerous, about an inch and a half in diameter, was removed also. Ovariotomy was done in April, 1886, and now, July, 1887, she is in excellent health. On deep pressure, a small movable tumor can be felt in the right umbilical or iliac region, but the omental tumor has apparently not increased in size.

CASE CCLI.—A simple enough operation, except that a hard (dermoid?) tumor, about the size of an English walnut and without a pedicle, was removed from the peritoneal region above the bladder; perhaps this was the left ovary which had become detached at some former time. The tumor was a multilocular one of the right ovary with adhesions, and the patient did perfectly well till the fifth day, when she began to have stiffness of the jaws and spasms of a convulsive nature. She died on the sixth day, of tetanus.

CASE CCLII.—Is the largest I have ever removed; the tumor and contents weighing 111½ pounds. The patient recovered.

CASE CCLXI.—Is remarkable as being a second ovariotomy on a patient 72 years old, and with a large ventral hernia, and in whom the uterus had to be removed with the tumor. The case was successful and the hernia was cured. Drainage was used.

CASE CCLXII.—Was another case where the uterus was removed with the ovarian tumor. It was successful.

CASE CCLXXIX.—I have called a recovery with a question mark. The operation was done on May 20th, 1887; from May 23d till June 13th, twenty-one days, her temperature was normal and her pulse was generally between 70 and 80; her appetite was good. On June 13th she awoke early and demanded an early breakfast, and ate, perhaps inordinately, of bread, oatmeal, and hashed veal stewed in butter. An hour later she had the juice of an orange. At 10 A. M. she complained of severe pain in the stomach, and later vomited. Her temperature rose to 105° and her pulse to 174, and she died. There was no autopsy. The patient was a hundred miles or more from Boston, and I am at a loss to account for the sudden death. Her sister and husband thought it was severe indigestion caused by the veal. It is unfortunate that there was no autopsy.

CASE CCLXXXV.—I have also called a recovery with a question mark. For three days after the operation her temperature was about 101°, pulse 100, but from that time on pulse and temperature were both normal. Drainage tube removed on sixth day. On the sixteenth day after operation she sat up out of bed for the first time. After being up for about three-quarters of an hour she complained of difficulty in breathing, which rapidly increased; she grew purple in the face, and died in an hour from her seizure. Autopsy showed both lungs filled with emboli, completely occluding the air passages. The origin of the embolism was not discovered. She had complained of some pain in her legs, not an uncommon symptom after ovariotomy, but there never had been any swelling. A careful examination of the femoral arteries failed to show any starting point for the trouble.

CYSTS, STITCHED TO SKIN, UNCOMPLICATED EXCEPT BY ADHESIONS.

I have been very agreeably disappointed of late years in the results following the drainage of ovarian cysts which could not be removed. I think I have stitched adherent ovarian or parovarian cysts to the edges of the incision, and have drained them and syringed them out patiently for several weeks eight times, and of these cases all have recovered.

The first of these cases was operated upon in the centre of Massachusetts, and I had nothing to do with the after treatment. The drainage tube was removed soon, and the patient recovered and gained flesh and strength, but in less than a year the growth increased, or a new tumor was produced, and an attempt to remove this tumor resulted fatally.

The second case was much relieved, but died subsequently of cancer of uterus and liver, which existed probably at the time of the laparotomy.

The third case was one of the broad ligament, complicated with tubercular peritonitis, and the patient is now, three years after operation, well and strong.

The fourth case was one of adherent papilloma, the opening never entirely closed, and the patient subsequently died of consumption a few months after operation.

The fifth case was one of the right broad ligament, and the patient is now well, two years after operation.

The sixth case is now in good health, stout and strong. There is still a sinus, with a pin-hole opening, discharging a drachm or two of pus a day.

The seventh case, one of double ovariotomy and stitching the cyst of the left ovary to the skin, recovered, contrary to my expectations, and is now well.

The eighth case also recovered.

CASES OF TUMORS COMPLICATED WITH OTHER DISEASES, AND IN WHICH THE CYSTS WERE STITCHED TO THE SKIN.

Of these there are seven, all fatal.

The first case was one of uterine cyst of large size; both ovaries had been removed

on account of cystic disease two years previously. The patient died on the thirteenth day after the operation, of septicæmia.

The second case was one of ovarian tumor and uterine tumor. The ovarian tumor weighed forty-five pounds, and when this had been removed a large uterine fibro-cyst came into view. This latter was opened, emptied, and as it could not be removed, its edges were sewn to the skin and drained. Death occurred on the fifth day.

The third case recovered so as to leave hospital and go home, but died soon afterward; the tumor was a very adherent one, of uncertain origin. There was no autopsy.

The fourth case was one of malignant uterine tumor, irremovable. The operation was fatal on the second day from shock.

The fifth case was one of extra-uterine pregnancy of seven years' duration. The cyst was emptied of about four pounds of offensive, yellowish-green, fatty fluid, and the complete skeleton of an adult fœtus. The case is reported in *The Boston Medical and Surgical Journal*, Vol. CXIV, page 457, and the bones, beautifully mounted by Dr. O. K. Newell, are in the Warren Museum.

The sixth case was one of adherent ovarian tumor complicated with a uterine fibroid. The latter was removed, the stump being treated intra-peritoneally. The case was rapidly fatal.

The seventh case was one of tubo-ovarian cyst, with hydrosalpinx on the other side, chronic pelvic peritonitis, fibro-myoma and polypus of the uterus. The tubo-ovarian cyst was punctured and stitched to the skin. At the autopsy, purulent peritonitis, granular degeneration of the kidneys, and the affections above mentioned were found. Death occurred on the thirteenth day.

LAPAROTOMIES FOR THE REMOVAL OF UTERINE TUMORS.

My hysterectomies and removal of uterine tumors number twenty-seven. It will be seen that although the record is not brilliant, and does not compare in any way with Dr. Keith's wonderful list, yet out of the last seventeen fourteen recovered, and none of the cases were done except for the reason that life was a burden, and death was impending. Familiarity with the operation has made me more skillful.

CASE III.—Was a supra-vaginal removal of a two-horned uterus (uterus bicornis), one horn of which was dilated and did not communicate with the vagina. A number of pounds of dark, bloody fluid was contained in this cavity and in the distended Fallopian tube. The stump was treated extra-peritoneally, and convalescence was rapid. The patient's age was eighteen. She is now, four years after the operation, well and working hard. The operation was done to relieve agonizing pain.

CASE VII.—Is a very remarkable one, in which four pounds of tumor, which could not be removed at the time of the operation, were gradually extruded through the wound after the wire of the serre-nœud had come off. The patient was a married woman, 36 years old; never pregnant. The tumor reached above the umbilicus and was mostly on the left side. Menorrhagia was continuous and at times violent, had been going on for eight years, and was attended with excruciating pain. Laparotomy was done on the 12th of June, 1884. The tumor had grown under and into the left broad ligament; the intestine was adherent to the apex of the tumor. Kœberle's serre-nœud was adjusted around the growth and the wire pushed down into the pelvis as far as possible. The loop had above it the right ovary and tube, but not the left, and the latter I was not able to feel. The wire was tightened as much as possible, and when screwed home another serre-nœud was put on and the first one was removed. This case showed the advantage of always having two serre-nœuds and induced me to get one with a longer screw. When I could compress no longer, I passed a long pin through the stump to keep it outside. Only about half the tumor was removed, the left side of the pelvis

being still filled with the growth. The top of the uterine cavity was opened. The clamp came away on the fifth day. And now began the very remarkable course of this case, which had so far been like so many others. The remaining portion of the tumor began to push out of the abdominal wound through the hole left by the stump, and as it rapidly grew larger and larger, elastic ligatures were tied around its base, daily, on and after July 15th. On the 18th of July, *i. e.*, thirty-two days after the clamp had come off, the protruding mass was pretty well pediculated. It was about as large as my head, vascular-looking, and was the portion of the tumor left behind at the time of the operation, and had not only been extruded, by uterine contraction I suppose, but had nearly doubled in size. I now tied a strong ligature of silk around the base of the tumor and cut the protruding part away with strong scissors. The tumor removed weighed *four pounds*. Some constitutional shock, vomiting, and a rise of temperature followed the removal of the tumor, but this soon subsided. In January, 1885, she was well, fat and free from pain, and has continued so ever since. In February, 1885, occurred the last uterine flow, and nothing has been seen since. The body of the uterus covered with a smooth cicatrix comes up against the abdominal scar at the seat of the incision, and there is now a small ventral hernia below it. So far as I know, this case is unique, though I do not know why the uterus should not be able to squeeze out a fibroid through an incision from above as well as through one made from the vagina. This case I watched with great interest, and the outcome has been very fortunate, for these partial removals of uterine tumors are apt to be unfortunate in their terminations.

CASE XIII.—Was one of the few in which drainage was used after hysterectomy. The patient was 30 years old, and the tumor was the size of an adult head. She is now (July, 1887) well and strong.

CASE XXIV.—Besides being a large tumor (38 pounds), is remarkable for its happy result, the top of the bladder having been taken off by the serre-nœud. The wire came away on the third day, and the urine flowed from the wound, only a little remaining in the bladder; after various plans had been tried, I finally decided to keep a Sims' self-retaining catheter in the bladder continuously. This was done successfully without any cystitis; the hole filled up very slowly, its healing being retarded by occasional overflows of urine, but at length, after about seven weeks, the patient went home with the sinus solidly healed and able to retain her urine the normal length of time.

CASE XXVI.—The operation in this case restored a patient to a life of comfort, and relieved her of great suffering and debility. The patient was a married woman 51 years old; she had had three children, of whom the youngest was 15. In appearance she was very pale and anæmic. Her flowing had always been excessive, except during the year 1882, when electrolysis had been employed and masses of sloughing tissue had passed out of the uterus into the vagina with great pain. Her tumor, which at that time was the size of a cocoanut, entirely disappeared after the electrolysis, and although very ill for many weeks she recovered and was comfortable, so far as the menorrhagia was concerned, for a year. In 1883 the flowing and pain recommenced and kept on increasing. February 12th, 1887, hysterectomy was done, the stump being treated extraperitoneally. At that time the woman was very pale from excessive flowing, there was a mitral murmur, her left leg was swollen, she was short of breath and had constant abdominal pain. The operation was simple, except that three wires broke while being tightened. Recovery was rapid and she went home free from pain.

REMOVAL OF UTERINE APPENDAGES FOR FIBROID TUMORS.

CASE I.—A married woman, 33 years old. I thought the tumor ovarian, from its apparent fluctuation. At the operation, October 15th, 1884, the tumor was found to be the uterus symmetrically enlarged and looked like the impregnated uterus. It was apparently full of fluid, but none could be obtained by aspiration. When the abdomen

was opened the tumor was tense, but when the operation was finished it was quite flaccid; both ovaries and tubes were removed; there was a slight quantity of ascitic fluid. Her monthly sickness has been profuse at times, and there has been more or less constant slight flowing for weeks. A serious flooding spell occurred in June, 1886, since which time the catamenia have ceased. The tumor is now (July, 1887) rather smaller than it was three years ago.

CASE II.—Was a great success. The patient was a married woman, 44 years old. Catamenia always irregular. At times she has been in insane hospitals, and has had delusions. At each menstrual period she is very violent and suffers greatly. Has had several attacks of severe uterine hemorrhage, requiring plugging. The abdomen was very tender, and was occupied by a tumor which was solid and extended from the cavity of the pelvis, which it nearly filled, to the umbilicus. She stated that she could no longer bear the continual pain and the monthly agony of menstruation. A few ounces of serum ran out on opening the abdomen, January 18th, 1885, and the tumor above mentioned came into view. It was a uterine fibroid. The ovaries and tubes were easily seized and removed. She recovered rapidly. Her depression left her, she was free from pain, menstruation entirely ceased, and the tumor diminished in size rapidly. On September 18th she came to my office, bright and well; I could not feel the tumor by external examination. She was very grateful, and had been cured not only of the tumor, but of her mental troubles. She is still (July, 1887) in perfect health.

CASE III.—Was a case of removal of the appendages for great hemorrhage accompanying a fibroid, about the size of a large orange, in a patient 34 years old. The patient was very much blanched. She had known of the existence of the tumor for three years. The abdomen was opened August 25th, 1885. The omentum came into view, and under it was a symmetrical, round fibroid, filling the pelvic brim. With considerable difficulty and force the uterine appendages were dragged up and removed. I could not get a sponge down between the pelvic brim and the tumor, and so could not sponge out the pelvis. The patient recovered rapidly. May 13th, 1886, she was well and strong, and had gained twenty-four pounds of flesh. For three months after the operation she had flowed unceasingly, but the whole amount was nothing to what she formerly had at one menstruation. From November, 1885, till February, 1886, the flowing wholly ceased; then she began to flow and continued to do so till May, 1886, since which time I have not seen her. From August, 1885, till May, 1886, *i. e.*, nine months, she said she had not lost one-tenth part of the blood she lost during the preceding nine months.

CASE IV.—Was one of extreme anæmia and suffering. I had to pull the tumor out of the abdomen to get at the ovaries, and then found them imbedded in the tumor. I dug them out and tied the pedicles, and pushed the tumor back. The whole operation required much force, and hysterectomy ought to have been done instead. The patient died on the third day.

CASE V.—A married woman, 36 years old; had been flowing for fourteen years, and now worse than ever. There was a fibroid about the size of an ordinary orange. Both ovaries and parts of both tubes removed on January 21st, 1886. She recovered rapidly, the uterine flowing has been unchecked, and in December, 1886, when I last heard from her, she was much discouraged, and was flowing as badly, if not worse, than before the operation.

REMOVAL OF UTERINE APPENDAGES FOR THE CURE OF NERVOUS DISORDERS.

CASE I.—I saw the patient, a single young lady, 22 years old, in March, 1883. Briefly her previous history was the following: She was taken ill in March, 1877, and has since been most of the time in bed, suffering severe pain in the left ovarian region.

Her menstruation had always been irregular, and was accompanied at times by sudden, severe, spasmodic pain in the left iliac fossa. These attacks were accompanied by confusion of ideas and severe pain and tenderness along the spinal column. At one time she became, to all appearance, insane, at times melancholy, at others destructive, and again abusive; her whole body at times became rigid. (Hysteria.) This state of things lasted for about six weeks, when she returned suddenly to a normal state of mind, and became bright and intelligent. After a few months she became ill again, and complained of the pain mentioned above as being very exquisite. The least touch on the left side of the abdomen seemed to cause pain. Specialists in cerebral, nervous and uterine diseases were consulted. The general conclusion was reported to me as being that she might obtain some relief from palliative measures. Morphia was given to her, and she became addicted to the use of the drug. She was for three months in a water-cure establishment in Maine, in 1879. In January, 1881, she went to the Adams Nervine Asylum in Boston, and remained four months. In May, 1882, she went to Dr. Ring's Sanitarium at Arlington Heights, near Lexington, Mass., and I saw her there. During these seven years there were some months when she could be partly about the house, but most of the time she was in bed. The uterus was very small and undeveloped, and an imperfectly defined body near the posterior portion of the cervix, on the left side, was thought to be possibly a prolapsed ovary. Pressure on this substance caused, apparently, agonizing pain. Removal of the ovaries was suggested to her, and she eagerly desired it to be done if there was a shadow of hope that it would benefit her. After hearing this history, and consulting with Drs. J. T. G. Nichols, Ring and Westcott, I agreed with them that removal of the ovaries would be a justifiable procedure, a safe operation and perhaps beneficial.

The operation was done March 26th, 1883. Her convalescence was normal, except that the left parotid gland became swollen on the fourth day, but this gradually subsided. At the end of two weeks she could walk about, and she went home at the end of six weeks. Her mind had become perfectly clear. The pain and soreness in the left iliac region had nearly gone, and she felt very glad that the operation had been done. So far as I could see there was nothing remarkable about the ovaries and tubes removed. One of the ovaries was smaller than the other, and both contained small cysts; the lining of the Fallopian tubes was fatty, but there was nothing to account for all the pain and nervous phenomena. The immediate relief of the operation was great, and for about a year the patient was very comfortable, but not strong. She never has had any of the old severe pain in the iliac region, and the relief from this seems to be due to the moral or physical effect of the operation, at any rate to the operation. In 1884 she had a time, when her hysterical symptoms returned, and she recommenced the use of morphia, but this was again given up and she recovered. In answer to a circular sent in December, 1886, she writes, after saying that she has never menstruated, "The operation was very successful in its results, excepting an adhesion to the bowels and great trouble with constipation. Up to a year ago last summer (*i. e.*, 1885) my health was better than for years, but owing to overwork, and a fall which injured the spine, I have been confined to my bed ever since that time." Of course she does not know whether there is an adhesion or not, but she does know that there is constipation. Now, looking at the present condition of the patient, in bed and confined to her bed now for two years past, I cannot see that the operation has been of much, if any, permanent good, though I ought in fairness to say that she and her family feel that it has been very beneficial.

CASE II.—This case is a brilliant one. The patient was 19 years old. In her childhood she was easily excited and not easily managed, subject to paroxysms of temper, in which she would scream, throw herself about, break furniture, and tear her clothing. At eight or nine years old she was found bathing with boys, naked, and she said

she had frequently had connection with older boys. She was sent to various Homes and Schools, but could not be made to mind. At length, at *nine years of age*, she was sent to the Taunton Insane Asylum, where she remained four years. Here she was very violent, noisy and destructive, perfectly reckless of personal injury, throwing herself headlong down flights of stairs if an attempt was made to secure her. At the end of four years she was removed to the Asylum for the Chronic Insane at Worcester. After studying the case for a year, and with the consent of her mother, personal chastisement was tried; at first with good effect. She went home from Worcester in May, 1880, and behaved admirably for four months. At this time she was sixteen years old. In October, after a scanty menstrual flow attended with considerable pain, she began to complain of her head, and appear nervous. After a few days, being agitated by the fact that her mother had found she was studying surreptitiously, contrary to her wishes, she rushed to the second-story window, and in an instant had jumped from the roof of a veranda, and was found screaming and maniacal on a walk below. She was now sent to Danvers Asylum, with hallucinations of sight and hearing. Dr. Goldsmith, in reporting her case,* says, "Since October 14th, 1880, she has been a patient at the Danvers Hospital, where she has engaged the sympathy and exhausted the resources of treatment, medical and moral, of every one who has come in contact with her." Not to make this account too long, I will say that Dr. Goldsmith decided to try the effect of removing the ovaries, and I did the operation July 20th, 1883. Recovery was rapid. She left the Asylum at the end of four weeks, and has since that time, for four years, been at home, perfectly well, living the same life as the other members of the family. She has not menstruated since the operation. A perusal of Dr. Goldsmith's paper, which has been reprinted with the title, "A Case of Moral Insanity," will well repay the reader.

CASE III.—Was an utter failure. The patient was a married woman, 27 years old, formerly a school teacher. At about 24 years of age she was married; within a few weeks unmistakable signs of mental derangement appeared; she attempted suicide by drowning and was sent to an Asylum. She was a very violent patient. When I saw her she was crazy and more or less demented. The superintendent, from whose Asylum she came, said that any operation would be perfectly useless, but her physician was convinced that her excitement was more at the menstrual period, and that her insanity was more or less connected with the sexual organs. But the superintendent was right. She recovered rapidly from the removal of the ovaries and tubes on December 13th, 1883. She was kindly cared for at the McLean Asylum for more than a year, and was neither better nor worse mentally. She destroyed everything she could, broke windows and struck her attendants, and was demented. She died, of phthisis, in the Worcester Lunatic Hospital, in 1886, never having menstruated since the operation. Her case showed how unnecessary perfect quiet is after laparotomy, for she had to be tied to the bed, and frequently broke all the fastenings and the straps of the Crosby bed on which she lay, and yet the wound united by the first intention, and she never had hernia.

CASE IV.—Another perfect failure, as far as relief and cure are concerned. Her symptoms were pain and tenderness in the left iliac region ever since she could remember, worse on exertion or on standing. Catamenia regular, painful. Married seventeen years, no children. Appetite poor. Went to the Adams Nervine Asylum and remained five months; came home more nervous, weak and hysterical. Dr. Chamberlain, of Lawrence, found a tender spot and a body in Douglass' space that he thought might be a displaced ovary. She stated that her life was a perfect burden, on account of general illness, hyperæsthesia, pain in back, in hands, etc., and she thought that all her symp-

*American Journal of Insanity, October, 1883.

toms seemed to originate in her side and from the pain that she felt between the umbilicus and the pubes. After etherization, her case reminded me of Case I, which had been improved so much at first. Both ovaries were removed on August 24th, 1885, but not the whole of both tubes. Nothing especially remarkable was found on examination of the ovaries. She recovered rapidly, and has suffered nearly all the time since as she did before the operation, on the whole, perhaps, in a milder degree. She menstruates regularly every twenty-two days, and menstruation is accompanied by much pain.

CASE V.—Was another case of hystero-mania with morphia eating, in a single woman, aged 28. The history corresponds somewhat with that given in detail in Case I. The ovaries and tubes were removed November 25th, 1885. She recovered, and wrote that she was getting well and beginning a "new life," and that the past had been a "nightmare" or "horrid dream." Her menstruation is irregular, but occurs about every three weeks. I cannot learn (July, 1887) that she is any better than before the operation. On the whole, I cannot say that my experience with these five cases would lead me to advise removal of the uterine appendages for the cure of nervous symptoms and hysteria, unless the operation were advised by a competent alienist. I acted in these cases as an instrument in the hands of others.

EXPLORATORY LAPAROTOMIES.

CASE I.—Was in a lady, 62 years old, so large that I could make no decided diagnosis, and in whom respiration could not be carried on in a recumbent position. I had to kneel down and cut upward while the patient lay on her side. About forty pounds of ascitic fluid were removed, and a soft, friable tumor attached to the sacrum and right ilium was found. It was impossible to remove the tumor. The abdomen was thoroughly sponged out and the wound sewn up. The patient recovered rapidly and was much relieved. The ascitic fluid *never reaccumulated*. About a year afterward the patient died, and at the autopsy, a soft sarcomatous mass, filling the pelvis and lower abdominal region, was found.

CASE II.—Was a case of ascites and burst papillomatous cyst, attached to the pubes, ischium, and pelvic organs generally. The ascitic fluid reaccumulated, and the patient died about eighteen months later, having steadily refused to be tapped.

CASE III.—Was a solid adherent tumor, whose attachments and origin were not determined. The patient recovered, but was not benefited nor harmed; her condition remained about the same. I think she is now dead, but she lived some years.

CASE IV.—Was a case of large malignant tumors of ovaries, liver, omentum, and intestinal organs generally. The patient recovered, and died in March, 1883, six months after operation.

CASE V.—Was that of a woman, 21 years old, with a considerable amount of ascitic fluid and many little thin-walled cysts attached to the peritoneum and intestines; the pelvis was more or less filled by a friable tumor filling the right side. Its exact attachments were not made out, but it was immovable and was not interfered with. The patient recovered, and during the next four years fell into the hands of several surgeons who tapped her. In 1885 she reported herself to Dr. J. Foster Bush as having entirely recovered, and that her tumor had entirely disappeared. If this be true, the growth must have been syphilitic, I think, perhaps of the nature of gumma.

CASE VI.—One of abdominal cancer. Recovered from the operation and went home, but probably did not live long.

CASE VII.—Was another case of general abdominal cancer with ascites, and was fatal.

CASE VIII.—On opening the abdomen, ascitic fluid ran out and coagulated fibrin, exactly like the coagulated fluid of pleurisy after it has been removed from the thorax; this was bailed, scooped and sponged out. The omentum had become an elongated tumor, of a pinkish coral color; the spleen was in the same condition. There was

general cancer. Neither the uterus nor ovaries could be felt; they seemed to be shut off by a wall of cancer. Nodules were felt in the mesentery. The patient recovered from the operation, and was quite comfortable for a time, but died about seven months later.

CASE IX.—Was one of tuberculous salpingitis, with cheesy masses extending upward to the diaphragm on both sides, and general tubercular disease of the peritoneal tissues matting them together. The operation was fatal. The age of the patient was 17 years.

CASE X.—This case was one of tubercular peritonitis with ascites, in a thin, emaciated, feeble, single girl of 21. What looked like the stomach distended with fluid filled the upper portion of the abdominal cavity. This tumor was about ten inches in diameter. Owing to the presence of lymph and adhesions, the liver and spleen could not be felt. Much lymph was lying in masses and flakes upon the abdominal viscera. There were deposits of tubercles sprinkled about on all the abdominal contents seen. The wound healed by first intention, but opened spontaneously on the eighteenth day, and gave exit to much clear serum. She went home in good spirits, July 12th, 1884. The catamenia, which had been absent since February, 1884, returned in January, 1885, and have been regular since. The wound continued to discharge, but the amount gradually diminished, until in May, 1885, it was very slight. She became fat and strong and able to do her housework, and gained over thirty pounds in weight. She was married in June, 1885. I saw her in November, 1886. She was strong and well. The wound had contracted to a little sinus, which would admit a probe, and which discharged about a drachm or less of pus a day. A wonderful recovery.

CASE XI.—Was that of a married woman, 46 years old. Her abdomen was full of ascitic fluid, and contained a tumor reaching to the ensiform cartilage. She had been tapped five times in four months. When the abdomen was opened, about twenty pounds of ascitic fluid ran out. The parietal peritoneum and that of the bowels, mesentery, and abdominal viscera was covered more or less with cancerous deposits, a few of which were removed for microscopic examination. The left ovary was a tumor about the size of a large football, and was covered with adherent intestines and with the growths above mentioned. It was impossible to remove the tumor, and the cancerous condition of the peritoneum seemed to preclude complete recovery. Two large india-rubber drainage tubes were put in on either side, and another small one in the pubic region, with the hope of establishing permanent drainage. In the first twelve hours much fluid was discharged, but this gradually ceased, and in a few days all the tubes were removed. I have never succeeded in establishing permanent drainage in ascites by means of tubes, and I never expect to. The track of the tube becomes surrounded with lymph, and the only portion of the peritoneal sac drained is that tubular portion in which the drain lies. Sometimes, as in Case X, Nature establishes drainage and cures the patient, but she does this without the aid of tubes. The operation was done on the 3d of January, 1885, and she died six months later.

CASE XII.—Was that of a little girl, ten years old. The abdomen was found filled with hard tumors, thoroughly adherent and immovable. The operation was done July 9th, 1885, and she died on the 27th of April, 1886, having lived very comfortably most of the time. The growth found its way through the skin of the abdomen about three months before death.

CASE XIII.—Was one of malignant abdominal tumor. Much more comfortable after laparotomy.

CASE XV.—Was one of small adherent tumor attached to the brim of the pelvis on the right side, and to the intestines. It could not be removed. The patient recovered from the operation, and died in July, 1887, eight months after recovery from the operation.

CASE XVI.—Was another case of malignant tumor of the omentum and peritoneum, in which I tried to establish drainage by means of rubber tubes, but the ascitic fluid accumulated in spite of the tubes. The operation was done November 12th, 1886, and was successful so far as immediate recovery went, but the patient died some months after her return home.

CASE XVIII.—Had ovariotomy successfully performed six years before. The exploratory operation was done in February, 1887, but the tumor of the remaining (left) ovary could not be removed. The operation was fatal.

CASE XIX.—Was another case of tubercular peritonitis in a fat, healthy-looking girl of 17. The operation was successful. Time enough has not elapsed to decide whether the abdominal exploration will be curative.

LAPAROTOMIES FOR RENAL TUMORS.

CASE I.—A married woman, aged 30, urinary symptoms coming on during pregnancy. In 1878 patient became pregnant, and when quite large, at eight months, noticed gravel in the urine, and suffered from sharp, lancinating pains in region of ureter; at times, the pain was so severe as to cause her to faint. Her confinement was completed, and in two years she became again pregnant, and was confined of a second child. After one of her attacks of pain, vomiting, cough and general constitutional disturbance, she noticed a swelling in the right hypochondrium and right lumbar region, which has increased since. At this time her urine was loaded with pus, and her micturition was very frequent. I saw her in April, 1882; at that time her urine was chocolate-colored; sp. gr. 1033; much sediment; reaction acid; albumen one-half per cent.; sediment, pus, blood, hyalin, and granular casts, and a few fatty casts. Her fæces contained much blood and pus. A tumor, the size of a large cocoanut, occupied the whole of the right hypochondrium, and reached nearly to the spine; it was uniformly firm and tense, but fluctuating and movable as a whole. On April 19th, a vertical incision about three inches to the right of the umbilicus was made; it was about four inches long. I came down upon a dense membrane (the peritoneum) covering the tumor. A director was thrust in and dark brownish fluid welled up; a free opening was then made, and about six ounces of inoffensive pus ran out. The kidney was then explored by the finger, and two calculi, one two inches and the other half an inch in diameter, were removed. A drainage tube was fastened in, and the wound dressed with carbolized gauze. Vomiting had been one of her symptoms for several months, and was not entirely relieved by opening of the abscess and the removal of the calculi. The blood and pus in the alvine discharges ceased, and she became much more comfortable, and could lie on the right side; the abscess was washed out daily, and at times small stones (one-eighth inch in diameter) and gravel were brought out. On May 2d the urine was pale, clear, with very little sediment, about one-half per cent. of albumen, and contained a few granular casts, pus, and epithelial cells. In about a fortnight she sat up, and ate pretty well, but continued very weak. The tube was removed on May 14th, as it was causing some soreness and the fistula seemed well established. On the 16th, another tube was placed in the wound. She gradually became weaker and weaker, and never really gained much, though the operation relieved her for a time. On May 23d she died. No autopsy was allowed. This patient, with serious disease of the kidneys, was not a good subject for any operation, but to-day I think I should make the opening in the loin instead of along the outer border of the rectus muscle, for I think the drainage would be better, though I am inclined to think the case was a hopeless one, if any case ought to be considered so.

CASE II.—The case, one of sarcoma of the left kidney, weighing three and a half pounds, occurred in a man, by occupation a seaman, 29 years old. His symptoms were the discovery of the tumor in the left hypochondrium in August, 1882, emaciation,

blood in the urine at times, pain in the back and loss of strength. After considerable study and examination, I decided that the tumor was renal sarcoma. An incision was made along the course of the left linea semilunaris, and the tissues divided. The empty descending colon was spread out very thin and stretched over the tumor, and was not immediately recognized as such, and was slightly wounded at the very first incision. The opening was sewn up with a continuous silk suture and the operation continued. The incision was carried downward near to the anterior spine of the ileum, and upward through the cartilage of the tenth rib; this incision was supplemented by another, at right angles, through the oblique and transversalis muscles as far as the quadratus. An incision was then made through the posterior layer wall of the peritoneum, and the tumor peeled out of its bed, very much as one would pull out a kidney at an autopsy. The renal vessels and ureter were then secured and tied, and the substance of the kidney cut through on a level with the pelvis. The seat of the tumor was then cleansed, a rubber drainage tube passed into the cavity from the loin, and the wound sewed up. Almost no urine was secreted after the operation, and he died, November 26th, 1883, two days after nephrectomy, only eight ounces of urine having been drawn from the bladder in the two days following the operation. At the autopsy, general peritonitis and cloudy swelling of the right kidney were found.

CASE III.—Was one of large sacculated right kidney, full of pus, in a woman 42 years old. I thought the tumor cancerous. The incision was along the course of the linea semilunaris. The ureter was easily isolated and secured in the lower angle of the wound. The renal vessels were secured and tied, and the kidney removed. I thought the operation went off very well, but the woman died on the third day with suppression of urine. The kidney and pus contained therein weighed ten and a half pounds. Only three ounces of urine, which was chiefly pus, was drawn from the bladder after the operation. At the autopsy there was not the least trace of peritonitis, and there had been no hemorrhage. I suppose the death was due to shock and suppression of urine.

MISCELLANEOUS LAPAROTOMIES.

REMOVAL OF TWO IMMENSE LIPOMAS.

CASE I.—This tumor occurred in a man 38 years old. The tumor was first noticed in March, 1881, though it must have existed long before that date. The tumor filled the abdominal parietes and seemed to fluctuate. It had been aspirated many times without any result; it was fast causing distress and entire inability to work. On October 30th, 1881, I made an exploratory incision fifteen inches long. There were slight adhesions to the liver. The tumor was about two feet long in its longest diameter, and about a foot long in its shortest. It was covered by peritoneum. I pulled the tumor out of the abdomen until I found that its envelope ran down toward the spinal column, and was then reflected upon the abdominal parietes, *i. e.*, it was retro-peritoneal. At the lower part there were no adhesions to the bladder, but the tumor seemed to have a central pedicle next to the spinal column, extending from the neighborhood of the cœliac axis downward along the lumbar vertebræ. As it was not known what organs the tumor might contain, and as it was feared that the removal of it might be fatal, from shock and hemorrhage, it was decided to replace the mass, and with great difficulty it was pushed back under the peritoneum and abdominal muscles and skin, and nearly one hundred sutures were required to close the incision. The patient recovered rapidly, and went home with the wound healed. (This operation was an exploratory incision, and should have found its place among the exploratory operations.)

The patient went about traveling in the horse-cars and going where he pleased. He walked with considerable comfort, wearing a supporting sling which held up the tumor by straps passing over his shoulders. He became more and more impatient to

have another attempt made to remove his burden, and I reluctantly and rather against my better judgment consented to try again. He said, "You know it is neck or nothing this time, Doctor." On the 5th of February, 1882, I opened the abdomen by the side of the cicatrix of the former incision, and removed two tumors weighing fifty pounds. They were myxo-lipomas. The incision was about twenty inches long, and a transverse incision four inches long was made on the left side just above the umbilicus. The ascending colon crossed the tumor transversely. The peritoneal capsule of the tumor was more or less torn and ruptured, and the tumor was lifted up with great difficulty, owing to its weight. The ascending colon was separated from the tumor with some difficulty and rolled off, after dividing and tying most of its mesentery. The vascular attachments of the tumor were clamped and tied or burnt, from time to time, as was necessary, and the tumor was removed from the abdomen. Another apparently purely myxomatous one now came into sight; after some hesitation this was removed also; it occupied the right hypochondriac and lumbar region, the liver being pressed upward and to the left into the epigastrium. The peritoneal capsule of this tumor was tough and strong; some of it was removed with the tumor and some of it was left behind. All bleeding points were now secured. Another tumor about ten inches long was now seen lying on the abdominal aorta and pulsating with it. This I decided to let alone. Everything looked as favorable as one could expect after so severe an operation; not much blood had been lost, the pulse was 85, and very feeble, but not extremely so. The operation had been thoroughly antiseptic. The intestine which had been pushed off the tumor, or rather from under which the tumor had been withdrawn, was largely deprived of its mesentery and might not be well nourished, but, with this exception, affairs looked as promising as after a severe successful ovariotomy. The patient was put in bed and the foot of the bed was elevated. He seemed to do very well and was conscious and comfortable five hours later, and seemed to be getting along very well. He asked for a drink of water, and when it was brought said, "I think I am going," and died. The autopsy was very kindly made by Dr. Gannett, who found three tumors of various sizes and the same character as those removed, and no peritonitis.

CASE II.—Was exactly like No. 1, except that the patient was a female, 61 years old. I saw her in 1881, and could not believe that the tumor did not contain fluid until I had aspirated it in many places without result. It fluctuated and gave a wave on percussion, but it was solid, or at least semi-solid. The operation was similar to the one just described, and the situation of the tumor the same. The patient died soon after the completion of the operation. The tumor weighed 35 pounds. The operation was done March 21st, 1882. These are the only two fatty tumors within the abdomen that I have ever seen.

A CASE OF PELVIC ABSCESS OF LONG STANDING COMMUNICATING WITH THE RECTUM.

The patient was a single woman, thirty years old. Nineteen months before I saw her, she had been attacked with pains in the bowels, abdominal distention, chills and fever, etc., *i. e.*, peritonitis. The date of this illness was January, 1884. The catamenia ceased for fifteen months. On and after March, 1884, she remained at home and seldom went out, on account of pain and discomfort in the pubic region. In April, 1884, pus was discharged from the rectum in varying quantities nearly every day. In August, 1884, I advised hot vaginal and rectal douches, and these were continued twice a day for six months. She improved and was able to go out, and the catamenia reappeared in April, 1885, and continued for three months. In June, 1885, she began to grow worse and the pus increased in amount. On palpation, a mass of induration was felt in the pubic and iliac regions. On August 29th, 1885, the patient was etherized, and the rectum dilated. An opening from the rectum into the abscess was

found toward the left at a point as high as the finger would reach, and a uterine sound was bent and passed in, and the tip brought up against the abdominal parietes. The tip of the sound was cut down upon and brought out through the anterior abdominal parietes at a point in the centre of a triangle, of which the base was a line from the umbilicus to the left anterior spine of the ilium, and the apex of the pubes. Considerable offensive grumous pus came from the rectum. A rubber drainage tube was passed from the abdominal wound downward, and out through the rectum and anus. To the finger the upper opening seemed to pass through the mesentery or omentum. In the next few days considerable pus was discharged from both openings. For twenty-four hours the discharge from the anterior opening was slightly fecal. At the end of a week the tube was shortened and drawn downward to allow the upper opening to close, for fear of establishing a permanent fecal fistula, and the tube protruded from the rectum. The presence of the tube in the anus became very uncomfortable, and it was removed, the opening in the rectum being daily dilated with the finger. The patient went home September 16th. On November 28th, 1885, I saw her and she looked much better than at any previous time during my acquaintance with her. She had gained flesh and color. There was still some induration in the pelvis, and pus was occasionally discharged from the anus. She died of acute phthisis in July, 1886. The lungs were filled with tubercles, in some places softened, and at the apex of one was a cavity. The pelvic organs were matted together, so that the outlines of the generative organs were lost. The abscess looked as if it might have been tubercular, perhaps tubercular salpingitis, but neither ovary nor Fallopian tube could be made out, and only by the sense of touch and by incision could the body of the uterus be made out.

CASE OF LAPAROTOMY FOR PERITYPHLITIC ABSCESS.

[Reported in the New York *Medical Record*, Vol. CXIV, page 388.]

A boy, eleven years old, began to have pain in the abdomen, January 6th, 1886, and on January 9th I opened the abdomen on a line a couple of inches above and a little behind the anterior spine of the right ilium, about four inches from the umbilicus and six inches from the spinous processes of the vertebræ. I came down on the healthy bowel, and felt other coils, behind and below, containing fecal masses, or else enlarged glands, either mesenteric or lumbar. These organs were adherent to one another by a recent plastic process, and on poking about with my finger and carefully separating them, an abscess containing about an ounce or more of offensive (rotten egg) smelling pus was opened. So far as possible I kept the pus out of the peritoneal cavity, and after emptying the abscess, put in a double drainage tube. The abscess continued to discharge for about four weeks. The boy is now stout and strong.

CASES OF INTESTINAL OBSTRUCTION.

CASE I.—Is a sequel of Case LVI of the ovariotomies, and is reported in the Boston *Medical and Surgical Journal*, Vol. CVII, p. 413. The obstruction was caused by annular stricture of the sigmoid flexure of the rectum. An artificial anus was established at the lower end of the former scar in the pubic region. The patient was very comfortable for nearly a year, and died of general abdominal cancer in November, 1882.

CASE II.—Was unsuccessful. The obstruction was in the splenic curvature of the colon, and the artificial anus was made in the cæcum. The patient was very stout and the weather very hot, the mercury standing at 96°, Fahrenheit, in the shade at the time of the operation. (Case reported in the Boston *Medical and Surgical Journal*, Vol. CX, p. 146, February 14th, 1884.)

CASE III.—Was caused by a band from a Meckel's diverticulum in a young man of twenty-one, who had had a fecal umbilical fistula from birth. This case is described

in the *American Journal of the Medical Sciences*, Vol. CLXXV, p. 56. Suffice to say that I did not find the band, that I relieved the obstruction by an artificial anus at the umbilicus, and that the patient died at the end of a week.

CASE IV.—Has been described in the ovariotomy cases, CCXXII.

I can only say that if I had known at the time of operating on Case III what I know now, the patient would have had a much better chance for recovery. Prof. Fitz's paper has taught me much, and I have operated successfully for the cure of a diverticulum opening at the umbilicus, *i. e.*, the omphalo-mesenteric remains of fœtal life.

LAPAROTOMY FOR PYOSALPINX AND A CASE OF TUBO-OVARIAN CYST FILLED WITH PUS.

CASE I.—May 21st, 1886. A single woman, twenty-two years old, plump and well-nourished, has had much pain in the rectum and vagina since July, 1885. Catamenia normal. On examination a hard tumor was felt in the left pelvic region the size of an apple, and the tissues between the vagina and rectum were thickened. I operated May 21st, 1886. With some difficulty the left ovary was pulled up, and a cyst the size of an orange was tapped and emptied of purulent-looking fluid. The ovary and the tube, which was the size of the thumb and filled with pus, were then removed, and the pedicle tied many times on account of bleeding. The right ovary was fixed between the rectum and the uterus, and was dislodged with great difficulty. I could not detach the right tube. The right ovary was about the size of a plum, and filled with many abscesses. The disease was probably of gonorrhœal origin. Drainage was used. Recovery was rapid. A suture was discharged some months later. She is now (July, 1887) well and strong.

CASE II.—One of suppurating tubo-ovarian cyst, in a woman forty-four years old. Operation done December 6th, 1886. Some ascites. Tumor largely composed of a dilated tube with a communicating sacculated ovary. About three pounds of offensive pus removed, and the tumor was separated with much violence from the neighboring parts. No pedicle could be found. The operation lasted over two hours. Transfusion was employed, twenty ounces of warm solution of chloride of sodium being put into the basilic vein; this restored the pulse and color. She died, however, about twenty-six hours after the operation. Very possibly, the origin was tubercular pyosalpinx.

ANTISEPTIC

Carbolic Acid Spray used. Sixth ovariotomy the first antiseptic one. ien *not answered, or have aied, it is known that they are*

No.	Date of Operation.	Age.	Kind of Tumor.	Which Ovary.	Catamenia since.	Children before.	Children since.	Married or single.	Length of Incision.	Rupture.	Ligatures heard from.	Health since.	Fibroid seen at Operation.	Fibroid removed at Operation.	Twisted Pedicle.	Result.	Drainage.	Adhesions.	Death quest Da and ca
6	Feb. 27, 1877	16	Multilocular cyst	Left	Yes	No	No	Single	Long	Yes	No	Excellent	No	No	No	Recovered	No	Yes	Health good in 1887.
7	March 30, 1878	20	"	"	"	"	"	"	"	"	"	"	"	"	"	"	"	"	Living in 1887. Has an abdominal tumor.
8	Aug. 31, 1878	58	"	"	No	One. Abortion	"	Widow	"	"	"	"	"	"	"	"	"	"	
9	Sept. 17, 1878	60	"	"	"	No	"	"	"	Unknown	"	"	"	"	"	"	"	"	
10	Sept. 29, 1878	24	"	"	Yes	Two	One. Girl, Nov., 1879	Married	"	No	"	"	"	"	"	"	"	"	
11	Nov. 8, 1878	48	Unilocular cyst	Both ovaries, broad ligaments and fundus of uterus		No		Single	"							Died	"	"	Death, 14 hours after operation.
12	Dec. 28, 1878	47	Papilloma	Right	No	One	No	Married	"	"	"	Poor	"	"	"	Recovered	"	"	Died, July, 1879. Cancer of lungs and abdomen.
13	Feb. 13, 1879	38	"	Left	"	No	"	"	"	"	"	"	"	"	"	"	"	"	Died, Sept., 1879. Cancer of abdomen.
14	March 6, 1879	26	Multilocular cyst	"	Yes	One	"	"	"	Yes	"	Excellent	"	"	"	"	"	"	
15	May 11, 1879	50	"					"	"				"	"	"	Died	"	"	Died, 3½ hours after operation.
16	June 26, 1879	38	"	Right	No	No	"	Single	"	No	"	Excellent	"	"	"	Recovered	"	"	Never has menstruated since operation. Only one ovary removed.
17	July 16, 1879	41	"	"		"		"	"							Died	"	"	Died an hour after operation.

No.	Date of Operation.	Age.	Kind of Tumor.	Which Ovary.	Catamenia since.	Children before.	Children since.	Married or Single.	Length of Incision.	Rupture.	Ligatures heard from.	Health since.	Fibroid seen at Operation.	Fibroid removed at Operation.	Twisted Pedicle.	Result.	Drainage.	Adhesions.	Death subsequently. Date and cause of.
18	Oct 1, 1879	41	Unilocular cyst	Right				Married	Long							Died	No	No	Died of hemorrhage, 10 hours after operation. Up to this time had used catgut ligatures, never have done so since.
19	Nov. 26, 1879	24	Mult. and dermoid	Both	Yes. Irregular and painful for 2 years, and then ceased	No	No	Single	"	Yes	No	Excellent	No	No	No	Recovered	"	"	
20	Dec. 21, 1879	33	"	Left	Yes	"	"	"	"	"	"	"	"	"	"	"	"	Yes	
21	Feb. 8, 1880	39	"	Right	"	Two. (Twins)	"	Married	"	"	"	"	"	"	"	"	"	No	
22	March 7, 1880	28	Unilocular cyst	Left. Broad ligament	"	Four	"	"	Short	No	"	Good	"	"	"	"	"	"	
23	March 23, 1880	37	Multilocular cyst	Left	No	Three	"	"	Long	"	"	"	Yes	"	"	"	"	Yes	Catamenia have never appeared since.
24	April 1, 1880	18	Unilocular cyst	"	Yes	No	"	Single	"	"	"	"	No	"	"	"	"	"	
25	April 17, 1880	48	"	"	No	Three	"	Married	Short	"	"	"	Yes	"	"	"	"	"	
26	April 29, 1880	58	Multilocular cyst	"	"	No	"	Single	"	"	"	"	"	"	"	"	"	"	
27	May 18, 1880	38	"	"	Yes	One	"	Married	Long	"	"	"	"	"	"	"	"	No	
28	July 10, 1880	57	Unilocular cyst	"	No	Five	"	"	"	"	"	"	"	"	"	"	"	"	
29	July 15, 1880	47	"	Left. Broad ligament	"	No	"	Single	"	"	"	"	"	"	"	"	"	"	Died, in 1881, of cancer of stomach.
30	July 31, 1880	47	Multilocular cyst	Right	"	"	"	Married	"	Unknown	"	"	No	"	"	"	"	Yes	Died in 1885.
31	Aug. 1, 1880	30	Dermoid cyst	Left	Yes	One	One. Girl, Oct., 1881		"	No	"	"	"	"	"	"	"	No	
32	Aug. 21, 1880	29	Multilocular cyst		"	Two	No	Married	"	"	"	"	"	"	"	"	"	Yes	

No.	Date of Operation.	Age.	Kind of Tumor.	Which Ovary.	Catamenia since.	Children before.	Children since.	Married or single.	Length of Incision.	Rupture.	Ligatures heard from.	Health since.	Fibroid seen at Operation.	Fibroid removed at Operation.	Twisted Pedicle.	Result.	Drainage.	Adhesions.	Death subsequently. Date and cause of.
33	Aug. 22, 1880	29	Multilocular cyst		Yes	No	No	Married	Long	No	No	Good	No	No	No	Recovered	No	Yes	40 ozs. serum removed from left thoracic cavity, four days after operation.
34	Sept. 1, 1880	46	"	Right		"		"	"				Yes	"	"	Died	"	"	Died, four days after operation.
35	Sept. 2, 1880	47	"	"	No	"	"	"	"	"	"	"	"	"	"	Recovered	"	"	Died, June, 1881. Cancer of abdomen.
36	Sept. 7, 1880	27	Papillomatous cyst	Left				Single	"				"	"	"	Died	"	"	Died, soon after operation.
37	Sept. 23, 1880	33	Multilocular cyst	"	Yes	Six	One. Girl, 1881	Married	3 inches	No answer	Not known	"	No	"	"		"		Has not answered.
38	Oct. 2, 1880	48	"	Right	No	Not answered		Single	3½ "	Not known	"	Not known	Not known	"	"	Recovered	"	"	
39	Oct. 6, 1880	38	Unilocular cyst	"	Yes. Irregular	Three		Widow	5 "	No	No	Good	No	"	"	"	"	"	
40	Oct. 24, 1880	45	"	Left	No	No	No	Married	2½ "	"	"	"	"	"	"	"	"	No	
41	Nov. 6, 1880	31	Multilocular dermoid	"	First two years, yes Irregular and scanty since	"	"	Widow	3 "	"	"	"	"	"	"	"	"	"	
42	Nov. 18, 1880	31	Multilocular cyst	Right	Not answered			Single	6 "	Not known	Not known		"	"	"	"	"	Yes	
43	Nov. 28, 1880	52	"	"	Has not answered	No		Widow	3½ "	"	"		"	"	"	"	"	"	Died, Nov., 1881. Asthma and heart disease. At autopsy, no ligature found, no pedicle, only pigment, where vessels had been.
44	Dec. 1, 1880	37	"	"	Died of operation. Diseased kidneys	"		Married	6 "				Yes	"	"	Died	"	"	

No.	Date of Operation.	Age.	Kind of Tumor.	Which Ovary.	Catamenia since.	Children before.	Children since.	Married or Single.	Length of Incision.	Rupture.	Ligatures heard from.	Health since.	Fibroid seen at Operation.	Fibroid removed at Operation.	Twisted Pedicle.	Result.	Drainage.	Adhesions.	Death subsequently. Date and cause of.
45	Dec. 18, 1880	54	Multilocular cyst	Left	Died, two weeks after operation. Peritonitis and erysipelas of face	No		Single	Long				No	No	No	Died	No	Yes	Facial erysipelas on fourteenth day.
46	Dec. 21, 1880	38	"	"		"		"	3 inches	No	No		"		"	Recovered	"	No	
47	Jan. 6, 1881	26	"	Right	Yes	"	No	"	5 "	"	"	Good	"		"	"	"	"	
48	Jan. 26, 1881	38	"	Left	Not	answered	Afterward married	"	5 "	"		Presumed to be good	"		"	"	"	"	
49	Jan. 27, 1881	63	Mult. and dermoid	"	No	Three		Widow		"	"	Poor	"			"	"	Yes	Died, May, 1881
50	April 5, 1881	40	Solid. Dermoid	Right	Not	answered		Single	Long	"	"	Better than before, except for heart disease	Yes	Yes	"	"	"	No	Died, 1884. Heart disease.
51	April 14, 1881	40	Multilocular cyst	Left	"	"		Married	Short	"	"		No	No	"	"	"	"	
52	April 16, 1881	42	Mult. and semi-solid		Died of operation			"	Long							Died	"	Yes	Died of exhaustion on third day.
53	April 17, 1881	29	Multilocular cyst	Both	No	Four	No	"	"	"	"	Good	"	"	"	Recovered	"	"	
54	May 5, 1881	34	"	Right	Yes	Three	"	"	4 inches	"	"	Not good,—fair	"	"	"	"	"	No	
55	May 22, 1881	39	"	"	No	No			5 "		"	Good	"	"	"	"	"	"	Died, Jan., 1886.
56	May 26, 1881	42	"	"	"	Two	"	"	7 "	"	"	Poor	"	"	"	"	"	Yes	Died, Nov., 1882, of cancer. An artificial anus was made in 1881, Dec. 21, on account of cancer of rectum. Passed a comfortable summer.
57	June 1, 1881	40	Unilocular cyst	Left	Yes	"	"	Widow. Married again	4½ "	"	"	Good	"	"	"	"	"	No	

No.	Date of Operation.	Age.	Kind of Tumor.	Which Ovary.	Catamenia since.	Children before.	Children since.	Married or Single.	Length of Incision.	Rupture.	Ligatures heard from.	Health since.	Fibroid seen at Operation.	Fibroid removed at Operation.	Twisted Pedicle.	Result.	Drainage.	Adhesions.	Death subsequently. Date and cause of.
58	June 9, 1881	42	Cyst	Left	No	Two	No	Divorced. Married again, June, 1885	Short	No	No	Good	No	No	No	Recovered	No	No	
59	June 11, 1881	51	Multilocular cyst	"	"	"	"	Married	4 inches	"	"	"	"	"	"	"	"	"	
60	June 21, 1881	18	Unilocular cyst	"	Yes	No	"	Single	4 "	"	"	"	"	"	"	"	"	Yes	
61	June 30, 1881	55	Sarcoma	"	No	"	"	Widow	6 "	"	"	Poor	"	"	"	"	"	No	Died. Cancer of stomach. Autumn.
62	July 7, 1881	30	Unilocular cyst	Right, and broad ligament	Yes	"	"	Single	5 "	"	"	Good	"	"	"	"	"	"	
63	July 11, 1881	23	Multilocular cyst	Left	"	"	Not answered	Married	4 "			"	"	"	"	"	"	"	
64	July 25, 1881	14	"	Right	"	"	One. Girl	Single	3 "	"	"	"	"	"	"	"	"	"	
65	July 27, 1881	49	"	Both	No	"	No	"	2½ "	"	"	"	Yes	"	"	"	"	"	
66	Sept. 1, 1881	24	Unilocular cyst. Broad ligament	Left. Broad ligament	Not answered	"		"	2½ "			"	No	"	"	"	"	"	
67	Sept. 8, 1881	25	Dermoid cyst	Left		"	"	"								Died, 5th day, acute mania	"	"	
68	Sept. 11, 1881	41	Multilocular cyst	"	Yes	Three	"	Married	5 "	"	"	"	"	"	"	Recovered	"	Yes	
69	Sept. 18, 1881	51	"	"	No	No	"	Widow	3½ "	Yes	"	"	Yes	"	"	"	"	"	Died, autumn, 1885, of apoplexy.
70	Sept. 27, 1881	60	"	"	"	Five	"	Married	5 "	No	"	Poor, from paralysis	No	"	"	"	"	No	
71	Oct. 4, 1881	45	"	"	Yes, until 1882, a few times	Eight	"	"	7 "	"	"	Good	"	"	"	"	"	Yes	
72	Oct. 5, 1881	45	Dermoid cyst	Right	Yes	No	"	Widow	6 "	"	"	"	"	"	"	"	"	No	Bladder opened at operation, sewed up, never any trouble.

No.	Date of Operation.	Age.	Kind of Tumor.	Which Ovary.	Catamenia since.	Children before.	Children since.	Married or Single.	Length of Incision.	Rupture.	Ligatures heard from.	Health since.	Fibroid seen at Operation.	Fibroid removed at Operation.	Twisted Pedicle.	Result.	Drainage.	Adhesions.	Death subsequently. Date and cause of.
73	Oct. 24, 1881	57	Multilocular cyst. Left ovary	Left	No	No	No	Single	Long	No	No	Good	No	No	No	Recovered	No	Yes	
74	Oct. 29, 1881	48	Multilocular cyst	Right	"	One	"	Married	4 inches	Yes	"	"	"	"	"	"	"	"	
75	Nov. 15, 1881	47	"	"	"	No	"	Widow	3½ "	No	"	"	"	"	"	"	"	No	
76	Nov. 19, 1881	73	"	"	"	Two	"	"	6 "	Yes	"	"	"	"	"	"	"	Yes	
77	Dec. 1, 1881	52	Colloid burst dermoid cyst	"	"	No	"	Married	Long	No	"	Good and poor. There was a sinus from upper part of wound, through bladder, and colloid material was always passing	"	"	"	"	"	"	Died, 1885, of cancer, etc. Stone removed from bladder, Oct., 1882. Nucleus, a hair from cyst.
78	Dec. 18, 1881	30	Multilocular cyst	Both				Single	"				"	"	"	Died of operation, three days	"	"	
79	Jan. 30, 1882	27	Dermoid cyst. Right ovary	Right	Yes	Four	"	Married	"	"	"	Fair	"	"	"	Recovered	"	"	
80	March 14, 1882	43	Fibro-sarcoma	"				Single	"				"	"	"	Died of operation	"	"	
81	March 19, 1882	50	Multilocular cyst	"	Yes, for six months	Three	"	Married	5 inches	Yes	"	Good for a time, then poor	"	"	"	Recovered	"	No	
82	March 20, 1882	45	"	"	No	One	"	Widow	Long	No	"	Good	"	"	"	"	"	Yes	
83	March 25, 1882	45	"	Left	Yes, till Jan., 1883	Yes	"	Married	"	"	"	"	"	"	"	"	"	No	
84	April 3, 1882	25	Unilocular cyst	Right	Yes	One	Two. 1883, female. 1885, male	"	4 inches	Yes	"	"	"	"	"	"	"	Yes	
85	April 4, 1882	32	Multilocular cyst	"	"	No	No	Single	4 "	No	"	"	"	"	"	"	"	No	
86	April 10, 1882	15	Dermoid cyst	Left	"	"	"	"	3 "	"	"	"	"	"	"	"	"	"	

No.	Date of Operation.	Age.	Kind of Tumor.	Which Ovary.	Catamenia since.	Children before.	Children since.	Married or Single.	Length of cision.	Rupture.		Health				Result.	Draining	Adhesio	De
87	April 12, 1882	32	Multilocular cyst.	Right	Yes	Three	Two. Nov., 1884, boy. Dec., 1886, girl	Married	Long	No	No	Good	No	No	No	Recovered	No	Yes	
88	April 19, 1882	23	Unilocular cyst	Right. Broad ligament	"	No	No	Single. Married, Nov., 1886	Short	"	"	"	"	"	"	"	"	No	
89	April 20, 1882	28	Multilocular cyst	Right	"	"	"	Single	Long	"	"	Died	"	"	"	"	"	Yes	Died of cancer of cicatrix and abdomen, June, '84.
90	April 22, 1882	59	"	Left	No	Four	"	Married	"	"	"	Good	"	"	"	"	"	No	
91	April 22, 1882	39	Cyst		Died soon after operation	One			"							Died, April 26	"	Yes	
92	April 23, 1882	48	Multilocular cyst	Right	No	Three	"	"	4 inches	Yes	"	"	"	"	"	Recovered	"	No	
93	April 29, 1882	60	"	Left	"	No	"	"	Long	"	"	"	"	"	"	"	"	Yes	
94	May 5, 1882	47	Unilocular cyst	Left. Broad ligament	"	Nine, and seven miscarriages.	"	"	Short	"	"	"	"	"	"	"	"	No	
95	May 13, 1882	41	"	Left	Died	One										Died after operation	Yes	Yes	
96	June 5, 1882	64	Multilocular cyst	Right	No	"	"	"	4½ inches	No	"	Poor	Yes	"	"	Recovered	No	"	Died of cancer of abdomen, Jan., '84.
97	June 12, 1882	24	Cyst	Left	Yes	No		Single	"	"	"	Good	No	"	"	"	"	No	
98	June 14, 1882	51	Multilocular cyst	"	No	Seven	"	Married	Short	Yes	"	"	"	"	"	"	"	"	
99	June 15, 1882	14	Dermoid cyst	"	Yes	No	"	Single	"	No	"	"	"	"	"	"	"	"	
100	June 19, 1882	42	Unilocular cyst	"	"	"	"	"	"	"	"	"	"	"	"	"	"	"	
101	June 22, 1882	22	Dermoid cyst	"		Has not answered		"					"	"	"	"	"	"	
102	June 27, 1882	37	Multilocular cyst	Right	"	Three	"	Married	Long	"	"	Pain in left side	"	"	"	"	"	Yes	
103	July 8, 1882	22	"	Left	"		One, female	"	Short	"	"	Good	"	"	"	"	"	"	

No.	Date of Operation.	Age.	Kind of Tumor.	Which Ovary.	Catamenia since.	Children before.	Children since.	Married or Single.	Length of Incision.	Rupture.	Ligatures heard from.	Health since.	Fibroid seen at Operation.	Fibroid removed at Operation.	Twisted Pedicle.	Result.	Drainage.	Adhesions.	Death subsequently. Date and cause of.
104	July 10, 1882	29	Multilocular cyst	Left	Not heard from	One		Married	Long	Unknown	No	Good, I suppose	No	No	No	Recovered	No	Yes	
105	Aug. 16, 1882	24	Unilocular cyst	Left. Broad ligament	Yes	No	No	Single	4 inches	No	"	Good	"	"	"	"	"	No	
106	Aug. 28, 1882	38	Multilocular cyst	Right				Married			"		"	"	"	Died on 5th day	"	Yes	
107	Aug. 31, 1882	28	"	Right dermoid	No	"	"	Single	Long	"	"	Poor	Yes	"	"	Recovered	"	"	Died of cancer, Feb., 1883.
108	Sept. 7, 1882	24	"	Right	Yes	"	"	"	Short	"	"	Good	No	"	"	"	"	No	Died of disease of kidneys, Oct., 1886.
109	Sept. 14, 1882	56	"	Left	No	"	"	"	Long	"	"	"	"	"	"	"	"	Yes	
110	Sept. 19, 1882	33	"	"	Yes	"	"	Married	"	"	"	"	"	"	"	"	"	"	
111	Sept. 26, 1882	42	"	Right	"	Three	"	"	Short	"	"	"	Yes	"	"	"	"	No	
112	Sept. 26, 1882	20	Unilocular of both broad ligaments	Both ligaments	Died, 3d day	No		Single	"		"	Died	No	"	"	Died	"	Yes	
113	Sept. 28, 1882	32	Multilocular cyst	Right	Yes	Two	"	Married	"	"	"	Good	"	"	"	Recovered	"	No	
114	Oct. 4, 1882	49	"	"	No	Six	"	"	Long	"	"	"	"	"	"	"	"	Yes	
115	Oct. 11, 1882	31	Broad ligament cyst	Left	Yes	No	"	Single	Short	"	"	Poor	"	"	"	"	"	No	
116	Oct. 26, 1882	64	Multilocular cyst	Right	No	One	"	Widow	Long	"	"	Good	"	"	"	"	"	"	
117	Nov. 8, 1882	21	"	"		No		Single					"	"	"	Died on 5th day	"	"	
118	Nov. 10, 1882	68	Multilocular cyst. Almost solid	"	"	One	"	Widow	"	Yes	"	"	"	"	"	Recovered	"	Yes, slight	Operated on again successfully, Feb., 1887.
119	Nov. 11, 1882	43	Papilloma	Both	"	No	"	Single	"	No	"	Fair	"	"	"	"	"	Yes	
120	Nov. 22, 1882	29	Unilocular cyst	Right	Yes	"	"	"	Short	"	"	Good	"	"	"	"	"	No	
121	Nov. 23, 1882	19	"	"	"	"	"	"	"	"	Good	"	"	"	"	"	"	"	
122	Dec. 16, 1882	49	Multilocular cyst	"	"	Three	"	Married	"	'	No	"	"	"	"	"	"	"	

No.	Date of Operation.	Age.	Kind of Tumor.	Which Ovary.	Catamenia since.	Children before.	Children since.	Married or Single.	Length of Incision.	Rupture.	Ligatures heard from.	Health since.	Fibroid seen at Operation.	Fibroid removed at Operation.	Twisted Pedicle.	Result.	Drainage.	Adhesions.	Death subsequently. Date and cause of.
123	Dec. 30, 1882	27	Multilocular cyst.	Left	Yes	No	No	Single	Short	No	No	Good	No	No	No	Recovered	No	No	
124	Jan. 1, 1883	53	"	"	No	"	"	"	Long	"	"	"	"	"	"	"	"	Yes	
125	Jan. 8, 1883	35	"	Both	Yes, once, 10 mos. after operation	One	"	Married	"	Yes	"	"	Yes	"	"	"	"	"	
126	Jan. 29, 1883	50	Dermoid cyst	Right	Died, 10 hours	Several		Widow	"							Died after operation. Had septicæmia from tapping, 5 weeks before		"	
127	Jan. 31, 1883	25	Multilocular cyst	Left	Yes	One	"	Married	"	"	"	"	No	"	"	Recovered	"	"	
128	Feb. 22, 1883	40	"	Both	No	"	"	"	"	No	"	"	Yes	"	"	"	"	"	Died May, 1884, pneumonia.
129	March 1, 1883	42	"	Right	"	Seven	"	"	"	"	"	"	"	"	"	"	"	"	
130	March 19, 1883	52	"	Left	"	No	"	Single	"	"	"	"	"	"	"	"	"	"	
131	March 27, 1883	32	"	Right	Yes	One	"	Married	Short	"	"	"	No	"	"	"	"	No	
132	April 4, 1883	49	Multilocular cyst. Dermoid.	Left	No	Three	"	"	"	Yes	"	"	Yes	"	"	"	"	Yes	
133	May 2, 1883	26	Cyst of left broad ligament		Yes	No	"	Single	"	"	"	"	No	"	"	"	"	No	
134	May 17, 1883	13	Multilocular cyst	"	"	"	"	"	"	No	"	"	"	"	"	"	"	"	
135	May 30, 1883	41	"	"	"	One	"	Married	"	Yes	"	"	"	"	"	"	"	Yes	
136	June 16, 1883	41	Papilloma	Both	No	Two	"	"	"	No	"	"	"	"	"	"	"	"	
137	June 19, 1883	23	Dermoid cyst	Right	Yes	No	"	Single. Married, Dec., 1884	Long	"	"	"	"	"	"	"	"	"	
138	July 3, 1883	24	Multilocular cyst	"	"	"	One, girl, Aug., 1886	Married	"	"	"	"	"	"	"	"	"	"	
139	July 12, 1883	39	"	Left	"	Two	No	Widow	Short	Yes	"	"	"	"	"	"	"	"	

No.	Date of Operation.	Age.	Kind of Tumor.	Which Ovary.	Catamenia since.	Children before.	Children since.	Married or Single.	Length of Incision.	Rupture.	Ligatures heard from.	Health since.	Fibroid seen at Operation.	Fibroid removed at Operation.	Twisted Pedicle.	Result.	Drainage.	Adhesions.	Death subsequently. Date and cause of.
140	July 20, 1883	19	Multilocular cyst	Right		No		Single	Short	Not heard from			No	No	No	Recovered	No	No	
141	Aug. 1, 1883	50	"	"	No	"	No	Widow	"	Yes	No	Good	"	"	"	"	"	Yes	
142	Aug. 4, 1883	31	"	"	Yes	During growth of cyst, she was pregnant and abortion was produced, and cyst tapped twice	Two. Easy labors. Aug., 1884. Dec., 1885. Both girls	Married	"	No	"	"	"	"	"	"	"	"	
143	Oct. 10, 1883	46	"	"	No	Five	No	"	"	Yes	"	"	Yes	"	"	"	"	"	
144	Oct. 12, 1883	26	Papilloma. Multilocular cyst	"	Yes	One	"	"	Long	No	Yes. Oct., 1885. Knot on pedicle came away unaltered	"	No	"	"	"	"	"	
145	Oct. 25, 1883	37	Dermoid cyst	Left	No	No	"	"	"	"	No	"	"	"	"	"	"	"	
146	Nov. 15, 1883	22	Multilocular cyst	"	Yes	Three	Two. Dec., 1884. Apr., 1886. Both girls	"	Short	"	"	Good, till 1884	"	"	"	"	"	"	Died, Sept., 1886, of consumption.
147	Nov. 21, 1883	27	Unilocular cyst	Left. Broad ligament and ovary	"	No	No	"	"	"	"	Good	"	"	"	"	"	No	
148	Dec. 3, 1883	39	Multilocular cyst	Left	6 times in 3 years	"	"	Single	"	Yes	"	"	"	"	"	"	"	"	
149	Dec. 26, 1883	73	Unilocular cyst	Right	No	Eight	"	Widow	"	No	"	"	"	"	"	"	"	Yes	
150	Jan. 16, 1884	25	"	R't. Broad ligament	Yes	No	"	Married	"	"	"	"	"	"	"	"	"	No	

No.	Date of Operation.	Age.	Kind of Tumor.	Which Ovary.	Catamenia since.	Children before.	Children since.	Married or Single.	Length of Incision.	Rupture.	Ligature heard fro	Health since	Fibroi at Op	F rem Op	Twisted Pedicle.	Result.	Drainage.	Adhesions.	Death subsequently. Date and cause of
151	Jan. 28, 1884	44	Unilocular cyst	Right broad ligament	Yes	Four	No	Married	Short	Yes	No	Good	No	No	No	Recovered	No	No	
152	Feb. 20, 1884	33	Multilocular cyst	Left ovary, right broad ligament	"	No	"	"	"	"	"	"	"	"	"	"	"	"	
153	March 8, 1884	66	"	Left	No	Two	"	"	Long	No	"	"	"	"	"	"	"	"	
154	March 24, 1884	61	Unilocular cyst	Left broad ligament	"	Five	"	"	Short	"	"	"	"	"	"	"	"	"	
155	April 2, 1884	24	"	Left	Yes	No	"	Single	"	"	"	"	"	"	"	"	"	"	
156	June 7, 1884	51	Multilocular cyst	Right	No	One	"	Married	Long	"	"	"	Yes	"	"	"	"	"	
157	June 7, 1884	55	"	"	"	No	"	"	Short	"	"	"	No	"	"	"	"	Yes	
158	June 9, 1884	22	Unilocular cyst	"	Yes	"	"	Single	"	"	"	"	"	"	"	"	"	No	
159	June 11, 1884	48	Multilocular cyst	"	"	One	"	Married	"	Yes	"	Good, except cystitis	"	"	"	"	"	"	
160	June 14, 1884	33	Unilocular cyst	"	"	No	"	"	"	No	"	Good	"	"	"	"	"	"	
161	June 23, 1884	52	Multilocular cyst	Left	No	"	"	"	Long	"	"	Fair	Yes	Yes 3¼ X 2½ X 1	"	"	"	"	
162	June 26, 1884	47	Unilocular cyst	Right	"	Eight	"	"	Short	"	"	Good	No	No	"	"	"	"	
163	July 2, 1884	48	Dermoid cyst	"	Yes,—irregular	No	"	"	"	Yes	"	"	"	"	"	"	"	"	
164	July 5, 1884	36	Multilocular cyst	"	Yes*	"	"	Single	"	No	"	Good, till 1885	"	"	"	"	"	"	Died, June, 1886, I think of cancer of abdomen.
165	July 10, 1884	24	Multilocular cyst, communicating with rectum	"	Died	"		"	"	Died						Died soon after operation. 2d day	"	Yes	
166	July 12, 1884	35	Multilocular cyst	Both	Until May, 1885. Not since	"	"	Married	"	No	"	Not very good	"	"	"	Recovered	"	No	

No.	Date of Operation.	Age.	Kind of Tumor.	Which Ovary.	Catamenia since.	Children before.	Children since.	Married or Single.	Length of Incision.	Rupture.	Ligatures heard from.	Health since.	Fibroid seen at Operation.	Fibroid removed at Operation.	Twisted Pedicle.	Result.	Drainage.	Adhesions.	Death subsequently. Date and cause of.
167	July 16, 1884	63	Cancer of right ovary and peritoneal organs	Right	Died	No	Died	Single	Long				No	No	No	Died on 3d day	No	Yes	
168	Aug. 18, 1884	58	Multilocular cyst	"	No	Five	No	Married	Short	No	No	Good	"	"	"	Recovered	"	No	
169	Aug. 18, 1884	52	"	"	"	"	"	"	"	"	"	"	"	"	"	"	"	"	
170	Sept. 11, 1884	28	"	Left	Died	No	Died	"	Long	Died			"	"	"	Died	"	Yes	
171	Oct. 9, 1884	56	"	Right	No	Three	No	Widow	Short	No	"	"	"	"	"	Recovered	"	No	
172	Nov. 5, 1884	37	"	Left	Yes	Two	One. Boy, June, 1886	Married	"	"	"	"	"	"	"	"	"	"	
173	Nov. 13, 1884	23	Unilocular cyst	Left ovary. Left broad ligament	"	"	No	"	"	"	"	"	"	"	"	"	"	"	
174	Nov. 27, 1884	27	Multilocular cyst	Left	"	No	"	Single	"	"	"	Poor. Cough	"	"	"	"	"	"	
175	Dec. 3, 1884	67	Unilocular cyst	"	No	"	"	Married	"	Yes	"	Good	"	"	"	"	"	Yes	
176	Dec. 4, 1884	42	Papilloma	Both	"	Two	"	Widow	Medium	No	"	"	"	"	"	"	"	"	
177	Dec. 5, 1884	40	Multilocular cyst	Left	Yes	No	"	Married	"	"	"	"	"	"	"	"	"	"	
178	Dec. 13, 1884	23	Unilocular cyst	Left broad ligament	"	"	"	"	Short	"	"	"	"	"	"	"	"	No	
179	Dec. 20, 1884	41	Multilocular cyst	Left	"	Six	"	"	"	"	"	"	"	"	"	"	"	Yes	
180	Dec. 27, 1884	37	Papilloma	Both	No	No	"	"	"	"	"	"	"	"	"	"	"	"	
181	Dec. 31, 1884	55	Multilocular cyst, dermoid	Left	"	One	"	Widow	"	"	"	"	Yes	"	"	"	"	"	
182	Jan. 17, 1885	50	Multilocular cyst	Right	Yes, for 18 mos.	Three	"	Married	"	"	"	"	No	"	"	"	"	"	
183	Jan. 20, 1885	54	"	"	"	Two	"	"	"	"	"	"	"	"	"	"	"	"	
184	Jan. 24, 1885	34	Unilocular cyst	"	Yes	Three	"	"	"	"	"	"	"	"	"	"	"	No	

No.	Date of Operation.	Age.	Kind of Tumor.	Which Ovary.	Catamenia since.	Children before.	Children since.	Married or Single.	Length of Incision.	Ruptu	[illegible]	H	[illegible]	[illegible]	[illegible]	[illegible]	Drain	Adh
185	March 2, 1885	46	Multilocular cyst	Right	Died	No		Single	Long				No	No	No	Died	Drainage tube inserted on day after operation	
186	March 3, 1885	63	"	"	No	"	No	"	Short	No	No	Good	"	"	"	Recovered	No	No
187	March 28, 1885	44	"	"		"		Married	Long				"	"	"	Died	"	Universal
188	March 31, 1885	34	"	Left		"		Single	"				"	"	"	"	"	Yes
189	April 8, 1885	24	Unilocular cyst, right broad ligament		Yes	"	"	Married	Short	"	"	Very good	"	"	"	Recovered	"	No
190	April 9, 1885	73	Multilocular cyst	"	Died	Ten		"	"						"	Died	Yes	Yes
191	April 12, 1885	26	"	Right	Yes	No	"	"	"	"	"	Good	"	"	"	Recovered	No	"
192	April 16, 1885	33	Unilocular cyst, right broad ligament		"	"	"	Single	"	"	"	"	"	"	"	"	"	No
193	April 18, 1885	64	Multilocular cyst	Left	No	"	"	Married	"	"	"	Poor	"	"	"	"	"	Yes
194	April 24, 1885	46	"	"	"	One	"	"	Long	Wound not healed	Yes. June, '85. Jan., '86. May, '86. Oct., '86 Dec., '86. Jan., '87.	Good	"	"	"	"	Yes	"
195	April 29, 1885	30	"	Both	"	No	"	"	"	No	No	"	Yes	Yes	"	"	"	"
196	April 21, 1885	34	Fibro-cystic tumor of right ovary	Right	Yes	"	"	"	"	"	"	"	No	No	"	"	No	No
197	May 1, 1885	27	Unilocular cyst	Left	Not answered	"		Single	"				Yes	"	"	"	"	Yes

No.	Date of Operation.	Age.	Kind of Tumor.	Which Ovary.	Catamenia since.	Children before.	Children since.	Married or Single.	Length of Incision.	Rupture.	Ligatures heard from.	Health since.	Fibroid seen at Operation.	Fibroid removed at Operation.	Twisted Pedicle.	Result.	Drainage.	Adhesions.	Death subsequently. Date and cause of.
198	May 4, 1885	39	Multilocular cyst	Left	No	No	No	Single	Short	No	No	Good for six weeks. Died of cancer	No	No	No	Recovered	No	No	Died of general abdominal cancer and also obstruction in region of pancreas.—Cancer in scar.—October 16, 1885.
199	May 6, 1885	36	"	"	Yes	"	"	"	"	Yes	"	Good	"	"	"	"	"	"	
200	May 11, 1885	35	Multilocular cyst, and fibroid of uterus	Both	Five times, slight and decreasing	"	"	Married	Long	No	"	"	Yes	Yes	"	"	Yes	Yes	
201	June 2, 1885	59	Unilocular cyst	Left	No	Three	"	"	Short	"	"	Good. Now another abdominal tumor	No	No	"	"	No	No	
202	June 2, 1885	45	Multilocular cysts	Both	"	Two	"	"	"	"	"	Good	"	"	"	"	"	Yes	
203	June 5, 1885	40	Multilocular cyst	Right	"	Yes	"	"	"	"	"	"	"	"	"	"	"	No	
204	June 13, 1885	51	Unilocular cyst	Left	"	No	"	Single	"	"	"	"	"	"	"	"	"	"	
205	June 20, 1885	35	Multilocular cyst, and fibroid of uterus	"	Yes	"	"	"	"	"	"	"	Yes	Yes	"	"	"	"	
206	July 2, 1885	44	Multilocular cyst	Right	"	One	"	Married	Medium	"	"	"	No	No	Yes	"	Yes	Yes	
207	July 15, 1885	47	Multilocular cyst, and fibroid of uterus	Left	Yes, irregular	"	"	"	Short	"	"	"	Yes	Yes	No	"	No	No	
208	July 17, 1885	61	Fibro-cyst, right. Multilocular cyst, left	Both	No	No	"	"	Long	"	"	"	No	No	"	"	"	Yes	
209	July 22, 1885	49	Multilocular cysts	"	"	Four	"	"	Short	Yes	"	"	"	"	"	"	"	"	

No.	Date of Operation.	Age.	Kind of Tumor.	Which Ovary.	Catamenia since.	Children before.	Children since.	Married or Single.	Length of Incision.	Rupture.	Ligatures heard from.	Health since.	Fibroid seen at Operation.	Fibroid removed at Operation.	Twisted Pedicle.	Result.	Drainage.	Adhesions.	Death subsequently. Date and cause of.
210	Sept. 7, 1885	49	Papilloma	Both	No	No	No	Married	Long. Bladder wounded and sewed up with 12 sutures	No	No	Good	No	No	No	Recovered	Yes	Yes	A remarkable case.
211	Sept. 14, 1885	40	Multilocular cyst	Left	"	Two	"	"	Long	"	"	"	"	"	"	"	No	"	
212	Sept. 30, 1885	23	Papilloma. Left broad ligament	"	Yes	No	"	Single	Short	"	"	"	"	"	"	"	"	No	
213	Oct. 5, 1885	63	Multilocular cyst	"	No	Two	"	Married	"	"	"		"	"	Yes	"	"	Yes	Died, Feb. 7, 1886, probably of cancer.
214	Oct. 8, 1885	18	Dermoid cyst	"	"	No	"	Single	Long	"	"	Poor, died	"	"	No	"	Yes	"	
215	Oct. 23, 1885	33	Multilocular cyst	Both	Died			"	Short				"	"	"	Died on 5th day	No	No	
216	Oct. 24, 1885	19	Unilocular cyst	Right broad ligament	"	"		"	"				"	"	"	"	"	"	
217	Oct. 17, 1885	39	Multilocular cysts	Both	No	Three	"	Married	Long	"	"	Good	"	"	"	Recovered	Yes	Yes	
218	Oct. 22, 1885	30	Multilocular cyst	Right	Yes	No	One, Nov., 1886, Girl	"	Short	"	"	"	"	"	"	"	No	No	
219	Oct. 26, 1885	20	"	"	"	One	No	"	"	"	"	"	"	"	"	"	"	Yes	
220	Nov. 9, 1885	30	"	"	"	"	"	"	"	"	"	"	"	"	"	"	"	No	
221	Nov. 23, 1885	38	Dermoid cyst. Multilocular	"	"	No	"	Single	Long	Yes	"	"	"	"	"	"	"	"	
222	Dec. 11, 1885	61	Multilocular cyst	Left	No	Ten	"	Married	"	No	"	Good till death from intestinal obstruction	"	"	"	"	"	Yes	Died, March 22, 1886, of intestinal obstruction. Strangulation through adhesions at cicatrix.
223	Dec. 9, 1885	43	Unilocular cyst	Right broad ligament	Yes	Four	"	"	Short	"	"	Good	"	"	"	"	"	No	

	Date at	A	K of T	Whic	Cata si	Child fo	Chl si	Marr Sir	Lengt cl	Ru	Lig heard	Healt	Fibro at Op	Fil remo Oper	Tw Pe	Re	Dra	Adh	Death que D and c
224	Jan. 9, 1886	38	Unilocular cyst	Right broad ligament	Yes	Two	No	Married	Short	No	No	Good	No	No	No	Recovered	No	No	
225	Jan. 14, 1886	21	Round-celled sarcoma	Right	Died	No		Single	Long				"	"	"	Died	"	"	
226	Jan. 16, 1886	51	Multilocular cyst. 86 lbs.	"	No	Two	"	Married	Short	"	"	"	"	"	"	Recovered	"	Yes	
227	Jan. 19, 1886	33	Multilocular cyst	Left	"	Four	"	"	"	"	"	"	"	"	"	"	"	"	
228	Jan. 27, 1886	35	"	Right	"	"	One, Dec., 1886. Female	"	"	"	"	"	"	"	"	"	"	"	
229	Jan. 28, 1886	52	Multilocular cyst. Gangrenous, from twisted pedicle	Left	Died	No		"	"						Yes. Cyst dark cranberry or purple color	Died			In articulo mortis at time of operation.
230	Feb. 1, 1886	26	Papilloma. Both ovaries	Both	Has not answered	One. Miscarriage		"	"				"	"	No	Recovered	Yes	"	
231	Feb. 8, 1886. Another operation March 5, 1887. Dr. Utley	49	Multilocular cyst, with peritonitis	Left	Yes	No	No	Widow	"	"	"	Poor	"	"	Yes	"	No	"	
232	Feb. 13, 1886	19	Multilocular cyst	Right	"	"	"	Single	"	"	"	Good	"	"	No	"	"	No	
233	Feb. 15, 1886	48	"	"	No	Two	"	Married	Long	"	"	"	"	"	"	"	"	Yes	
234	Feb. 16, 1886	53	Papilloma	"	"	Four	"	"	Short	"	"	"	"	"	"	"	"	No	
235	March 4, 1886	24	Multilocular cyst	"	Yes	No	"	"	"	"	"	"	"	"	"	"	"	"	
236	March 30, 1886	25	Multilocular cyst. Ascites	"	"	"	"	Single	Long	"	Yes	"	"	"	"	"	"	"	
237	April 1, 1886	36	Multilocular cyst	"	"	"	"	Married	Short	"	No	"	"	"	"	"	"	"	
238	April 5, 1886	35	"	"	"	Two	"	"	"	"	"	"	"	"	Yes	"	"	"	

No.	Date of Operation.	Age.	Kind of Tumor.	Which Ovary.	Catamenia since.	Children before.	Children since.	Married or Single.	Length of Incision.	Rupture.	Ligatures heard from.	Health since.	Fibroid seen at Operation.	Fibroid removed at Operation.	Twisted Pedicle.	Result.	Drainage.	Adhesions.	Death subsequently. Date and cause of.
239	April 29, 1886	45	Papilloma	Both	Died	No		Single	Long				No	No	No	Died	No	Yes	Died. Obstructed intestine and peritonitis.
240	April 22, 1886	42	Multilocular cyst	"	No	Five	No	Married	"	No	No	Good	"	"	"	Recovered	"	Yes. Cancer (?) of omentum removed	
241	May 22, 1886	41	Dermoid cyst.	Right	Yes	No	"	Single	Short	"	"	"	"	"	"	"	"	Yes	
242	June 3, 1886	26	Multilocular cyst	Left	"	One	"	Married	"	"	"	"	"	"	"	"	"	No	
243	June 7, 1886	37	Unilocular cyst	Right	"	Two	"	"	"	"	"	"	Yes	"	"	"	"	"	
244	June 8, 1886	23	"	"	No	"	"	"	Long	Not healed	"	Poor		"	"	"	"	Yes	
245	June 9, 1886	22	Multilocular cyst. Piece of omentum removed	Left	Yes	Three	"	"	"	No	Yes	Good	"	"	"	"	"	"	
246	July 7, 1886	43	Multilocular cyst	Right	Has not answered	Ten	"	"	"	"	No	Presumed to be good	"	"	"	"	Yes	"	
247	July 8, 1886	44	"	"	Yes	No	"	Single	Short	"	"	Good	Yes	"	Yes	"	"	"	
248	July 9, 1886	56	"	Left	No	"	"	Widow	"	"	"	"	No	"	No	"	No	No	
249	July 27, 1886	34	Unilocular cyst	"	Yes	"	"	Single	"	"	"	"	"	"	"	"	"	"	
250	Sept. 4, 1886	23	Multilocular cyst	Right	"	"	"	"	"	"	"	"	"	"	"	"	"	"	
251	Oct. 9, 1886	48	"	Right, and tumor in front of bladder	Died of tetanus	Two		Married	"				"	"	"	Died on 5th day, of tetanus			
252	Oct. 16, 1886	60	Unilocular cyst. 111½ lbs	Left	No	No	"	Single	"	"	"	Fair	"	"	"	Recovered	"	Yes	Died, April, 1887.
253	Nov. 10, 1886	43	Multilocular cyst, dermoid, gelatinous	"	Not heard from	"	"	"	Long	"	"	Good	Yes	"	Yes	"	"	"	
254	Nov. 20, 1886	15	Multilocular cyst	"	"	"	"	"	Short	"	"	"	No	"	"	"	"	No	

	Date al		E of T	Whic	Cata si	Child fo	Chi si	Mar Si	Lengt cli	Bu	Lig heard	Healt	Fibro at Op	Fil remo Oper	Tw Pe	Re	Dral	Adhe	Death que Di and ca
255	Dec. 15, 1886	31	Broad ligament	Neither	Yes	No	No	Married	Short	No	No	Good	No	No	No	Recovered	No	No	
256	Jan. 10, 1887	32	Multilocular cyst	Left	"	Three	"	"	Long	"	"	"	"	"	"	"	"	Yes	
257	Jan. 18, 1887	45	Multilocular cysts	Both	Died	One		"	Short							Died			
258	Jan. 29, 1887	22	Multilocular cyst	Left		No		Single	Long	"	"	"	"	"	"	Recovered	"	No	
259	Feb. 3, 1887	37	"	"	Yes	"	"	Married	Short	"	"	"	"	"	"	"	"	"	
260	Feb. 9, 1887	72	"	Left, and uterus also removed	No	One	"	Widow	Long	"	"	"	"	"	"	"	Yes	Yes	
261	Feb. 17, 1887	30	"	Both	"	No	"	Married	"	"	"	"	"	"	"	"	No	No	
262	Feb. 19, 1887	36	"	Left, and uterus also removed	"	"	"	Single	"	"	"	"	"	"	Yes	"	Yes	Yes	
263	Feb. 23, 1887	48	Unilocular cyst	Left broad ligament	"	Two	"	Married	Short	"	"	"	"	"	No	"	No	No	
264	Feb. 25, 1887	57	Multilocular cysts	Both	"	Four	"	"	Long	"			"	"	"	Died	Yes	Yes	
265	Feb. 26, 1887	23	Unilocular cyst	Left	Died	Three		"	Short				"	"	"	"	No	"	
266	Feb. 27, 1887	61	"	"	No	Six	"	"	Long	"	"	"	"	"	"	Recovered	"	"	
267	March 28, 1887	48	Dermoid cyst	Right	"	Two	"	"	"	"	"	"	"	"	"	"	"	"	
268	March 30, 1887	55	Multilocular cyst	Left		Four		"	"				"	"	"	Died	Yes	"	
269	April 11, 1887	40	"	Both	"	No	"	Single	Short	"	"	"	"	"	"	Recovered	No	"	
270	April 14, 1887	23	"	Left	Yes	"	"	Married	"	"	"	"	"	"	"	"	"	No	
271	April 18, 1887	60	Multilocular cysts	Both, and broad ligament	No	Five	"	Widow	"	"	"	"	"	"	"	"	"	Yes	

No.	Date of Operation.	Age.	Kind of Tumor.	Which Ovary.	Catamenia since.	Children before.	Children since.	Married or Single.				He				I	Dri.	Adhes	
272	April 19, 1887	44	Multilocular cyst	Right	Yes	Three	No	Married	Short	No	No	Good	No	No	No	Recovered	No	Yes	
273	May 2, 1887	40	"	"	Not heard from	One		"	"	"	"	"	"	"	"	"	"	"	
274	May 5, 1887	53	"	Left	No	No		Single	"	"	"	"	"	"	"	"	"	No	
275	May 10, 1887	33	Dermoid cyst	"	Yes	"		"	"	"	"		"	"	"	" (?)	Yes	Yes	Died, 16th day, of pulmonary embolism.
276	May 11, 1887	43	"	Right	No	One		Married	"	"	"	"	"	"	"	"	No	No	
277	May 12, 1887	55	"	"	"	Three		"	"	"	"	"	"	"	"	"	"	Yes	
278	May 16, 1887	33	Multilocular cyst	Left	Yes	No		Single	"	"	"	"	"	"	"	"	"	"	
279	May 20, 1887	28	"	Right	"	One		Married	"	"	"		"	"	"	" (?)	"	"	After nearly four weeks of normal pulse and temperature, she died suddenly, apparently of indigestion.
280	June 8, 1887.	53	Colloid tumors	Both	No	Eight		"	"	"	"	"	"	"	"	"	"	No	
281	June 30, 1887	35	Multilocular cyst	"	Yes	No		Single	"	"	"	"	"	"	"	"	"	"	
282	July 7, 1887	45	Papilloma	"		Three		Married	"	"	"	"	"	"	"	"	"	Yes	

HYSTERECTOMIES AND REMOVAL OF UTERINE TUMORS BY LAPAROTOMY.

No.	Age.	Date.	Kind of Tumor.	Catamenia since.	Children before.	Married or Single.	Incision.	Rupture.	Treatment of Pedicle.	Adhesions.	Health.	Cause of Death.	Drainage.	Weight of Tumor.	Result.	Remarks.
1	40	April 5, 1881	Myoma	Unknown	No	Single	Long	Unknown	Intra-peritoneal. Tied and cauterized	No	Good		No	1½ lbs.	Recovered	
2	22	May 11, 1882	Fibro-cyst and both ovaries cystic	Died	"	"	"		Intra-peritoneal. Tied and burnt	Yes	Died	Debility	"		Died	
3	18	April 11, 1883	Two-horned uterus filled with bloody fluid	No	"	"	"	No	Extra-peritoneal. Tied with silk and kept outside	No	Good		"	5 "	Recovered	
4	43	April 24, 1883	Fibro-myoma	Died	Six	Married	"		Extra-peritoneal. Clamp	Yes	Died	Septicæmia	"	"	Died	
5	35	Oct. 9, 1883	"	"	No	Single	"		Intra-peritoneal. Tied	"	"	"	"	8 "	"	
6	44	Nov. 3, 1883	"	"	"	Married	"		Intra-peritoneal	"	"	Shock	"	45 "	"	
7	36	June 12, 1884	"	No	"	"	"	Yes	Extra-peritoneal. Kœberle's serre-nœud	"	Good		"	2¼ "	Recovered	
8	52	June 23, 1884	Fibroid	"	"	"	"	No	Intra-peritoneal. Tied	No	"		"	1 lb.	"	
9	26	Aug. 6, 1884	Fibro-myoma	Died	"	Single	"		Extra-peritoneal. Kœberle's serre-nœud	"	Died	Septicæmia	"	7½ lbs.	Died	
10	26	Dec. 6, 1884	Fibro-myomatous cyst	Yes	"	"	"	"	Intra-peritoneal. Tied and burnt	Yes	Good		"	3½ "	Recovered	
11	34	Feb. 5, 1885	Fibro-myoma	Died	"	Married	"		Extra-peritoneal. Kœberle's serre-nœud	"	Died	Hemorrhage	"	8 "	Died	
12	50	Feb. 14, 1885	Myoma	"	"	Single	"		Intra-peritoneal. Ligature and cautery.	"	"	Peritonitis	"	50 "	"	
13	30	April 29, 1885	Fibro-myoma	No	"	Married	"	"	Extra-peritoneal. Kœberle's serre-nœud	"	Good		Yes	8 "	Recovered	
14	35	May 13, 1885	Fibroid. Ovaries cystic also	Irregular	"	"	"	"	Intra-peritoneal. Tied	No	"		"	Small	"	
15	35	June 20, 1885	Fibroid and an ovary cystic	Yes	"	Single	Short	"	"	"	"		No	"	"	
16	47	July 15, 1885	Fibroid	"	One	Married	Long	"	Extra-peritoneal. Kœberle's serre-nœud	"	"		Yes	¾ lb.	"	

No.	Age.	Date.	Kind of Tumor.	Catamenia since.	Children before.	Married or Single.	Incision.	Rupture.	Treatment of Pedicle.	Adhesions.	Health.	Cause of Death.	Drainage.	Weight of Tumor.	Result.	Remarks.
17	50	Sept. 18, 1885	Myxo-fibroma	No	No	Single	Long	No	Extra-peritoneal. Kœberle's serre-nœud	No	Good		No	5 lbs.	Recovered	
18	34	Jan. 18, 1886	Fibro-myoma	"	"	"	"	"	Extra-peritoneal	"	Excellent		"	15 " 9 inches of uterine cavity	"	
19	46	Feb. 22, 1886	"	Died	"	"	"	Died	"	"	Died	Peritonitis. Ulceration of the cæcal valve, pelvic abscess. Death 27th day	"	16 lbs.	Died	
20	46	Oct. 6, 1886	Fibro-myxoma	No	Six	Married	"	No	"	Yes	Excellent		"	23 "	Recovered	
21	38	Jan. 11, 1887	Fibro-myoma	Died	One	"	"	Died	"	No	Died	No peritonitis. Death on 5th day. Physiological shock, apparently	"	6 "	Died	
22	45	Jan. 22, 1887	"	No	No	Single	"	No	Extra-peritoneal, including a piece of the bladder	"	Good		"	38 "	Recovered	A portion of bladder was included in serre-nœud, and urine came from the wound for four weeks.
23	72	Feb. 9, 1887	Normal uterus	"	One	Widow	"	"	Extra-peritoneal, tied with silk, and held outside with a pin	Yes	"		Yes	Incorporated with left ovary	"	Ventral hernia cured at same time.
24	51	Feb. 12, 1887	Fibro-myoma	"	Three	Married	"	"	Extra-peritoneal	No	"		No	7 lbs.	"	
25	36	Feb. 19, 1887	Uterus incorporated in an ovarian tumor	"	No	Single	"	"	Extra-peritoneal, tied with silk, and held outside by a pin	Yes	"		Yes	Incorporated with left ovary	"	Twisted pedicle
26	38	March 18, 1887	Fibro-myoma	Died	"	"	"	"	Extra-peritoneal. Pelvic portion of tumor left behind			Obstruction of ureters by wire	No	22 lbs.	Died	
27	48	April 29, 1887	"	No	"	"	"	"	Extra-peritoneal	No	"		"	16 "	Recovered	

LAPAROTOMIES. ATTEMPTED AND PARTIAL REMOVAL OF UTERINE AND OVARIAN TUMORS.

No.	Date.	Age.	Cause of Operation.	Result.	Remarks.
1	Feb. 3, 1880	56	Ascites and sarcomatous ovaries	Died	Died on the 10th day. Diseased kidneys and probably peritonitis. No autopsy.
2	June 14, 1881	17	Sarcoma of omentum, mesentery and pelvic organs	"	Died of shock in a few hours. About 13 lbs. of encephaloid cancer removed.
3	July 13, 1881	50	Uterine fibro-cyst	"	Cyst emptied and walls sewed up and dropped back.
4	Nov. 21, 1881	33	" "	"	Cyst emptied. 10½ lbs. solid matter removed. Cyst walls and walls of tumor brought outside and held by a steel sound.
5	Dec. 29, 1881	36	" "	Recovered	Cyst emptied. 14 lbs. fluid removed. Another fibroid, size of a plum, removed. Died some years later.
6	Feb. 19, 1882	50	Solid sarcoma or cancer of uterus, ovaries, bowels and pelvic contents	Died	Shock caused death an hour after operation.
7	May 6, 1882	42	Ascites and cystic tumor in pelvis	"	Cysts (small) punctured. Uterus involved in tumor.
8	Aug. 24, 1882	39	Adherent ovarian cyst	"	Nothing removed.
9	Sept. 11, 1882	52	Fibro-cyst of uterus	"	Portion of tumor (7 lbs.) removed.
10	Sept. 21, 1882	52	Adherent ovarian tumors	"	Ascitic and ovarian fluid removed and a portion of cysts.
11	Nov. 15, 1882	29	Cancer of ovaries and peritoneal organs	"	One ovary removed, and ascitic fluid and a portion of cancerous omentum for diagnosis.
12	April 21, 1883	30	Ascites and general abdominal cancer	"	Probably growth originally, papilloma of ovaries.
13	May 21, 1883	36	Fibroid of uterus, and tumor of both broad ligaments and uterus	"	Cysts and fluid removed, partially.
14	July 19, 1883	41	Tumor filled with pus, adherent to bowels, small and large, and pelvis	"	Probably a salpingitis. Died in convulsions in 24 hours. No autopsy.
15	Nov. 27, 1883	46	Cystic fibro-myoma and ovarian	"	Partial removal of uterus and 19 lbs. of fluid.
16	Oct. 18, 1884	54	General abdominal cancer. Originally, perhaps, ovarian	"	Twenty-five lbs. of fluid removed.
17	May 7, 1885	40	Uterus filled and covered with fibro-myomatous tumors	"	A sebaceous mass removed, and probably more or less of the right ovary.

LAPAROTOMIES FOR OVARIAN CYSTS, STITCHED TO SKIN.

No.	Date.	Age.	Cause of Operation.	Result.	Remarks.
1	July 18, 1883	47	Adherent cyst, origin probably ovarian, inseparable	Recovered	One side of cyst sewed to skin, and drained.
2	March 4, 1884	65	Tumor punctured. Ovarian, probably	"	Tumor stitched to skin, and drained. Died subsequently, of cancer of liver and uterus.
3	Aug. 13, 1884	38	Tumor probably ovarian, or of broad ligament, and tubercular. Peritonitis	"	Tumor drained, and stitched to skin. Well in 1887.
4	Oct. 6, 1884	40	Tumor ovarian—adherent	"	Tumor stitched to skin, and drained. Died of phthisis, 1885.
5	June 3, 1885	31	Cyst of right broad ligament	"	Cyst sewed to skin, and drained. Well in 1887.
6	June 12, 1885	37	Multilocular ovarian cyst—adherent	"	Cyst stitched to skin, and drained. Well, except for slight purulent discharge in 1887.
7	May 25, 1886	41	Cysts of both ovaries. Left, adherent and inseparable	"	Cyst stitched to skin, and drained. Perfectly well in 1887.
8	June 3, 1886	40 (?)	Multilocular ovarian adherent cyst, with thick walls	"	Cyst stitched to skin, and drained. Perfectly well in 1887.

LAPAROTOMIES FOR CYSTS COMPLICATED WITH OTHER DISORDERS.

No.	Date.	Age.	Cause of Operation.	Result.	Remarks.
1	July 3, 1879	45	Uterine fibro-cyst	Died	Walls of cyst stitched to skin. Both ovaries had been removed in 1877, on account of cystic disease.
2	Oct. 11, 1882	65	Uterine fibro-myomatous cyst, and ovarian cyst	"	Ovarian tumor removed. Uterine cyst stitched to skin.
3	Aug. 1, 1883	42	Tumor probably ovarian, but exact nature unknown. Almost solid	Recovered	Tumor stitched to skin, and drained. Went home, but died soon afterward.
4	Sept. 29, 1883	About 40	Semi-solid tumor attached to bladder and all neighboring organs. Probably ovarian, originally	Died	Tumor stitched to skin, and drained.
5	March 8, 1886	35	Cyst of extra-uterine pregnancy, seven years old	"	Cyst stitched to skin, and drained. All the skeleton of an adult fœtus removed. Specimen in Warren Museum of Harvard College.
6	June 5, 1886	45	Partial removal of adherent ovarian cyst, and of fibroid tumor of uterus	"	Cyst stitched to skin, and drained. Died of shock and peritonitis.
7	April 5, 1887	45	Adherent tubo-ovarian cyst. Fibro-myoma. Hydro-salpinx	"	Cyst stitched to skin, and drained. Died of acute peritonitis.

EXPLORATORY LAPAROTOMIES.

No.	Date of Operation.	Age.	Cause of Operation.	Result.	Remarks.
1	Dec. 12, 1878	62	Ascites, and pelvic tumor of a sarcomatous nature	Recovered	The ascites never recurred. She died a year afterward.
2	Feb. 22, 1881	48	Papillomatous adherent tumors and ascites	"	Died eight months later, Sept. 9, 1882. At the autopsy, the tumors were found to be irremovable.
3	Aug. 18, 1881	36	Solid sarcoma, probably not cystic. So adherent as to be immovable, and originally ovarian. Some ascites	"	Died a year or two later.
4	Sept. 22, 1881	21	Ascites, and pelvic tumor, immovable	"	Tumor is *said* to have disappeared in 1885, after many abdominal tappings.
5	Sept. 20, 1882	37	Ascites, and general abdominal cancer	"	Died, March 19, 1883, six months after laparotomy.
6	May 3, 1883	34	Ascites, and general abdominal cancer	"	Is thought to have died about three months later.
7	Aug. 9, 1883	60	Ascites, and general abdominal cancer	Died	In three days, died of peritonitis.
8	March 22, 1884	66	Ascites, and general abdominal cancer	Recovered	Died about six months later.
9	June 13, 1884	17	Tubercular salpingitis and cheesy deposits in abdomen, of tubular and tubercular origin	Died	Death from peritonitis, on third day.
10	June 19, 1884	21	Tubercular peritonitis	Recovered	Fat and well in 1886. Married. Has gained thirty pounds.
11	Jan. 3, 1885	46	Cancer of all abdominal viscera. Ascites and drainage	"	Died, six months later.
12	July 9, 1885	10	Cancerous abdominal tumor, of unknown origin	"	Died, May 1, 1886, nine months after laparotomy.
13	Nov. 20, 1885	62	Cancer of omentum, probably. Ascites	"	Died of pneumonia, March 13, 1886. Health, up to that time, better than for many years. Much benefited by operation.
14	Feb. 19, 1886	28	Enlarged uterus, and apparent tumor in left iliac region	"	No removable tumor found.
15	Nov. 2, 1886	65	Tumor in right iliac region	"	Tumor involving and including small intestine—not removable.
16	Nov. 12, 1886	56	General abdominal cancer	"	Drainage. Ascitic fluid removed. Died some months later.
17	Feb. 16, 1887	63	General abdominal cancer	"	Went home well. Not since heard from.
18	Feb. 22, 1887	55	Abdominal tumor	Death. Septic peritonitis	Ovariotomy, six years before.
19	April 20, 1887	17	Ascites. Tubercular peritonitis. Ovary and tube removed for diagnosis	Recovered	Drainage. Prof. Fitz reported the disease tubercular. Went home well and strong.

FIVE CASES OF REMOVAL OF UTERINE APPENDAGES FOR THE CURE OF UTERINE HEMORRHAGE.

THREE CURES. ONE NO IMPROVEMENT. ONE DEATH.

No.	Date.	Age.	Cause of Operation.	Catamenia since.	Result.	Recovery or Death.	Remarks.
1	Oct. 15, 1884	33	Soft fibro-myoma	Flowing slightly much of the time Catamenia ceased June, 1886	Recovery	Recovered	Tumor, in 1887, smaller. Her monthly sickness has been less than before operation, but a rather continuous leaking of blood till June, 1886.
2	Jan. 18, 1885	44	Fibro-myoma and menstrual Insanity	No	Perfect recovery. Tumor has disappeared, and the patient is happy and overjoyed at her recovery	"	In Worcester Insane Asylum about 1871. The resources of the town where she lived had been exhausted in attempts to restrain and control her during her menstrual periods.
3	Aug. 25, 1885	34	Profuse bleeding from fibro-myoma	Flowed for five weeks. Now catamenia is regular and normal	Perfect recovery Tumor about the same size	"	Much less blood is now lost than before operation.
4	Oct. 7, 1885	30	Profuse bleeding from fibro-myoma	Died	Died	Death from peritonitis	It would have been better to have removed the uterus. The tumor was a large, wet, spongy fibro-myoma, and the ovaries were only got at after the tumor had been pulled out of the abdomen. Subsequently, the tumor was stuffed back.
5	Jan. 21, 1886	36	Bleeding fibro-myoma	Yes. Excessive flowing	No improvement	Recovered from operation	Tumor somewhat larger December 29, 1886, and patient's condition worse.

FIVE CASES OF REMOVAL OF UTERINE APPENDAGES FOR NERVOUS DISORDERS.

ONE CURE. FOUR NO GREAT IMPROVEMENT.

No.	Date.	Age.	Cause of Operation.	Catamenia since.	Result.	Recovery or Death.	Remarks.
1	March 26, 1883	22	Hystero-mania	No	Much improvement for 2 years, since then in bed	Recovered	I think there is no permanent improvement, though the patient states that there is.
2	July 20, 1883	19	Moral insanity. Operation done at Danvers Insane Asylum	"	Complete relief. Perfectly well	"	Case reported by Dr. Goldsmith in *American Journal of Insanity*, October, 1883.
3	Dec. 13, 1883	27	Mania. (Chronic)	"	No improvement whatever	"	Died in Worcester Asylum, of phthisis, 1886.
4	Aug. 24, 1885	35	Severe pain in left ovarian region, causing nervous symptoms	Yes. Regular	No great improvement	"	About the same as before operation. Catamenia painful.
5	Nov. 25, 1885		Pain, nervous excitement, and hystero-epilepsy	Yes	No great improvement	"	The same condition as before operation.

LAPAROTOMY FOR OTHER PURPOSES THAN THE REMOVAL OF OVARIAN TUMORS. TWO IMMENSE FATTY TUMORS.

No.	Date.	Cause of Operation.	Result.	Remarks.
1	Oct. 30, 1881	Immense lipoma. Retro-peritoneal. Patient, a man aged 38. Operation exploratory	Recovered	The tumor was found to be broadly attached behind the peritoneum, and the abdominal wound, extending nearly from the Ensiform cartilage to the pubes, was sewed up.
2	Feb. 5, 1882	Same patient, who desired to try the operation as a last resort	Died	Tumors removed weighed 56 pounds. More myxomatous tumors remaining behind. Cases reported in *Boston Medical and Surgical Journal*, Vol. cviii, pp. 35 and 241.
3	March 21, 1882	Patient, a woman with a fluctuating tumor from which nothing would run when aspirated. Aged 61	"	Tumor removed, a myxo-lipoma, weighing 35 pounds, retro-peritoneal. Case reported in *Boston Medical and Surgical Journal*, Vol. cviii, pp. 35 and 241.

RENAL TUMORS.

No.	Date.	Sex.	Cause of Operation.	Result.	Remarks.
1	April 19, 1882	Female	Calculi and abscess of right kidney. Aged 30	Recovery from the operation. Death one month later	A case of calculous nephritis of long standing, in a worn-out, feeble woman. Stones removed, measured two inches in diameter. Incision at outer border of right rectus muscle.
2	Nov. 23, 1883	Male	Sarcoma of left kidney. Aged 29	Died	Case reported in *Boston Medical and Surgical Journal*, Vol. cx, p. 73. Suppression of urine and peritonitis.
3	Aug. 1, 1884	Female	Large sacculated kidney (right) filled with pus. Tumor weighed 2 pounds. Aged 42	"	Suppression of urine. Case reported in *Boston Medical and Surgical Journal*.

LAPAROTOMY ON ACCOUNT OF INTESTINAL OBSTRUCTION.

No.	Date.	Age.	Sex.	Cause of Operation.	Treatment.	Result.	Remarks.
1	Dec. 21, 1881	43	Female	Intestinal obstruction, caused by cancer of the descending colon, seven months after ovariotomy	Artificial anus in pubic region	Recovered	Very comfortable for nearly a year. Died of general abdominal cancer, November, 1882.
2	July 10, 1882		"	Intestinal obstruction, caused by cancer of the descending colon, in the splenic region	Artificial anus in cæcal region	Died	Patient very stout. Weather very hot.
3	Feb. 18, 1884	21	Male	Intestinal obstruction, caused by a band from a Meckel's diverticulum	Artificial anus in umbilical region	"	Great relief for a week. Case described by Prof. Fitz, in the *American Journal of the Medical Sciences*, Vol. clxxv, p. 56.
4	April 3, 1886	61	Female	Intestinal obstruction, caused by strangulation of the intestine at the site of the cicatrix of ovariotomy	Artificial anus at site of old cicatrix	"	Described in text.

LAPAROTOMY ON ACCOUNT OF PERI-TYPHLITIC ABSCESS.

No.	Date.	Cause of Operation.	Treatment.	Result.	Remarks.
1	Jan. 11, 1886	Peri-typhlitic abscess and peritonitis	Laparotomy in right ilio-lumbar region	Recovered	Well and hearty in 1887.

LAPAROTOMY FOR PYOSALPINX AND SUPPURATION IN OVARIES AND TUBES.

No.	Date.	Age.	Cause of Operation.	Result.	Remarks.
1	May 21, 1886	22	Double pyosalpinx and abscesses in both ovaries (gonorrhœal).	Recovered	Both ovaries filled with abscesses, and adherent. The right tube could not be distinguished. Many ligatures, or, rather, many knots. Ligature discharged seven months later.
2	Dec. 6, 1886	44	Suppurating tubo-ovarian cyst, with general old pelvic peritonitis (tubercular?)	Died	Pelvic organs pressed together, and only right Fallopian tube and uterus distinguishable.

LAPAROTOMY FOR LONG-STANDING PELVIC ABSCESS.

No.	Date.	Age.	Sex.	Cause of Operation.	Result.	Remarks.
1	Aug. 29, 1885	30	Female	Pelvic abscess for 19 months. Abscess probably tubercular	Recovered	Improved by operation, which consisted of passing an India-rubber drainage tube from the pubic region through pelvis and rectum, and out through the anus. Died of general tuberculosis, July 9, 1886.

OVARIOTOMIES BEFORE THE ADOPTION OF LISTERISM; *i. e.*, PRIOR TO 1877.

No.	Date.	Age.	Kind of Tumor.	Which Ovary.	Catamenia since.	Children before.	Children since.	Married or Single.	Length of Incision.	Rupture.	Ligatures heard from.	Health since.	Fibroid seen.	Fibroid removed.	Twisted Pedicle.	Result.	Drainage.	Adhesions.
1	1872	30 (?)	Multilocular cysts	Both		No		Single	Long				No	No	No	Died	No	Yes.
2	April 24, 1873	49	Multilocular cyst	Right		"		"	"				"	"	"	"	"	"
3	Feb. 17, 1874	34	Papilloma	Both		"		Married	"				"	"	"	"	"	"
4	Feb. 18, 1874	43	Multilocular cyst	Right		"		"	"				"	"	"	"	"	"
*5	May 18, 1875	36	Unilocular cyst	Left		Yes		"	Short				"	"	"	"	"	No.

* Only one of the above cases, the last, was at all promising. I think I could easily cure a similar one now.

RÉSUMÉ OF TABLES.

		Recoveries.	Deaths.
282	Ovariotomies,	248	34
27	Hysterectomies,	17	10
32	Partial Removals of Uterine and Ovarian Tumors,	10	22
19	Exploratory Operations,	16	3
10	Removals of Uterine Appendages,	9	1
14	Miscellaneous Laparotomies,	5	9
384	Laparotomies,	305	79

Dr. MANLEY, of New York, read the following paper:—

REPORT OF A CASE OF GUNSHOT WOUND OF THE ABDOMEN, WITH PERFORATION OF THE LARGE INTESTINE, WITH COMMENTS, ETC.

RAPPORT D'UN CAS BLESSÉ DE PORTÉE DE FUSIL DE L'ABDOMEN, AVEC PERFORATION DU GRANDE INTESTIN, AVEC DES GLOSSES, ETC.

BERICHT ÜBER EINEN FALL VON EINER SCHUSSWUNDE IM UNTERLEIBE MIT DURCHLÖCHERUNG DES GROSSEN DARMES, NEBST ERKLÄRUNGEN, ETC.

BY T. H. MANLEY, M. D.,

Of New York City.

On the night of the 17th of June, 1887, at 10 o'clock, J. C. was brought by the ambulance to the Harlem Hospital, of New York City.

He had mortally wounded his wife by a pistol shot (thirty-four calibre Smith & Wesson's revolver) through the abdomen, and immediately after, in a moment of phrenzy, turned the weapon on himself, shooting himself in three places. One ball struck the frontal bone, and was found just above the left orbital ridge, flattened; the second passed through the inner aspect of the left arm and out through the opposite side. The abdomen was pierced by another bullet, one inch from the medial line, to the left, in the left *epigastric* region, directly over the stomach.

The patient was a large, vigorous man, 41 years old, of fine muscular development, something more than six feet tall, weighing two hundred pounds, and apparently enjoying good health previous to the injury. I saw him shortly after midnight, a little more than two hours after the wounds were inflicted. At that time he was dazed, suffering, though but slightly, from shock. The pulse was steady, of fair volume, one hundred a minute. The respirations were rather rapid, of the thoracic type. He had not vomited any, nor complained of pain, but said that he felt very weak. He was given three ounces of whiskey by the mouth, and immediately anæsthetized with ether. He resisted the ether with great energy, and took nearly a pound before he came well under its influence.

On examination the abdomen was found much distended, and on percussion gave unequivocal evidence of there being some liquid substance filling it throughout its whole extent. The opening made by the missile was probed with great care and delicacy, but all that this search revealed was, that the ball took a downward and backward direction. Its place of entrance into the abdominal cavity could not be made out. With the evidence of internal hemorrhage, however, and the absence of profound shock, and with the general safety, by which the abdomen may be opened under modern surgical methods, it was decided to make an exploring incision into the peritoneum. As it was midnight we had to illuminate with a portable gas-lamp, the kind used in making throat examination. It answered the purpose very well. A cut was made into the *linea alba*, beginning just below the ensiform cartilage, carrying it down four or five inches, close to the navel. When the peritoneum was punctured, bright red arterial blood welled up through the incision in large gushes. It seemed as though a half gallon of blood was lost, on a rough estimate. The wound was now rapidly enlarged so as to make room for admission of the hand, and the speedy examination of the pelvic viscera. Our first efforts were directed toward finding, if possible, the source of hemorrhage, and controlling it; so that we could then more safely and leisurely follow the course of the bullet, and try to dislodge it. I may state, that before

we made our incision, we blanketed the patient well and laid close to him many jars of hot water, to provide against heart failure or additional shock. As the different segments of the intestinal canal were carefully removed and examined they were covered with warm, wet towels, and each quickly returned. The peritoneum was sponged, and by tracing the oozing the source of hemorrhage was soon discovered. It was found to be a branch of the superior mesenteric, torn completely across in the furrow made by the bullet. The bleeding orifices, being deeply imbedded in the adipose tissue of the meso-colon, were quickly secured with fine catgut ligature; one on each end. Fecal discharges mixed freely with the blood. The discovery of the bleeding-point opened the way to finding the intestinal wound, for close to it could be seen the double perforation of the gut, the anterior border of the descending colon, about one inch below the angle, the ball passing into and through the lumen of the bowel, making two openings, one of entrance and one of exit. The ball passed so close to the margin of the gut that there was not more than half an inch of space between each perforation. The colon was filled with liquid fæces, which, with even moderate handling, freely escaped through the openings and mingled with the effused blood. The edges of the gut were carefully approximated and sutured with catgut—continued suture—care being taken to include with the needle only the peritoneal and muscular coats, leaving the internal or mucous lining of the bowel free. Further search was now made for the bullet, but without success. The peritoneal cavity was thoroughly irrigated with a very warm preparation of Thiersch's solution, all blood clots removed, the parts well sponged. A large drainage tube was now inserted at the base of the incision, carrying it into the peritoneal cavity, the viscera returned, and the wound in the median line sewed, including the peritoneum, the whole length of the suture. Antiseptics were rigidly applied from the beginning to the end, and no detail omitted. The time occupied in performing the operation was a little more than an hour.

The man reacted well, the pulse never flagged, and there was no vomiting when consciousness returned. The house-surgeon, Dr. Robert Lewis, was directed to give morphine freely, *i. e.*, as frequently, and in such doses, as were needed to control pain, or modify it. He rested comfortably for three or four hours to awaken the day following with great pain and frequent spasm of the abdominal muscles, especially of the rectus. He vomited twice in the morning, and passed a very restless day, the pulse going up to 120 per minute, and the temperature rising to 101⅛°.

The progress of the case was uneventful after the first day, except for the behavior of the incision. He took nourishment well, in liquid form, from the beginning, which consisted mostly of milk punch, eggnog, oatmeal gruel and beef tea. On the third day he had a free movement from the bowels, which acted spontaneously. The discharge was semi-liquid and contained no evidence of blood. The temperature during the first week fluctuated between 98½° and 101°. He always urinated without the need of the catheter. There was well-marked peritonitis after the first day. The abdomen was very much distended by a tympanitic state of the bowels, and was extremely sensitive over its whole area. As a sequence of the constant and terrible tension, the result of this swelling and the torturing spasmodic action of the muscles, it was evident on the fourth day that the wound would not heal by primary union. The drainage tube carried away a large quantity of the red stained serum, and was removed on the third day. On the fourth day, when it was found that the entire centre of the wound had given way, and with each strain a large mass of omentum protruded, it was deemed advisable not to wait any longer, but make another endeavor to bring about firm union.

Again our man was placed under ether. This time the edges of the peritoneum were pared and brought together separately, with medium-sized catgut suture. The subcutaneous fibrous tissue of the linea alba and integument were this time secured with the silver wire—interrupted suture—each placed half an inch apart and carried through

the whole length of the wound. Similar antiseptic precautions were observed as in the original operation. The whole was covered with antiseptic gauze and this retained with a broad, strong binder. When the effect of the ether passed off, he expressed himself as feeling much more comfortable about the belly. Unfortunately, as before, the same pain annoyed him in the wound, aggravated with every respiration, which he strove to ease by powerful contractions of the muscles, and again, too, we found that all the central sutures had torn through the tissues; those alone remaining at each end of the wound, where there was little tension. The peritoneum, however, had healed thoroughly, from end to end. The ultimate closure of the wound was slow but perfect. Healthy granulation sprung up, and in six weeks from the time of shooting, by keeping the abdominal muscles under restraint with the binder, and the patient very quiet, we got good, solid union. We had been unable to obtain any clue as to where the bullet lodged, though from the course it took we are inclined to believe that it lies encysted somewhere in the deep muscles of the back. It has given him no annoyance thus far, and to-day he is in the enjoyment of perfect health.

The above is the report of my case, faithful and unvarnished, and is offered simply as a contribution on the subject of abdominal section in gunshot wounds. But, while on the subject, I beg to be permitted to make a few comments on the *modus operandi* of the operation, its indications, and contra, and mention something which has an important bearing, in my mind, in every case, which I have never heard spoken of, or seen anything written on.

The attention of the profession of America was first specially directed to the general safety and expediency of laparotomy for the treatment of gunshot or stab wounds of the peritoneum by that late distinguished surgeon, Dr. Marion Sims, who, shortly before he died, presented an essay on the subject in the New York Academy of Medicine, in which, with great fervor and eloquence, he "claimed there was a future for the operation," and he, indeed, went so far as to advise the exploratory incision for all obscure maladies of the abdominal viscera.

It must be admitted that his great advocacy of the propriety of the operation gave it its first great impulse in this country; therefore, to him we are largely indebted for the marvelous results realized by his labors in this and other directions which have proved the soundness of his reasoning.

The operation of abdominal section has, perhaps, interested the members of the medical profession more than any other modern procedure in surgery. Sims claimed that the intolerance of the peritoneum was much exaggerated, and that many lives were lost through the surgeon's timidity.

With the aid of Lister's great discovery, the application of antiseptics, it is claimed, when intelligently and faithfully used, septic contamination is impossible, and that the abdomen may be readily opened without danger to life or any ultimate serious consequences.

While regarding laparotomy for abdominal lesions as one of the greatest triumphs of modern surgery, I must emphatically demur from the general opinion that it is a trivial affair, but must, rather, express my own convictions—which I have come to from what I have seen in my own cases and others—that it is one of the most serious in its consequences, immediate and remote, that we are ever called upon to perform, *particularly when we operate on the male.*

Quite a number of abdominal sections have been made in New York within the past five years, *on men*, for injuries of various descriptions within the peritoneum—something more than twenty-five, I am informed—my case being only the fifth successful one. A mortality, then, of about eighty per cent. This, too, with skillful hands and all the accessories of modern surgery. The only way to account for the large mortality is to attribute it to indiscriminate cutting.

Profound shock, a species of mortal syncope, so frequently found after abdominal injuries, should be regarded as a contraindication to immediate operation. I know that some deny that this condition should cause delay, or that its existence should in any way interfere. But I am persuaded that this is wrong. I am sure no good surgeon would do an amputation, unless, under the most grave emergencies, without waiting for reaction to set in; for there is no doubt but an abdominal section is as dangerous to life as any ordinary amputation. Hence, then, the abdomen should not be opened without reasonable prospects of good reaction. I am informed that in all the fatal cases recorded death ensued before thorough reaction was established.

The influence of sex. An incision into the abdominal cavity of a man is always a very serious matter, much more so than with a woman. When we look at the physiological difference in the sexes and diverse nature of occupations, we can easily see that this assertion is unassailable. The abdomen of the male is *always* in constant motion. His respiration is largely diaphragmatic. Ventral protrusion—hernia—is common with him, while it is rare with women. The abdomen is constantly undergoing change in shape and size, and its cavity is the only one in the body which permits, on great strain, the escape of its contents. We cannot keep the wound *at rest* in the male. That constant up and down motion of the diaphragm painfully disturbs and decidedly interferes with primary union. We cannot bring to bear, in his case, the firmly-adjusted bandage and throw the aspiratory function on the thoracic muscles without occasioning him great distress or even endangering life.

With a woman, on the contrary, who is accustomed to an iron-framed casket, with strong cording, we can bring almost any pressure on the abdomen without the least discomfort or danger. Through habit and the demand of modern fashion she normally breathes with the thoracic muscles alone, and, as a consequence, what is borne with great distress in the male is worn by her with comfort. The breathing, then, with one is carried on above the diaphragm, and with the other largely below. Here, then, we find the key to the difficulty—an obstacle in one sex unknown in the other; a reason, too, why the ulterior consequences in one are more serious than the other.

The rent made into the integuments in reaching the peritoneal cavity is glued together by cement substance and connective tissue,—a tissue, we know, of feeble vitality, liable to ulcerate on the slightest irritation, and very apt to give way on a moderately severe strain, and leave a hernial protrusion—something that practically invalids a man the remainder of his life. A woman, whose occupation is not so laborious as a man's, and who can wear a firm support without discomfort, is not so liable to rupture of the cicatrix. We all know how the parts may, and do, give way after herniotomy, where but a very small incision is made. How much more liable is this to occur when the breach in the abdominal walls is very much larger and where the parts are dependent?

With regard to the technique of the operation: One of the first questions to arise is the location and extent of incision. When we have an opportunity, we prefer an incision through the median line. Here we encounter no large blood vessels and mutilate no muscular tissue. Taking it as a whole, the intestinal tube, at least, is more accessible with this opening than any other.

To get quickly down to and thoroughly explore the peritoneal cavity requires a free incision. By this incision time is saved, and a much more searching exploration can be made. Besides, here, the gut and serous membrane are but a short time exposed. The large opening is as easily sutured as the one of smaller dimensions.

With these advantages, however, there are many serious drawbacks to the large incision, and if we wish to diminish shock, we will make the opening as small as is compatible with necessary manipulation. Again, if we look to the patient's future, if

a hard-working man, we will strive to leave as small and compact a cicatrix as possible; for the smaller this is the less will be the liability to hernia.

The drainage tube is frequently used in this operation. It is hard to understand what purpose it can serve. I used it because it was customary. In the future, under similar circumstances, I shall try to dispense with it. When we have used every possible care to make our operation antiseptic, freed the peritoneum of all foreign substances, it is difficult to see what there is to drain. Its very presence would seem to imply that there was some morbid accumulation left behind. Some claim that by carrying off the surplus liquids it arrests bleeding in some manner or other. But it is not quite clear what good can be attained by draining off the healthy secretions of the peritoneum, or even a little blood mixed with them. The absorbents alone would quickly dispose of any undue quantity, without unpleasant consequences. Besides, I have seen cases in which the drainage tube gave a vast amount of trouble, of pain and discomfort to the patient and embarrassment to the surgeon. The great objection to it is that it acts often as a seton, interferes seriously with primary union, and leaves, when removed, a long fistulous tract, which for months prevents the complete closure of the wound. A wound which should heal in a week or two is often delayed for a long period of time by its use, and the opening it leaves frequently requires the most tedious and protracted care of the attendant to close. The tendency lately, it seems, has been to over-drain, to use this valuable means of carrying off morbid products indiscriminately. To use it in abdominal surgery, except to carry off pus or putrid substances, does not seem to be justified by reason or experience.

The incision, after abdominal contents have been dealt with, should be hermetically sealed. The slightest puncture in the peritoneum will leave a hernia, so that special care should be taken to close the sac completely. Its divided edges heal very quickly, much more so than the skin. To be sure that its edges are thoroughly approximated, it would seem that the best way of dealing with it would be, while the wound is well opened, to suture it separately with fine, strong catgut sutures—something which will become absorbed, and after being deeply buried by the strong cutaneous stitch, will give no further annoyance. For the skin and subcutaneous tissue the aponeurotic expansion of the abdominal muscles in the median line, a strong, non-irritating substance is needed. Silver wire appears well adapted for this. It may remain indefinitely, and will only give way by great tension.

The great difficulty to deal with in a man, abdominal movement in respiration, cannot be controlled by any agency whatever, but may be much modified by a well-adjusted bandage, which will not only give much comfort, but likewise contribute substantial support to the mutilated tissues. It should be carefully watched, however, and be drawn and slackened as occasion may require.

The patient, after the operation, should be confined in bed until perfect union is accomplished. He should not be permitted to make the slightest movement with his body or lower limbs; indeed, everything should be done to avert any disturbance of the wound. Hence, when desirous of changing his position, he should be always carefully lifted. No medicines are needed except something to ease pain or overcome restlessness, and for this morphine has no equal, given hypodermically, from one-eighth to a half grain to a dose.

Diet should be liquid, non-stimulating, and the greatest caution taken that the stomach is not overfilled. No doubt more die in abdominal operations from overfeeding than the contrary. It is well to hold back till the patient has an appetite, then feel our way carefully till we are sure it is safe to feed more liberally.

To recapitulate the salient points in connection with abdominal section:—

The first and most important consideration is shock. Its presence should be regarded

as a sign of great gravity, and ominous of a fatal termination of the case. Nothing will justify a laparotomy when shock exists, except indubitable evidences of hemorrhage, which may be arrested by the incision.

2. The importance of constantly bearing in mind that abdominal section in a man is always a very serious operation, whether done for exploratory purposes or to remove morbid growths or foreign substances. The incessant up-and-down action of the diaphragm in the respiratory movement peculiar to the male is apt to interfere with quiet and rest to the wound, and cannot, by any means known, be entirely obviated.

3. Again, because of the sex, the abdominal incision should not be made unnecessarily large, because of the impossibility of a man wearing, during the remainder of his life, the firm support to the abdominal walls that a woman can; and, further, because of the more laborious character of his occupation, the greater is the liability to rupture at the seat of incision.

4. For simple exploratory purposes, the median incision, through the linea alba, is incontestably the best; the exceptions, when the knife should open the peritoneum in another situation, are when we can absolutely locate the lesion, and wish to reach and remedy it with as much ease and expediency and as little mutilation as possible.

5. The drainage tube can serve no useful purpose in the healthy peritoneum, which is freed from all septic contamination, as there is nothing of an abnormal character to drain off, but, on the other hand, it may, and often does, seriously interfere with immediate union and greatly retards the healing of the incision.

6. Antiseptics, until it can be proven that cleanliness alone will render septic poisoning impossible, should always be intelligently employed, as they give the patient the benefit of what is generally considered by most surgeons of infinite value and relieve the operator of any misgivings in this direction.

DISCUSSION ON THE FOREGOING PAPERS.

Dr. Robert Reyburn, of Washington, D. C., stated that the case of Laparotomy following gunshot wounds of abdomen was an interesting one, and the treatment excellent. Exception, however, must be made to some of the surgical doctrines of the paper, and more especially his advising the dispensing with the drainage tube.

The great feature that distinguishes abdominal surgery of the present day, is the insisting upon absolute drainage, and removal of all foreign and putrid matters from the cavity of the peritoneum. In fact, the old idea, that cutting or wounding the peritoneum was necessarily fatal, was a remnant of an old superstition. When these patients did die, it was, in the great majority of cases, not from the fact that the peritoneum was wounded, but on account of shock or septicæmia, caused by the absorption of putrid materials, through the lymphatic glands of the serous membranes. It was his opinion that an incision continued from the wound, was better than one in the linea alba.

Dr. Hingston, Montreal, Canada, called upon by the Chairman, said:—

The field traversed by the readers of the papers is so vast and so varied that it is impossible to follow any of them closely. A few questions, however, suggest themselves: 1st, In gunshot wounds in the abdomen, when should we operate?

When shock is over? No! Shock is misleading. It may be severe without being dangerous, and when not markedly severe may be dangerous in the extreme.

When there is hemorrhage from the bowel? Not always; for persons often recover where hemorrhage has occurred from the bowels and blood has shown itself

in the urine. But when fæces shows itself in the abdominal wounds there is no alternative—the operation must be performed.

There are cases, and they are the more numerous, where the surgeon is in doubt as to the advisability of operating. When in doubt—operate. An exploratory incision may give information of vital moment, and if not, the patient's chances of recovery are not materially lessened.

Dr. Parkes has drawn attention to the frequency of sloughing caused by the sutures. This accords with my own observation. The surgeon, in his desire to approximate the edges closely, is often apt to use more force than is necessary. Approximation should be secured without drawing, folding, or puckering; and agglutination goes on with such surprising rapidity that contact of the cut serous edge, which in the first instance remains, is quickly followed by plastic exudation, and that again quickly by vital connection.

Before touching upon the paper by Dr. Senn I may be permitted to say that it is one of the ablest and most logical to which I have ever listened—and I have heard a good many in my time. His paper really commences where he laid it aside for the blackboard. Exception, however, is taken—unwillingly taken—First, that the time occupied in the operation is of extreme importance and makes the difference between death and recovery. Length of time is certainly of great moment, but a dread of prolonging the operation must never be permitted to hurry us. The old-time advice, "*Hatez lentement*," is especially applicable to abdominal surgery. The small number of sutures used in Dr. Senn's method is a much greater advantage than what is implied by a saving in time. Every additional suture is undoubtedly an additional disadvantage, and the reduction of Dr. Senn's method is of great moment.

The statement made, if I am correct, that in intussusception gangrene is not the result of deficient supply of arterial flow but of venous congestion, has surprised me. I do not deny its accuracy (I am not in a position to do so). I do not endorse it (it is so contrary to my previous belief). I draw attention to it as a most important statement and one pregnant with interest.

I have already said, when discussing Dr. Parkes' paper, that agglutination takes place rapidly. In two difficult and tedious ovariotomies I performed, which occupied two and a half hours each, fairly firm agglutination was taking place at the upper part of an extensive wound while I was groping for bleeding vessels at the lower part. A force which might be approximately estimated at one and a half ounces, was used to separate the cut edges.

In a case where a sponge was suspected of having been shut up in the abdominal cavity, I opened the wound within an hour of its closure, nothing like agglutination appeared in the wound of the skin, but the peritoneal edges already appeared as if a solution of gum acacia had been penciled over them, and held them together.

Dr. Homan's paper, as might have been expected, was a manly, straightforward narrative of his successes and failures. Ten per cent. of the ovariotomies had been followed by ventral hernia. This appeared to be large, and it would be well to learn in what way the abdominal cavity had been closed, what were the sutures and to what extent had the abdominal parietes been involved in the sutures.

But the most important statement was that concerning removal of the ovaries for purely nervous affections. Four of these had been without beneficial result, one alone had been relieved. This statement, from one of such undoubted reliability, will, it is to be hoped, tend to diminish the frequency of normal ovariotomy. The immunity which has so largely attended abdominal explorations has led, within the

past few years, to a frequency of removing healthy ovaries which is alarming. The question is one deeply interesting to society. Dr. Homans' frank relation of his experience—and it is that of most eminent surgeons—will, it is hoped, diminish the frequency of surgical meddling, which is alarming—an epidemic, in fine. It is surprising how different is the experience of different operators. Some obtain for their patients very great relief, others obtain little or none. I am disposed to think with the latter that nervous affections (and to these I confine myself) which are relievable by the operation, would in many cases get better without. The operation is now-a-days more or less a fashion, as was tonsillotomy in the days of Pope, when the urchins in the streets were wont to call out, "There he goes with his tonsils out." The cry of now-a-days would indicate a mutilation of more serious moment, and here the interest of morals and of the State may alike suffer. Dr. Homans, with his large experience, has operated, in nervous disorders, five times, and in these, he tells us, he acted as an instrument in the hands of others. This is as it should be. It would have been interesting to hear how many times the importunities of patients, friends and physicians have been resisted. In a field of less extent I know something of what it is and how difficult it is to resist their importunities, but I have more than once been gratefully thanked by the patient for having not listened to her own pleading. In less important matters than the maiming of a female, operations are resorted to only after consultation with competent professional authority, and here the writer puts it in the best possible form before the Congress when he says: On the whole, he would not be prepared to advise removal of the uterine appendages for the cure of nervous disorders, unless the operation were advised by a competent clinicist.

Dr. J. P. Satterwhite, of Louisville, Ky., expressed as his judgment that, in all wounds penetrating the abdominal cavity, there should be a careful exploration by enlarging the wound to determine with regard to hemorrhage or wounds of the intestines, and act accordingly.

Dr. Link, of Terre Haute, Indiana:—

I reported a case to the Mississippi Medical Association, in July last, of a little boy eleven years old, injured by the explosion of a railroad torpedo. The fragment of the shell, which I have with me, measures about six lines in length and three in width, weighing just two grains. The fragment, after passing through the hems in front of the patient's pantaloons, penetrated the walls of the abdomen at a point about one and one half inches to the left and below the umbilicus, passing upward, wounding the ileum in four places, passing twice through the mesentery, lodging in the third fold. I sewed up the wounds in the intestines; tied the mesenteric fold containing the bleeding vessels, with silk; sponged out the peritoneal cavity; closed the wound in the abdominal walls with deep sutures, and fixed the whole more securely by broad strips of adhesive plasters extending around the body. The patient recovered rapidly and is now out and perfectly well.

What I wish to say is, that I do not believe that these cases of intestinal wounds will all die without operation, but I do believe the proper teaching is to operate in all cases, making exploratory incisions in doubtful cases. In this case hemorrhage was so severe that the operation was imperative without delay. We should teach the general practitioner to operate in all cases immediately.

Dr. J. B. Murphy, of Chicago: To Dr. Charles Theodore Parkes, of Chicago, belongs unquestionably the honor of bringing intestinal gunshot wounds to their present very prominent position before the profession. His untiring labors in writing,

as well as his more praiseworthy labor in experiments on the lower animals, certainly deserve our commendation, and more so that of the more unfortunate victims of gunshot wounds of the intestines, who have heretofore been left to the mercies of divine providence, to die or recover hap-hazard, for want of rational, decisive, scientific surgical treatment.

Dr. Senn's most excellent paper, which held us in rapture for two hours yesterday afternoon, advances many original ideas sustained by experiments, and promises to give to this subject a new impetus which will carry us into a "*Wonderland*" of abdominal surgery.

It is my pleasant duty to be able to add another recovery to the list of successful cases mentioned by Dr. Parkes in his paper yesterday, making a total of ten recoveries and thirty-eight cases operated upon, three of which cases I operated upon myself, with two recoveries. Of the last one, which was operated upon two weeks ago to-day, I will give you a brief history.

Patient, G. S. (colored), aged 35 (chronic drunkard by occupation), on the 23d of August, 1887, while eating dinner, was shot by his wife with a 32-caliber pistol. The ball entered just below the margin of the ninth rib, 5½ inches from the median line. He was taken to the hospital. I saw him two hours after the shooting, at which time his pulse was 76, full and regular, temperature 98.6, and no symptoms of shock, no dullness on percussion in the lower part of the abdomen, nor was there an absence of hepatic dullness.

It was questionable whether the ball had entered the abdominal cavity at all, as it was situated so far to the right side of the body. I decided to follow the tract of the ball by enlarging the wound, and soon found the opening into the abdomen. I then enlarged the incision to 5½ inches in length, in a direction inward and downward toward the umbilicus. I found that the ball had passed through the liver about an inch from its border, through the convex surface of the transverse colon, about one inch from its beginning, leaving a bridge of intestinal wall from one-half to three-fourths of an inch between the point of the entrance and point of exit. The ball passed on into the muscles of the back, without injuring the duodenum, which was in close proximity to its passage. I then drew the injured colon out of the abdomen, removed the bridge of intestine with scissors, freshened the surface of the wound, having then an opening in the bowel about 1¼ inch in length. I first united the mucous membrane and muscularis with a continuous catgut suture, then sewed over that also with a continuous catgut suture the peritoneal covering; cleansed the abdomen with boric solution, united the parietal peritoneum with a continuous catgut suture, and the muscles and skin with an interrupted silk suture. The first twenty-four hours after the operation the patient had some vomiting from the effects of the ether, but from that on he had no unfavorable symptoms. His temperature at no time exceeded 99°. On the 2d of September (the day before I left for this city) I removed the primary dressing and found a complete union. The patient was allowed to return to usual diet.

To-day, September 5th, I received a telegram stating that the patient was *all right.*

Conclusions drawn from my Cases.—In none of the cases did the symptom of shock predominate. Not even in the second, notwithstanding there was upward of a quart of blood in the abdominal cavity from the bullet wound through and through the liver. In no case was there nausea or vomiting, and the pulse in none exceeded 80, not even in the first case, where there were eleven wounds in the intestine. There was dullness in the lower part of the abdomen in the second case, due to the large

quantity of blood. There was not a diminished area or absence of hepatic dullness in any of the cases, a symptom so frequently mentioned by many writers on this subject. Nor was there present in the external wound fæces, as was yesterday suggested by my friend Dr. Hingston, of Montreal, and I admonish you, gentlemen, against waiting for the appearance of fæces in the external wound before operating, as was suggested by that gentleman. In none of the cases did the quantity of fæces, escaped from the bowel, exceed a drachm, not even in the case with so many intestinal wounds. The wounds in the bowel are so ragged that the edges fall back and prevent the immediate escape of the bowel's contents. If you wait until the contents of the bowel appear at the external opening, you wait to make an ante-mortem operation, as your patient has already a septic peritonitis. This would not be conservative surgery, but irrational and criminal surgery.

The operation should be made immediately and a thorough exploration of the abdomen, with a thorough toilet of the same, because the operation, of itself, is almost devoid of danger, while the presence of fecal matter in the abdominal cavity is absolutely dangerous. When a blood vessel is injured, no surgeon hesitates to tie it to prevent his patient from bleeding to death. When the bowel is injured, it is equally imperative, and the surgeon's duty, to sew it and prevent his patient from getting a fatal peritonitis. We have no choice in the latter more than in the former. It is the straight line of duty, and he who fails to do it, fails to do his duty as a surgeon, and is not worthy of the charge confided to him. Waiting is cowardly surgery, and not conservative, and I protest against its being clothed with that mantle.

Incision.—In the majority of cases the incision in the median line will be found more advantageous, and in people with narrow bodies the liver can be thoroughly examined with this incision, even to the finding of the point of exit of the ball from the posterior surface, as in my second case. In men with broad bodies, as in my last case, it would be impossible to bring the ascending or descending colon into the wound made in the median line to repair it, if injured. Therefore, in cases where the point of entrance is very far from the median line, and where it is supposed the colon is injured, it is advisable to make the incision transversely toward the umbilicus, from the point of entrance of the bullet, except in cases where it is supposed that the bullet passed transversely through the body, where the median incision is the only one that will admit of a proper examination of the parts. The complication of a wound in the liver, which occurred in two of my cases, played no important part, except in the second case, where there was a considerable hemorrhage from that organ, which had ceased at the time of operation and therefore required no special attention.

Suture.—Catgut was used after first freshening the margin of the wound in the intestine, uniting first the muscular and mucous layer with a continuous suture, then covering this with a layer of peritoneum, also with continuous suture.

The period of time required for primary union with plastic exudate is less than thirty-six hours, as in my first case, upon which I made a post-mortem thirty-six hours after the operation. I found the catgut in the eleven wounds in the bowel completely covered from sight and the bowel water tight. Some of the sutures were entered transversely and some longitudinally, but no symptom of venous stasis.

Toilet.—The toilet consists in sponging out the abdominal cavity with a saturated solution of boric acid, suturing the parietal peritoneum with catgut, then the muscular coat with catgut, if the transverse incision is made, and the skin and subcutaneous tissue with silk suture. Put on the permanent antiseptic dressing;

put the patient on milk diet, and give sufficient anodynes hypodermically to allay pain.

Dr. W. W. Dawson, Cincinnati, Ohio, said:—

A few years ago, before my class, I made some twelve experiments to test the effect and extent of rectal injections. I placed a tube in the rectum, secured it with a cord and then slowly injected water. When the colon was filled, the ileo-cæcal valve refused to yield in eleven of the twelve procedures. When great force was used, the colon was ruptured. Dr. Senn found it to yield only when the bowel below was very greatly distended. In the instance where there was incompetency, I found the valve defective, or imperfectly formed.

Dr. Bolty, our distinguished countryman, a few years ago reported a case where he forced an injection from the anus to the mouth. Now I am inclined to think that where this can be done, the valve will be found defective.

Dr. Cowden, of Rock Island, said:—

Dr. Senn, in his paper, makes this statement, if I understood him rightly, "injections which pass the ileo-cæcal valve distend the bowel so as to cause a longitudinal rupture of the peritoneal coat, with but one result, the death of the animal."

This may hold good with the dog, but, from my own experience in the matter, I know it not to be true in the human subject.

Some years ago I was so unfortunate as to get intussusception of my bowels, the invaginated portion coming down so that the index finger well introduced into the rectum could circumscribe the invaginated portion. After many days of horrible suffering and untold agony, not relieved by anodynes, etc., my abdomen tympanitic and largely distended, and after being advised by the late Prof. Gross to introduce a trocar into the bowels for the purpose of immediate relief, I adopted the following method, viz.: Procuring a rubber tube, five feet long, one inch and a half in diameter, making the calibre about one inch, then attaching one end of this hose to the neck of a gallon bottle with the bottom broken off, the free end of the tube was introduced into the rectum. The bottle was then filled with warm water and gradually elevated, as I could stand the pressure, until it was raised to the full length of the tube. By the time the bottle had been elevated about three feet from the bed, the gas began to escape, bubbling out through the water, affording me great relief from my suffering, and removing the tympanites. The bottle was slowly elevated to the full length of the tube, and all the while kept full of water. After this had been done for a few moments, there was a sharp report, and the water soon disappeared from the bottle; my abdomen became at once distended to about the extent that it had been tympanitic. I then believed the bowel had been ruptured and the water had entered the abdominal cavity.

Some fifteen or twenty minutes thereafter the water began to pass off, and shortly after that my bowels moved very freely, so that from that time on my recovery was assured, although I did not allow my bowels to move, only when in the recumbent position, for almost a year after that time. The point is that, in my case at least, the injection passed beyond the ileo-cæcal valve, and I, discounting the dog, am still alive.

Dr. W. F. Peck, Davenport, Iowa, said:—

The papers submitted for discussion are unusually important and interesting. That presented by Dr. Senn is replete with theoretical and experimental suggestions,

which have undoubtedly required great skill and much patient labor. If the views expressed and based upon operations on the lower animals prove correct when demonstrated in the living human subject, a great advance will have been made in abdominal intestinal surgery. But until the experiments which have been made by Dr. Senn on the lower animals have been verified on the human subject, the profession should not be held to the strict accountability which the doctor demands. If it shall be practically demonstrated that the extensive stitching of the bowel can be done away with by reason of special placing of the traumatic surfaces, and that sufficient adhesion can be secured in from twenty-four to forty-eight hours to permit of the natural function being properly performed, the profession will welcome the improvement, and the lives of thousands will undoubtedly be rescued thereby. Before another Congress, no doubt, experiments and reports will be made.

The report of Dr. Homans is one of the most interesting presented, because it gives an open, frank experience of a large number of most important cases. The variety encompasses nearly all of the pathology which surgical skill has reached. The errors are as openly stated as the favorable achievements, and the conclusions are certainly instructive and most interesting. In speaking of the ovarian practice, I feel the necessity of special comment upon the too frequent practice of removing the ovaries and tubes. The ease with which the operation upon these organs may be performed, and the alleged connection of them with, in many instances, an uncertain symptomatology, induces many inexperienced to venture upon surgical procedures totally unwarranted, and in not a few instances causes results unsatisfactory and sad. The great progress in the removal of abdominal tumors is conceded in Dr. Homan's paper. In an experience of seventy-eight cases I have realized the fact. An experience is the most certain demonstrator of the truth. My first three cases died with the benefit of what I thought was good antiseptic treatment. Clamp practice with the pedicle was employed. Later on the pedicle was tied and dropped, and any doubtful vessel was also tied. In one instance forty-seven ligatures were left in the abdomen, the patient recovering without any serious trouble. Next I cut off the pedicle, tied the vessels, and sewed the opposite membranes when the stump included a broad attachment. Results were also, in this plan of operation, good. The last and best course with me is: No special attention to what is termed antiseptic practice. The patient is well cleaned, the room well ventilated and the sponges thoroughly deprived of any foreign substances. Ether as an anæsthetic. Limbs confined by bandage restraint. Sufficient incision and removal of tumor as conditions may indicate. The three important divisions which I regard are—First, after gaining access to the interior of the abdomen—especially when any adhesions exist—guard against clots of blood forming from the vessels lacerated by the separation of adhered surfaces. This is best and safest done by very large sponges—prepared and kept for the purpose—being introduced between the intestines and the internal abdominal wall, immediately after the opening, and then permitted to remain until the pedicle shall be managed. Second, the pedicle is best managed by clamp and actual cautery. The cooking or destruction of the connecting link between tumor and part embraced between levers of clamp should commence rather high up, say, from two to three inches, and the destruction should proceed slowly down to the floor of the clamp, the wet towels under the clamp limiting the heat to pedicle between the levers. After all of the stump above the braces is removed, great care should be exercised in separating the levers from the stump by an ivory separator, so that no tearing of the stump by the serrated braces can occur. With this careful course completed, the stump is

dropped into the fossa to heal in a short time. Third, the opposing surfaces of the abdominal wound are to be brought together with like tissues, carefully approximated. With this, thoughtfully and thoroughly done, little or no fear need be entertained of umbilical or abdominal hernia. In my seventy-eight cases I saved fifty-nine—the largest rate of mortality being in early practice, when experience was limited. The greater the practice the greater the percentage of success, providing all of the conditions of asepticism and nutrition are carried out.

Dr. DONALD MACLEAN, of Ann Arbor, Michigan, then read the following paper:—

THREE CASES OF LAPARO-NEPHRECTOMY ILLUSTRATED.

TROIS CAS DE LAPARO-NÉPHRECTOMIE ILLUSTRÉS.

DREI FÄLLE VON LAPARO-NEPHRECTOMIE ILLUSTRIRT.

BY DONALD MACLEAN, M.D., L.R.C.S.E.,

Professor of Surgery and Clinical Surgery in the University of Michigan.

The object of this paper is to report, in condensed form, the main facts of three cases of surgical disease of the kidney, along with an account of the operations performed for their relief, the complications which arose in the course of treatment, and the results obtained in each case. The specimens derived from the cases in question are here presented for inspection by the members of the Section:—

CASE I.—MRS. A. A., of Benzonia, Michigan, æt. twenty-five, presented herself at my clinic, at Ann Arbor, in October, 1884. She handed me a letter from her medical attendant, Dr. S. E. Taylor, informing me that she was suffering from an abdominal tumor, which he believed to be ovarian.

Patient was married and had three children, the youngest one year old and just weaned. Her general health was excellent. On examination an abdominal tumor, evidently cystic, was found, large enough to give her the appearance of a woman at the seventh or eighth month of pregnancy. The outlines of the tumor were indistinct, especially toward the upper part, where it seemed to shade off gradually. On the most careful inquiry, no fact or symptom which in any way pointed to disease of the kidney could be elicited; not only so, but the patient positively insisted that the tumor was first observed in the form of a smooth, round swelling in the left iliac region, and that it gradually enlarged *from below upward*. An operation for its removal, by abdominal section, was determined upon, under the belief that the tumor was an ovarian cyst, although it is a fact that I publicly (*i. e.*, to my class) expressed, previous to the operation, serious doubts as to the correctness of the diagnosis. On October 27th, 1884, the operation was performed, under chloroform, in the public amphitheatre of the University Hospital, at Ann Arbor.

On opening the abdominal cavity the great omentum was found very extensively adherent to the front of the cyst and to the parietal peritoneum. This was carefully separated, and, as it bled copiously, ligatures were freely used and a large portion of the lacerated omental tissue was clipped off. The cyst was then tapped and a clear fluid of a dark color drawn off. A mass which had every appearance of being the pedicle of an ovarian tumor, and which was fully believed to be so, was ligated and divided, and the tumor to some extent displaced upward toward the abdominal cavity, where it seemed to be bound down with an unusual degree of firmness. On looking into the pelvic cavity at this stage of the operation, I was surprised to observe the left

ovary, enlarged to the size of a goose egg, and cystic, still in its natural position. It was at once removed after ligation of its pedicle. I then endeavored to trace upward the large cyst, which, owing to the extensive adhesions and from matting together of all the abdominal structures, was a task of extreme difficulty and danger. Nothing in the appearance of the cyst had up to this time suggested a suspicion as to its true nature. It was not until it had, with much trouble, been partially freed from its bed, and a great volume of blood welled up from the deep and irregular cavity, indicating rupture of the renal vessels, that the idea of its being a cystic kidney suggested itself to my mind. The hemorrhage was instantly arrested by grasping the vessels with a strong pair of forceps and immediately ligating them *en masse*. The remaining attachments of the tumor were dense and complicated, but were at length disposed of without injury to any important structures. Free hemorrhage, from many points, required many ligatures. The cavity was very carefully cleansed, all loose tags of tissue were carefully clipped off, and the displaced structures as accurately as possible restored to their natural position. In separating the adherent small intestines from the cyst wall its peritoneal covering was stripped off to a slight extent at three or four different points, but the intestine was not torn. The right ovary was discovered to be cystic and about the size of an orange, and was at once removed.

The uterus was enlarged and congested, so that I suspected an early stage of pregnancy, but later developments showed this suspicion to be incorrect. The abdominal incision was accurately closed with deep silk and superficial horse-hair sutures.

The patient rallied very well from the shock of the operation. Six hours later the catheter was passed and a pint of urine drawn off. The temperature ranged between 99° and 101°, pulse from 100 to 120 for the first few days. She suffered somewhat from nausea and vomiting, but, by the use of small doses of morphia hypodermically, these symptoms were effectually controlled. On the fourth day the silk stitches were removed and the condition in general was favorable. On the fifth day, patient was permitted, by a relative in attendance, to drink freely of cold water, contrary to orders, and serious disturbance of the stomach and bowels followed ; tympanites, vomiting and, finally, free purgation ensued and resulted in serious depression of the vital forces. About this time symptoms of irritation appeared at the points of suture. The temperature was 101° and the pulse 100. November 6th, temperature 103°, pulse 120, pus of an unhealthy character oozed from several of the suture openings, and a probe passed through one of these entered the abdominal cavity. The douche was freely used, and large quantities of pus washed out, after which the temperature and pulse fell to nearly normal, and the general symptoms were decidedly improved. Quinine and stimulants were freely administered. On November 13th the discharge from the abdominal cavity was very abundant and contained *fecal matter*. The intestines had evidently given way at one or more of the points from which the peritoneum had been stripped at the operation. The temperature at this time was 104°, pulse 120, and the general appearance unfavorable. This alarming state of affairs continued, with very little change, till the 18th, when the quantity of the discharge was greatly diminished, its fecal character gone, the temperature 100°, and the pulse 110. From this time on everything progressed favorably, and, December 20th, patient was permitted to leave the hospital, although at that time a small opening remained, from which a slight discharge of healthy pus flowed. A letter from the patient, dated January 27th, 1885, announced her complete recovery. The sinus closed immediately after the escape from it of a silk ligature an inch in length. A letter dated just one year from the date of operation reported the patient in excellent health, and recorded the fact that she had just taken the first prize as the best equestrienne at the county fair !

It will be seen that the cyst, as I now present it for your inspection, retains, to some extent, the general form of the original organ. In the interior, at the time of

operation, a mass as large as a walnut was discovered, which, on examination afterward, was found to consist mainly of oxalate of lime. One fact in the history of this case remains to be told. Some days after the operation the patient's mother arrived at the hospital, and on questioning her as to her daughter's history, she stated, unequivocally, that when the patient was four years of age she had a severe fall from a swing, which caused great pain in the left side, and that for some days following the accident, the urine contained blood. She further stated that, for a good many years afterward, patient suffered from occasional attacks of severe pain in the region of the left kidney, and at those times passed blood in her urine. Had the patient herself been able to give us the benefit of these facts at first, it is not improbable that nephrectomy would have been the operation determined upon at first instead of ovariotomy. At all events it is but reasonable to suppose that a very direct relation existed between that old injury and the condition which was found at the time of the operation.

CASE II.—Mrs. F. T., æt. 40, of Otsego, Michigan, wife of a farmer, applied to me in May, 1886, on account of a tumor of the abdomen on the left side. Her health had generally been good; she menstruated at the age of 14; at 16, suffered from strumous enlargement of the glands of the neck; married at age of 20; has had three children. About six years ago, patient discovered an enlargement about the size of an egg, the locality of which was described by her physician as "too high for the ovary and too low for the spleen." At the time of the discovery of the tumor she describes her condition as one of general malaise, soreness, chills and general prostration, and she says that this condition has continued more or less ever since. The tumor has grown much more in the last year than ever before, and has led her to consult physicians regarding it. On admission, the pulse and temperature were normal; digestion good; bowels regular; urine normal; menstruation regular. Family history—Father died of apoplexy, æt. 72; mother still living, æt. 72, healthy; *one sister died of what was said to be a malignant tumor of the abdominal cavity*; four brothers, three living and healthy; one died of apoplexy; uncles and aunts on both sides phthisical. On examination a large tumor was found in the abdomen, mainly on the left side, but extending all the way across to the opposite side. It was lobulated and on palpation seemed to be semi-solid. Its boundaries were pretty distinctly defined toward the right side and above, but to the left the outline was lost toward the renal region. No connection with uterus or ovaries. A tumor, soft, solid or cystic, of the left kidney was diagnosed with a great degree of confidence, and its removal by abdominal section promptly determined upon. The operation was performed on May 31st, 1886, at the Detroit Sanitarium. The size of the mass and the uncertainty as to its relations and attachments led me to prefer the transperitoneal method. The tumor was exposed by an incision in the middle line; when the two layers of the peritoneum were divided the tumor was exposed and found to be cystic and lobulated. An effort was made to deliver the tumor entire, and this was successful except that at the very moment of delivery a rupture occurred permitting the escape of a great quantity of material which closely resembled soft-soap; none of this material was permitted to enter the abdominal cavity, which was very carefully guarded with towels and sponges. The renal vessels were secured with a catgut ligature. The ureter was found filled with the same kind of material which was contained in the cyst itself. This was carefully pressed out and the ureter tied with a catgut ligature. The posterior layer of peritoneum was drawn together, like the mouth of a purse, and securely tied with a catgut ligature. The abdominal cavity required very little cleansing. The incision was closed as usual with silk and horse-hair sutures. After the operation the case progressed very favorably, although unpleasant symptoms, such as pain, vomiting, fever, etc., were not altogether wanting. Flatus passed on the second day. The bowels moved with aid of enema on the fourth day. Menstruation began on the fifth day, and on that day all the sutures, silk and horse hair, were removed. The progress of the case

was regarded as quite satisfactory up to the eleventh day. The temperature ranged between normal and 101°, the pulse between 100–110. On the day mentioned, the pulse and temperature began to go up, and the general appearance of the patient became proportionately unfavorable. This continued until the 18th day, when the pulse was 140 and the temperature 104°. Of course suppuration was suspected. Examination of the anterior abdominal surface was entirely negative, there was no pain and no tympanites; fluctuation was, however, detected in the lumbar region, and a free opening evacuated a large quantity of very fetid pus. Urgent symptoms of collapse followed this operation, and the most active means of stimulation had to be applied. Toward morning improvement was noted, and at 6 A. M. she felt much better, temperature 101.8°, pulse 120. From this time forward the progress was all that could be desired. The abscess cavity was washed out daily with disinfectant lotions, and it gradually closed up. On July 5th, 1886, she left for home, and when heard from, a few days ago, she was in excellent health. An examination of the specimen obtained from this case will show that no trace of kidney tissue remained, the whole of the organ having become degenerated into this large multilocular cystic mass. When, how or why this process took place or commenced to take place nothing in the history enables us to even surmise.

CASE III.—Nettie W., æt. 22 months, brought to me March 1st, 1887, by Dr. G. R. Richards, of Detroit. At that time the child's health seemed to be unimpaired. The family history was found, on inquiry, to be exceptionally good in every direction. My attention was invited to a tumor in the right hypochondriac region, which was large enough to give the child's figure a peculiarly prominent appearance. In regard to this growth, Dr. Richards informed me that it had been first noticed in November, 1886, when it seemed to be about the size of an egg, free from pain or tenderness, and distinctly movable. At that time the child was suffering from whooping cough, and the doctor recommended the application of a broad abdominal bandage, to counteract the effects of the strain on the muscles, and to afford support to the tumor; no other treatment was advised. On examination, on March 1st, we found a tumor well defined, movable, free from tenderness and large enough to cause the peculiar appearance of the child's figure, which has already been mentioned. Although the tumor was neither painful nor tender, the parents informed us that the little patient occasionally exhibited signs of general distress, and more especially that she had great difficulty in lying down and rising up. A tumor of the kidney was diagnosed and its removal strongly recommended. The operation was performed on March 8th, 1887. The trans-peritoneal method was selected on account of the relatively large size of the tumor. Chloroform was the anæsthetic used. The incision was made just external to the rectus abdominis muscle. No difficulty of any kind was met with in the operation, which occupied but a few moments in its performance. The vessels and the ureter were ligated *en masse* with a carefully prepared silk ligature. The posterior peritoneum was very carefully closed with catgut sutures, and the abdominal incision by silk and horse-hair sutures. The dressing consisted of iodoform gauze and borated absorbent cotton. The shock proved to be comparatively slight, and reaction was all that we could wish. Camphorated tincture of opium in small doses effectually controlled pain and restlessness, and although the temperature was upward of 100° from the very first, still for several days the general appearance of the patient was most promising. March 13th, the fifth day, all the stitches were removed, and the wound seemed to be completely and firmly healed. Flatus passed freely and the stomach was able to retain food, still the temperature was 103° and the pulse 120. There was no tympanites and no local signs of irritation. A broad strap of adhesive plaster, and a broad flannel bandage were used to give efficient support to the abdominal walls. On the 15th, the temperature was 104° and the pulse 150, and still the little patient was bright and cheerful. The dressings were all removed for the purpose of making a thorough investigation and determining, if pos-

sible, the nature and locality of the maintaining cause of the high temperature. While this was being done the little patient, who was quite calm and good natured, suddenly gave a violent cough which forced the abdominal incision to part throughout its entire length, permitting the omentum and small intestine to protrude through the opening. I instantly replaced the viscera and held the edges of the incision in contact until chloroform was administered, when I reapplied the sutures. Before the wound was closed, however, I took the opportunity to make a most cautious and yet thorough examination with the view of determining, if possible, whether any local condition existed which might account for the persistent constitutional irritation as indicated by the high temperature and the rapid pulse; nothing could be detected which in the least degree tended to clear up the mystery or to furnish indications for active interference. Very slight symptoms of shock followed this unexpected complication, and the lips of the wound seemed to reunite readily as before; still the general condition did not improve, the temperature ranged between 102° and 105°, and the pulse was proportionately rapid. Quinine and opium suppositories were administered as the symptoms seemed to demand, and every effort was made to sustain the patient's strength by the administration of concentrated nutriment by mouth and rectum. Tympanites was added to the other unfavorable conditions. The strength gradually failed, and the child sank and died, exhausted, on the morning of the 17th, the ninth day after the operation. At the autopsy, which was very carefully conducted by Dr. W. G. Henery, the following facts were noted :—

First.—In the abdominal wound union of the lips had taken place.

Second.—The intestines were distended with gas.

Third.—Slight inflammatory changes had occurred in the great omentum and in the small intestine in the right hypochondrium. At this point the folds of intestine and the omentum were glued together by very feeble adhesions.

Fourth.—The posterior peritoneum, which had been sutured with catgut at the time of the operation, was found entirely healed. It was, however, discolored and thickened by inflammatory action.

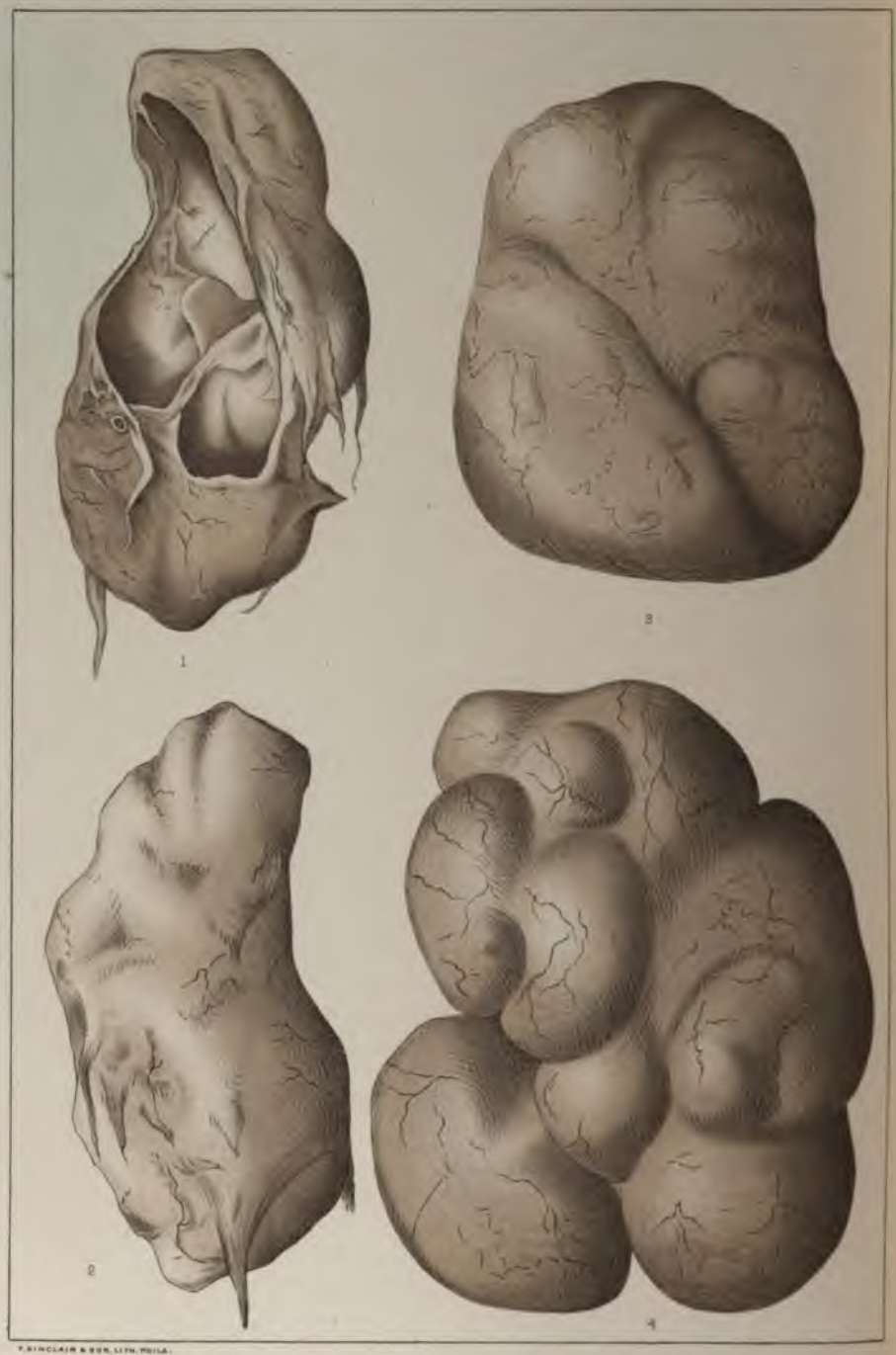
Fifth.—On slitting up this layer of peritoneum and exploring the bed of the tumor behind it, a very small quantity, perhaps one drachm, of thick, dark-colored pus was found surrounding the ligatured pedicle of the tumor. The most careful examination failed to discover any additional morbid condition.

From the three cases here described (which, it is only right to state, constitute the *sum totum* of my personal experience with nephrectomy), the following deductions seem to me to be rational and practical :—

First.—The great difficulty, if not absolute impossibility, of arriving at a certain diagnosis in all cases of tumor of the kidneys, is well illustrated by the case of Mrs. A., the first of the series.

Second.—All three cases go to show that although the lumbar operation may very justly be regarded as the safer method, and should always be performed where admissible, still there is a class of cases in which the trans-peritoneal operation is the only one open to the surgeon. I think that when the size and relations and history of the tumors here recorded are considered, the propriety of the method of operating which was followed will meet with general approval.

Third.—All three of these cases serve to impress very forcibly the paramount importance of furnishing efficient drainage, and that irrespective of the thoroughness with which antiseptic precautions are used. In Case No. 1, owing to the extensive laceration of the posterior peritoneum, the best if not the only possible means of drainage would have been by a counter-opening through the lumbar or posterior wall of the abdomen. Whether this precaution would have prevented the dangerous suppuration and the fecal complication with which we had to contend, I am not prepared to say,



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Laparo-Nephrectomy. (McLean)

but there is reason to believe that the after-treatment would have been by that means materially simplified and recovery hastened. In the other cases we might have resorted to this expedient of a counter-opening, or we might, with even greater advantage, have resorted to the method of stitching the edges of the wound in the posterior peritoneum to the edges of the anterior abdominal wound, at the same time inserting a drainage tube into the cavity left by the tumor, having its outer extremity projecting through the abdominal incision. This precaution might have facilitated recovery in Case No. II, and in Case No. III it might have saved the patient's life.

Fourth.—Case No. I is especially worthy of note as illustrating the extent of mutilation (one kidney, both ovaries, major part of great omentum) and the aggravated complications which may be recovered from in the abdominal cavity, and that, too, when antiseptics have been omitted, or, at any rate, when they have signally failed of their usefulness, as we all know that they will sometimes do even in the most skillful, careful and experienced hands.

DISCUSSION.

Dr. F. LANGE: There can be no doubt that in a small percentage of cases nephrectomy must be performed by abdominal section in front. Cases of large hydronephrosis or of neoplasms of such size that lumbar incision is insufficient for their removal, belong to this class. In all cases, however, of moderate enlargement of the organ, and especially in those which, by the existence of infectious material within the diseased organ, justify the apprehension of possible infection of the peritoneal surface during the act of operating, I should give preference to the lumbar incision. I have satisfied myself in several cases of pyonephrosis that good-sized tumors, by preliminary nephrotomy might be so much reduced in size that after a couple of weeks their removal by lumbar incision, eventually connected with exsection of the distal portions of the last two ribs, becomes feasible. The best after-treatment in such cases is the open, antiseptic treatment, with loose tamponade by iodoform gauze. I have heretofore removed a kidney seven times, always by lumbar incision. My first two cases died in consequence of the operation. The last one, in which the operation was done for tuberculous disease, died four weeks later, from progressing tuberculosis of the bladder and kidney of the other side. The operation apparently did not in any way speed or cause his death. I did this operation, because, from the evident thickening of the ureter near its entrance into the bladder, I concluded the corresponding kidney to be similarly diseased, and because two other cases, in which I had removed a kidney, though being conscious that the other side was affected to a certain degree, had taught me that the operation could be done in such cases and would to a great degree diminish the patient's suffering and prolong his life. Both these patients are alive, the one two years, the other three months, after the operation. It seems that a diseased condition of the other kidney, if the same be not too far advanced, does not absolutely contraindicate nephrectomy. In this respect further experience must be gathered in order to lay down rules. This is not the proper place to give the histories of my cases. The majority of them have been published before.

Let me only add a few words about the best methods of exploratory incision of the kidney. In 1885 I had published in the *Annals of Surgery* a short article, in which, from the anatomical study of crosscuts of the frozen cadavers, I recommended, in order to get the most easy access to the pelvis of the kidney, to proceed along the posterior surface of that organ. I take the opportunity to call your attention to the fact, that this is the shortest and most easy way. The patient, of course, must be in the prone position, with slight inclination toward the side of the operation, and

a small pillow pushed under his stomach. I have since always adopted this method, and find it useful and justified by the anatomical condition of the parts. Allow me to refer in that respect to my remarks at the last Surgical Congress in Berlin.

At last I should like to add one practical hint for the extirpation of the kidney. Cutting across the ureter as early as possible, eventually after ligation, will make the organ more movable and allow of more easy access to and safe handling of the vascular pedicle.

Mr. EDMUND OWEN, F. R. C. S. London, remarked that there were several factors which must necessarily influence the choice of the operation. If there were doubts as to the nature of a supposed renal tumor, the trans-peritoneal operation must be adopted; so, also, if the tumors were very large, though he thought tumors of a larger size than was generally imagined could readily be removed by the interval between the last rib and the iliac crest. Then the special line of practice of the surgeon himself must necessarily have a great influence on the choice of the operation. The gynæcological surgeon, for instance, would probably in almost every case adopt the anterior operation, while the general surgeon would lean rather to the posterior operation. In the case of renal tumor with perinephritic suppuration, the posterior wound must be chosen on account of aseptic and drainage precautions. When the anterior operation was adopted the *linea semilunaris* answered well for the incision, and care should be taken to reach the kidney round the outer side of the colon, so that there might be no risk of wounding the colic, or sigmoidean artery. Mr. Owen then briefly narrated a case in which he successfully removed a diseased kidney by the anterior operation.

Dr. F. HERFF, of San Antonio, Texas: After listening to the very elaborate paper of Dr. Maclean, I hesitate to enter into a discussion regarding nephrectomy. I have only performed that operation twice, and there are hundreds of cases collected from which better conclusions in regard to the methods, whether by lumbar or by abdominal incision, can be drawn than from my two observations. I may, however, state my belief that lumbar nephrectomy, while the more rational method, is only feasible in cases where the kidney has not attained a very large size or where it is not too much loosened from its attachment and forms a floating tumor.

I further believe, that in suitable cases the abdominal operation is the easiest, and in fact not more difficult than the average ovariotomy, and that it is hardly more dangerous.

All those points, however, have been discussed before, and I should not waste your time by repeating them, but one of my cases presented such exceptional features that I may be pardoned for relating it briefly.

In 1883 I was consulted by a lady, young, married and two months pregnant, for a tumor on the right side of the abdomen. It had the size and shape of a kidney, was very movable and was diagnosed as a floating kidney.

The lady had no pain and was in good health, and I advised her to wear a support and not have the tumor meddled with.

In 1885, some time after her delivery, she consulted me again. The tumor had increased to the size of a child's head, was very troublesome and prevented her from work. Her health was failing and she insisted upon an operation, which I performed June, 1885.

The incision into the abdomen was made long, extending beyond the umbilicus. Several adhesions to the omentum and intestines were easily detached, the tumor enucleated from the peritoneal covering and both vessels and ureter ligated. A

long drainage tube was inserted into the pouch left by the enucleation and led out at the lower corner of the abdominal incision.

No bad symptoms disturbed the re-convalescence and she left, completely cured, after three weeks. In fact, she gave me less trouble than all my hysterectomies and many of my ovariotomies. She has since borne a child, and I saw her only a few days ago, in perfect health.

The remarkable part of the case is the origin and histological character of the tumor.

The kidney formed the lower end of the tumor and was perfectly normal in size and structure, with the exception of a small part of the upper end, which was adherent and constituted a part of the neoplasm.

The new growth originated in the supra-renal capsule, and was in fact a degeneration of that organ. This could plainly be proved by the structure and characteristic pigmentation of the supra-renal capsule still traceable in parts of the tumor.

The new formation exhibited the microscopic appearances of a sarcoma and was classified under the microscope as the round-cell variety. In the middle of it softening had begun and a part of it was transformed into a bloody pulp, such as we see often in sarcomas at the period of sloughing.

It is reasonable to expect that after two years of perfect health the disease will not return, which fact sustains the views of those who classify sarcoma as a disease, which is not always malignant and not unfrequently cured by early removal.

I never have read an account of the removal of the supra-renal capsule, where the kidney was sound, and will add that the preparation is still in my possession.

Dr. DAWSON, of Cincinnati: I arise merely to congratulate Dr. Maclean and the Section upon the value of this paper. It is another evidence of the advance of abdominal surgery.

I very much regret that we could not have had the personal history of Dr. Dunlap's connection with abdominal surgery. In 1843, in Ohio, at that period far from what it is now in general intelligence and medical scholarship, Dr. Dunlap was called to see a lady suffering from an abdominal tumor. She had been tapped time and again, but a large mass still remained after each operation. The lady insisted upon Dr. Dunlap removing this morbid growth, and said if he did not do so, she would send for a butcher and have him do the work. She had heard that a woman similarly affected had been relieved.

The only literature which Dr. Dunlap could find was a short article in "Cooper's Surgical Dictionary" condemning the operation. He, however, yielding to the urgent solicitations, made a large section, removed the mass, his patient living twenty days, when she died from an intercurrent affection.

I thus refer to Dr. Dunlap, to show that he was almost as much of a pioneer in abdominal surgery as McDowell. They were the forerunners, the John the Baptists, beginning the way which has finally led us up to such results as we see to-day, the best fruits of which are here found in Prof. Maclean's cases—one life at least saved.

Dr. HINGSTON said that he had precisely the same number of cases as Dr. Maclean, and while he agreed with the reader in nearly all he had said, he differed from him as to the mode of removal. He preferred the post-perineal method when it is possible to remove the tumor in that way. Two of the cases were fatal; one recovered. The fatal cases were abdominal section. The tumors could not be removed in any other way. One was solid, occupied the whole abdominal cavity and rested on the brim of the pelvis. The patient died from shock.

The second operation through the anterior abdominal parietes was for a hydronephrotic kidney of the left side. There, also, then was no possibility of removing it in any other way. The patient died within twenty hours.

The other case—the second in point of time—was a post-peritoneal one. The diseased kidney, though small, was removed in order to relieve the patient's sufferings. Room was found between. The patient, a young woman, had made an excellent recovery, and is to-day in excellent health.

The first operation (fatal) was performed upward of thirteen years ago; the second (favorable) in 1885, and the third (fatal) in 1887.

As the second case was so constitutionally satisfactory, so little disturbance, it appears that the post-peritoneal method is preferable and should be performed when practicable.

The following paper was read by Dr. TRICOMI, of Rome, Italy :—

LE MICROÖRGANISME DE LA SUPPURATION AIGUË.

THE MICROÖRGANISM OF ACUTE SUPPURATION.

DER MIKRO-ORGANISMUS DER AKUTEN EITERUNG.

PAR LE DR. ERNESTO TRICOMI.

Plusieurs chirurgiens, pathologists et bacteriologists se sont beaucoup occupés, depuis dix ans, des microörganismes des suppurations aiguës.

Ogston, Rosenbach, Passet et plusieurs autres ont trouvé différentes espèces de microörganismes piogènes. Pendant quatre ans j'ai examiné plus de 200 cas d'abcès aigus, 20 cas de flegmons, 10 cas de furoncles et un grand nombre de suppurations des plaies traitées antiseptiquement, mais dans lesquelles le pansement avait failli, et j'ai trouvé constamment une espèce de microörganisme piogène, qui dans les préparations microscopiques se présente en forme de chaînes, de grappes, ou même isolé ; mais les cultures sont toujours les mêmes par leurs caractères semblables à celui décrit par Rosenbach sous le nom de "staphylococcus pyogènes aureus."

En faisant des cultures fractionnées je n'ai réussi qu'à isoler toujours la même espèce, parce que j'ose dire, malgré les recherches de Ogston, Rosenbach, Passet, etc. que l'abcès aigu, le flegmon et le furoncle sont des maladies infectieuses produites par une seule espèce de microörganisme, c'est-à-dire sont des cultures nettes de micrococcus pyogènes aureus.

Il faut chercher la raison des différentes formes du même procès dans la constitution des malades et dans les différentes régions qui en sont le siége.

Dans la région antérieure du cou et dans les membres régions riches de vaisseaux lymphatiques, il est plus facile de trouver un flegmon ; dans la région postérieure du cou, ou sur le dos, régions peu riches de lymphatiques où la peau est très épaisse, il est plus facile de trouver le furoncle ; l'abcès, on le trouve fréquemment où sont les ganglions lymphatiques p. e. aux régions aisselaires, inguinales, etc.

J'ai cherché la porte d'entrée des microörganismes, et il n'est pas rare de la trouver dans la bouche, dans les suppurations de la région antérieure du cou, dans les membres dans les suppurations aisselaires ou inguinales et d'apprendre qu'une action mécanique a précédée dans la région postérieure du cou toutes les fois qu'on y trouve un furoncle.

Les injections de pus ou de cultures dans le connective subcutané des animaux

inférieurs (lapins, cochons d'Inde, souris, rats, etc.) donnent naissance à un abcès chaud, à une maladie tout à fait locale, et si l'on fait des blessures sur le dos de plusieurs lapins on peut bien observer que celles traitées par du pus ou par des cultures sont retardées dans leur guérison, à cause du procès de fermentation qui altère celle de l'organisation.

Dans le sang des individus vivants, affectés d'abcès, de flegmon ou de furoncle je n'ai jamais trouvé microörganisme, la même abscence je l'ai trouvée dans les individus morts de flegmon et dans les animaux inférieurs tués en différents intervals après l'inoculation.

L'examen microscopique de l'abcès spérimental montre d'abord une infiltration cellulaire d'éléments ronds et ensuite un tissu connectival jeune, un vrai granulome, que limite le pus, et on voit des microlibres, d'autres dans les globules de pus et un grand nombre disposé sur la parois intérieure.

Dans les préparations microscopiques des parois du flegmon on voit un grand nombre de chaînes de micrococci dans les lymphatiques du derme, mais plus abondants dans le connective subcutané, où les cellules adipeuses sont infiltrées de micrococci. Les cellules fixées de connective sont quelquefois appliquées sur les fasciaux et autres fois sont libres. Les vaisseaux sont dilatés et les tissus remplis de cellules lymphatiques.

Mes recherches ont été faites d'après les méthodes de Koch.

Le microörganisme piogène ne produit pas la septicemie ou la piœmie ; en effet, les injections faites dans la connective subcutané, dans les veines ou dans les cavités séreuses des animaux inférieurs ne donnent qu'une maladie locale ; il est aussi différent de streptococcus érésipèlatus non seulement par la culture, mais encore par les expériments d'infection.

Le streptococcus érésipèlatus ne fond pas la gélatine de Koch, et inoculé dans les oreilles des lapins donne naissance à une inflammation simple étendue spécialement le long du cours des veines ; le micrococcus piogène, au contraire, fond rapidement la gélatine et inoculé produit une inflammation purulente peu étendue.

Si l'on injecte dans une oreille d'un lapin une petite quantité de culture de streptococcus érésipèlatus et depuis 24 heures une culture de micrococcus piogènes, on peut voir que l'inflammation purulente est plus étendue de celle de l'oreille opposée où l'on a injecté, par contrôle, la même quantité de micrococcus piogènes.

Si, au contraire, on injecte dans une oreille d'un lapin le micrococcus piogène et après 24 heures le streptococcus érésipèlatus, on peut voir que l'érésipèle est plus étendue de celle de l'oreille opposée, où l'on a injecté par contrôle la même quantité de streptococcus érésipèlatus.

Le mélange des deux microörganismes produit, inoculé dans l'oreille, le même effet que dans le dernier cas, et on peut, par les cultures fractionnées isoler les deux parassites ; si, au contraire, l'injection a été faite dans les muscles on obtient alors seulement une inflammation purulente sans pouvoir isoler le streptococcus érésipèlatus.

Je crois, Messieurs, que tous ces faits expliquent beaucoup des problèmes qu'il y a encore dans l'érésipèle en relation avec la suppuration. L'érésipèle est une inflammation simple produite par un microörganisme déterminé, elle devient purulente par l'introduction d'un autre microörganisme lequel arrive ou par la même solution de continuité par laquelle est pénétré le streptococcus érésipèlatus, ou par les écorchures épidermoidales, que sont ainsi fréquentes dans l'érésipèle.

De mes recherches donc il résulte que dans les abcès aigus, dans les flegmons et dans les furoncles il n'y a qu'une espèce de microörganisme, toujours la même, laquelle inoculée dans les animaux inférieurs ne produit pas septicemie, piœmie ou érésipèle, mais une inflammation purulente locale.

Dr. DENNIS, of New York, read the following paper:—

A REPORT OF A SUCCESSFUL CASE OF AMPUTATION AT THE HIP JOINT FOR REMOVAL OF A SARCOMA OF THE THIGH, WITH REMARKS.

RAPPORT D'UN CAS HEUREUX D'AMPUTATION À L'ARTICULATION DE LA HANCHE POUR L'ELOIGNEMENT D'UN SARCOME DE LA CUISSE, AVEC OBSERVATIONS.

BERICHT ÜBER EINEN ERFOLGREICHEN FALL VON HÜFTGELENK-AMPUTATION ZUR ENTFERNUNG EINES OBERSCHENKEL-SARKOMS; MIT BEMERKUNGEN.

BY FREDERIC S. DENNIS, M.D.,

Of New York City.

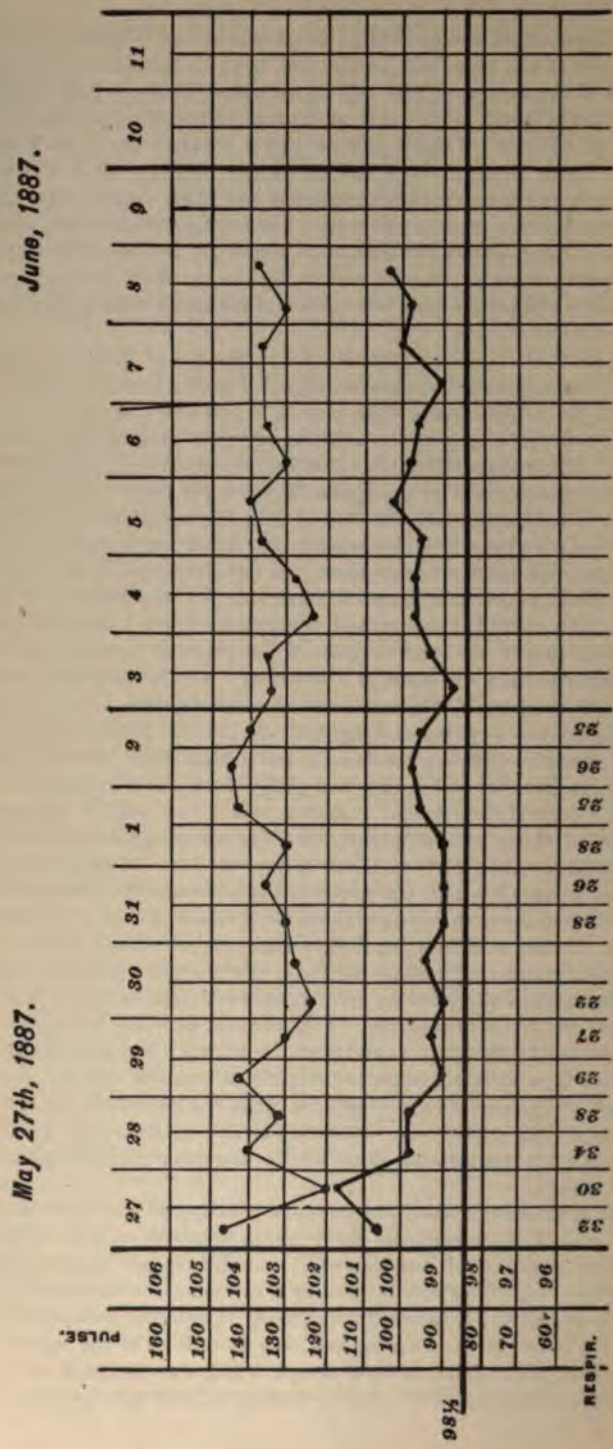
In selecting one of my cases of amputation at the hip joint for the removal of a malignant growth, it is not my intention to enter upon a discussion of the subject of sarcoma of the long bones. It is rather with a view of placing a successful case upon record, for future reference and study, that this case is now published. The few cases hitherto reported have not enabled the surgeon to arrive at any positive conclusion, and it is only by collecting and studying the cases to be published from time to time, together with those already reported, that any information of positive value can be obtained. The object of the writer is also to provoke a discussion upon this most interesting topic, and thus induce surgeons to report their cases and to offer any remarks upon the subject that may be of interest in studying the nature of these malignant growths affecting the femur. This subject becomes at once of great importance when the startling clinical fact is mentioned that out of twenty-eight cases of sarcoma of the thigh only two were alive a year after an operation had been performed for the removal of the tumor. The following case was sent to me through the kindness of Dr. Johnson, of Blairstown, N. J.

Walter J. Rutan, æt. 17, was admitted to the Harlem Hospital May 19th, 1887. A careful investigation into his family history failed to reveal any hereditary tendency to malignant disease. An examination into his personal history was also negative. He states that he has always enjoyed good health and weighed, until recently, one hundred and twenty-five pounds. At the time of his admittance into the hospital his weight was about eighty pounds and his general condition was poor and anæmic. He had rapidly lost flesh and his general appearance was that of one suffering from malignant disease. In January, 1887, five months prior to his admittance to the hospital, he states that he first experienced a dull, aching pain in his left thigh. This pain would continue at intervals of from one to six hours, and it was more severe at night. Shortly after the onset of the pain he noticed a swelling in the middle of the thigh corresponding to the painful part, and this swelling rapidly increased. He gives no history of a fall or blow, and states that the pain appeared spontaneously. During the middle of March, or about two months after the appearance of the first symptoms of pain, he consulted a physician, who aspirated the swelling and drew off about ten ounces of bloody fluid. The aspiration of the tumor was followed by a slight temporary relief, probably from the removal of the cause of tension. He continued, however, shortly afterward, to again experience pain in the part and the swelling began again to increase. The pain was relieved by hypodermic injections of morphia until he was seen by Dr. Johnson, in May of the same year. At this time he was brought to me by Dr. Armstrong from Dr. Johnson. Upon admittance to the Harlem Hospital, in New York City, he presented the general appearance already described. Upon examination of the thigh a large tumor

was apparent. The swelling was fusiform in shape with a slight bulging upon the inner side. The skin over it was tense and purple, and large tortuous veins could be seen over the surface of the tumor. There was no œdema of the leg or foot. The tumor measured ten inches in length and about twenty-seven inches in circumference at the point of greatest prominence. Palpation of the tumor revealed the signs of softening in certain parts. There were points of fluctuation over different parts of the thigh. The glands in the inguinal region corresponding to the thigh were slightly enlarged. The enlargement was evidently not a metastasis, but a sympathetic or irritative swelling of the glands. The diagnosis was sarcoma of the thigh, and amputation at the hip joint was proposed as the only measure to save life. A thorough examination of the lungs, heart, kidneys and other organs was made and nothing was revealed to interdict the operation.

On May 26th, 1887, about five months after the appearance of the first symptoms of pain in the thigh, the amputation was performed, ether having been administered. Dr. Garmany applied the rubber elastic tube, passing it just above the crest of the ilium and just internal to the tuberosity of the ischium, after the manner suggested by Dr. Jordan Lloyd, of Birmingham, England. The leg was previously held elevated for five minutes, but no elastic bandage was applied to the leg or thigh. The simple appliance mentioned entirely controlled the vessels, with the exception of the obturator artery, which was at once ligated, so that scarcely any blood was lost from the stump and the operation was practically bloodless. I cannot commend too highly this simple, safe and efficient method of completely arresting the hemorrhage in amputation at the hip joint. The method of Langenbeck was employed, and I transfixed the upper flap, and in doing so cut the anterior part of the capsular ligament of the hip joint, which exposes to view the head of the femur. Dr. Truax placed his fingers under the anterior flap, as it was cut from above downward under the femoral vessels. As soon as the flap was cut upward Dr. Truax firmly compressed the femoral and its branches between his fingers, and forceps were applied at once to the femoral and its branches as the flap was pulled back above Poupart's ligament. I then completely divided the capsular ligament, seized the head of the bone with my left hand, pulled it forward sufficiently to allow the amputating knife to pass behind the bone and parallel with it until the femur was free. A single stroke of the knife from behind forward liberated the posterior flap, and the entire limb was removed. Dr. Manley then seized the open mouth of the divided vessels, and in a few minutes all the vessels were ligated, and thus any hemorrhage was prevented. The wound was dressed in an aseptic manner with catgut ligatures, catgut sutures, and a few silver ones to support the flaps; also drainage tubes were introduced. The dressing was applied under irrigation of bichloride of mercury. A thin film of iodoform was sprinkled over the wound and antiseptic dressings placed over the stump. The patient rallied after the operation, and his pulse, temperature and respiration, together with his general condition, were in every way satisfactory. Upon the sixth day following the operation I dressed the stump. The tubes were removed, the line of union was complete and primary. I append the temperature chart for the fourteen days following the amputation. (See next page.)

He had made a most satisfactory recovery, and his appetite was excellent; in fact, he was apparently well, when suddenly the dressings appeared stained and were foul smelling. Upon removal of the dressings some considerable pus appeared from the cavity of the acetabulum. This was followed by a severe hemorrhage on the twenty-first day after the operation. I removed the dressing, opened the stump in the centre and found that the source of the bleeding was from the bottom of the acetabulum. I found in the bottom of the joint cavity a slough, which was the remains of the ligamentum teres. This slough was evidently the starting point of the abscess and also the


May 27th, 1887.
June, 1887.
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cause of the secondary hemorrhage. The bleeding was from the artery accompanying the ligamentum teres. The acetabulum was plugged with iodoform gauze and the gauze was removed in a few days. The patient did well, and left the hospital with the wound closed except a small sinus leading down to the bottom of the acetabulum.

The tumor was sent to the Carnegie laboratory, where it was carefully examined by Dr. Frank P. Grauer, who returned to me the following report:—

I. THE MICROSCOPICAL APPEARANCE OF THE TUMOR.

Large, soft, hemorrhagic tumor, fusiform in shape, situated on the anterior surface of the thigh and involving the lower three-fourths of the thigh. The length of the tumor is ten inches and five inches in depth to the bone. The base of the tumor was from the shaft of the femur. The circumference, after the growth had shrunken in alcohol, was about seventeen inches.

II. THE MICROSCOPICAL APPEARANCE OF THE TUMOR.

A section of the bone from the shaft of the femur in the centre of the tumor shows the following changes after staining the specimen with hæmatoxylon and eosin. With a low magnifying power deeply blue stained masses resembling small islands, so to speak, are seen situated among a large number of granules. With a higher power the blue masses are found to be composed of bone cells, and the small granules to be nuclei of round cells. These were the only form of cells present in this section. At the same time a small amount of connective tissue was found in the section, probably the remains of the periosteum. A section of the bone at the edge of the tumor shows that the periosteum is not involved; but that the round cells begin beneath the periosteum in its osteogenetic layer and extend into the bone tissue, where the changes, although more recent, resemble those found in the section of bone from the centre of the tumor. Different pieces of the tumor were taken, viz.: four and two inches from the periphery, also one at the periphery. These specimens were examined in order to follow the changes that occurred. The first specimen, viz.: four inches from the periphery, shows it to be composed of round and spindle cells, the latter predominating, and in one spot the beginning of a mucoid degeneration. The second specimen, viz.: two inches from the periphery of the tumor, is found to be very vascular and contains a large number of blood vessels in it. The mucoid degeneration is more advanced than in the preceding specimen. It is composed of round and spindle cells, and in certain parts of the field a structure is seen typical of an alveolar sarcoma, viz.: stroma, alveoli and cells.

The third specimen, viz.: from the surface of the tumor, is not as vascular as the preceding, and it is composed of round and spindle and giant cells. The mucoid degeneration is also well marked. At the same time the periosteum is preserved intact with a few bone cells.

A study of these changes shows the origin of the tumor to be sub-periosteal. The periosteum has been pushed forward in front of the tumor and the membrane is seen in the periphery of the growth.

The diagnosis of the tumor from a microscopical examination is a sub-periosteal mixed-cell sarcoma.

Remarks.—The points of special interest in this case are:—

First, the presence of a sarcoma in a boy in perfect health with no hereditary taint.

Second, the absence of any exciting cause to explain the development of the malignant tumor.

Third, the great rapidity of the local growth in five months, and the severe general constitutional disturbance.

Fourth, the absence of any metastasis in any of the organs or glandular system.

Fifth, the marked rapid convalescence and return to health in a few weeks after the removal of the sarcoma.

Sixth, The occurrence of a secondary hemorrhage in the artery accompanying the ligamentum teres.

Seventh, The unfavorable prognosis for operation in his enfeebled condition, and the present prospects of a complete cure as a result of the amputation.

Eighth, The positive diagnosis, from the microscopical examination, as to the *origin* of the growth, by sections made, beginning at the periphery of the tumor and extending to the bone.

Ninth, The correspondence between its soft, fluctuating feel and its very rapid growth and consequent malignancy.

Tenth, The infection limited to the shaft of the bone, while both extremities were entirely free from the disease, thus simulating chronic inflammation of the diaphysis, and forming, in respect to site, an exception to the general rule.

Eleventh, The absence of a spontaneous fracture of the femur with extensive disease of the diaphysis.

DISCUSSION.

In discussing Professor Dennis' paper, Dr. MYERS remarked that MacEwen used only digital pressure in the amputation of the thigh joint.

Professor MACLEAN, of Detroit, Michigan, said that Dr. Dennis' method was the principal point in the paper; he had had the best results in Lister's abdominal compressor.

Dr. WEEKS, of Maine, stated that he used the ordinary rubber bandage with a compress, the bandage being four thicknesses and secured much the same as Dr. Dennis's.

Dr. MANLY, of New York, thought it difficult to find anything to take the place of Lister's compressor.

Dr. REYHER, of St. Petersburg, Russia, said he had had some unpleasant experiences with Listerism. He was in the habit of first excising the head of the bone, then piercing the thigh and ligating in two parts, then amputating the legs; the flaps were then under the control of the surgeon, and he could work at leisure, but he usually put a rubber bandage above.

Dr. N. SMITH thought that the less tissue left the better; he was well satisfied with Brodie's operation, the anterior and posterior skin-flaps; as for controlling hemorrhage, the hands were best; he had not had much experience with the rectal staff.

Dr. SPRENGEL, of Dresden, Saxony, remarked that about one-fifth of these cases healed primarily; one-fifth in about three weeks, and one-fifth in about two to three years, and then perhaps would die from the amputation and the prolonged drain on the system.

Dr. E. S. GARNER, St. Joseph, Mo.: I wish to be corrected if mistaken, when I say that Dr. Dennis made the statement that of all cases of osteo-sarcoma of the femur there is only one case on record which was alive one year from the date of operation. Some five years ago I assisted Dr. G. F. Shrady, of New York, to amputate a thigh for an enormous osteo-sarcoma of the lower third of the femur. The case had progressed further than ordinary, because of the patient's unwillingness to submit to the operation. The wound healed primarily, and at the end of two weeks the patient was about the ward on crutches.

Eighteen months subsequently there was no recurrence. The case was of particular interest, because of the fact that shortly afterward the same operator amputated the thigh of a patient, the subject of a femoral osteo-sarcoma, whose general condition was very much better than was that of the first patient, and where the diagnosis was made at a very early period, who died from visceral involvement within a few months. These two cases show, I think, the difference in the liability to return in growths of the same general and microscopical character. The first case is recorded among the histories of the Presbyterian Hospital, New York, and has, I think, never been placed on record.

Dr. ROBERT T. MORRIS, of New York, expressed it as his belief that malignant sarcomas belonged to the infectious microbic diseases, and that they were strictly local at the onset. He believed in radical operation at an early date, but thought that operation was useless after general infection had occurred; and that it was also useless in proper cases for operation, unless the operator were familiar with his subject in a scientific way. In support of the theory of microbic causation of malignant sarcomas, he drew comparisons between these tumors and the tumors which were known to be caused by microbe growth, as those of actinomycosis and of tuberculosis. He also showed that some tumors of plants, caused by the presence of more highly-organized parasites, were quite similar to some of the tumors of warm-blooded, vertebrate animals. The speaker said that in operating for the cure of sarcoma, we must have regard to the patient's life only, and must sacrifice his beauty and usefulness if need be, but on no account should an incomplete operation be done.

Patients ought to be impressed with the necessity for returning early for successive operation on recurring growth.

The following paper was read:—

DISTENTION OF THE CAPSULE IN DISEASE OF THE HIP JOINT.

DISTENSION DE LA CAPSULE DANS LA MALADIE DE L'ARTICULATION.

AUSDEHNUNG DER KAPSEL IN DER COXALGIE.

BY EDMUND OWEN, F. R. C. S.,

Surgeon to St. Mary's Hospital and The Children's Hospital, London.

It is, I apprehend, generally admitted that the sero-synovial effusion which is usually associated with the early stage of acute disease of the hip joint has an important influence on the stiffness and on the faulty position of the thigh. Further, there can be little doubt that the pain of acute disease is chiefly due to the distention of the capsule. (The pain of *chronic* disease may have little association with the intra-articular effusion, as the sensory nerves have by this time adapted themselves to the unnatural tension, the pain being chiefly due to ulceration within the joint.) When the capsule is distended, there are, in addition to the usual signs of the joint disease, a deep-seated fullness under the middle of Poupart's ligament, which has the effect of advancing the femoral vessels toward the surface, and a doughy swelling beneath the gluteus maximus, in the situation of the upper part of the neck of the femur. Such a swelling can be due only to intra-articular effusion, for it reaches from front to back of the joint, and it may present an obscure sense of fluctuation.

The object of this communication is to direct more general attention to this phase of hip-joint disease, to recommend a more systematic inquiry as to the existence of the effusion in each individual case, and to urge that the condition, when discovered, be

promptly dealt with on the lines which modern Surgery has supplied us for dealing with acute effusions in other joints, such as the elbow, ankle, and knee.

CASE.—A boy of nine years was recently admitted to St. Mary's Hospital, London, under my care, on account of extreme pain in the right hip and knee. He could not bear the limb to be touched. It was semi-flexed and slightly everted, and any attempt to bring it down flat caused a tilting of the pelvis and an arching of the loins. On gently abducting the limb the pelvis moved with it. There was some flattening of the buttock; a slight fullness could be made out deep in the region between the great trochanter and the ischial tuberosity. The furrow in the length of Poupart's ligament, moreover, was less marked on the affected side. The pain had come on suddenly, rather more than a fortnight before; during that interval he had been attending as an out-patient, and was in great distress; he had to be carried to the hospital wearing a posterior splint.

Chloroform was administered, but the limb remained stiff and springy. A canula and trocar were then thrust through the back of the capsule, and about a teaspoonful of sero-purulent fluid containing flaky lymph was withdrawn. Immediately the femur could be fully extended, and the joint was loosened. An extension apparatus was applied, and the boy was returned to bed. On the fourth day afterward we found him free of pain and apprehension, and his convalescence, which dated from the paracentesis, ran a favorable and perfect course.

The objection may well be raised that this was not a case of hip-joint disease in the usual sense of the term, but one of simple purulent synovitis. I will admit it. But if the capsule had not been punctured, would not the synovitis have probably entailed a suppurative arthritis? I think so. And if this theory be accepted, I may claim that potential hip-joint disease was cured by aspiration of the distended capsule.

Let me here offer the assurance that I am not claiming the revelation of some new method of treatment. Every day we puncture acutely distended joints—particularly knees—and with the greatest advantage. But, so far as my experience serves, we do not lay ourselves out for the relief of the distended hip joint as we do in the case of the knee. Possibly it is because "out of sight, out of mind." I do not say that tapping the hip joint is as often wanted as that of the knee joint, but what I would urge is, that we should always be on the lookout for this condition, and that even in cases where we cannot be absolutely certain of the existence of distention, we should give the patient the benefit of the doubt by making exploratory punctures of the joint through the gluteal region. With a well-washed buttock, clean fingers and instruments, and some common sense, the measure is, as practice has shown, devoid of harm and risk, while, on the other hand, it may prove itself of extreme clinical value.

DISCUSSION.

Dr. QUIMBY, Jersey City, said:—

MR. CHAIRMAN, I wish to offer thanks to Professor Owen for bringing before this Section—in his laconic style—this original suggestion of the importance of the early performance of paracentesis, in suspected effusion into the hip joint, the result of acute articular osteitis.

Surgeons, as a rule, are always ready, and apt, at opening abscesses in the joints in other parts of the body, while they seem to shrink from, or ignore, almost entirely, the significance of the early stages of morbus coxarum of the hip joint, or until, at least, the symptoms have assumed such a formidable degree of progress that, in cases especially of tubercular or strumous diatheses, the head of the bone is destroyed by inflammatory action.

This is due, no doubt, to various causes, prominently among which is the difficulty

in detecting the effusion, as the disease is deep seated, covered with a large mass of muscular and adipose tissue, making it difficult to detect a small amount of effusion, or the true nature of the disease in its early stages.

We should recognize the fact, that this disease is almost confined to the young, previous to the age of puberty, when inflammatory action is more destructive, often separating the epiphysis from the head of the femur. Therefore the treatment should be more active in the early stages, and I regard the suggestion of Professor Owen, of making an early explorative incision, very valuable, which may have the effect of creating a desired revolution in the early treatment of hip-joint diseases.

Dr. LANGE said: I am afraid that the conclusions drawn by the author about the curative value of his procedure *vis à vis* to cases of hip-joint disease, are too far going. Suppose the child, whose history we just heard, has really had that typical hip-joint disease which, in children, almost without exception, starts from the bone (and from the title of the paper it seems justified to assume that the author thinks it was one of these cases at an "early stage"), then we must have some doubts as to the definitely curative effect of this one puncture. That punctures of the hip joint in acute gatherings of fluid will be of palliative and symptomatic value in tuberculous cases also, nobody will doubt. I have satisfied myself of that repeatedly. I must say that I have always proceeded from in front. In the narrated case, the child has either had an effusion into the joint of non-tuberculous origin, and may be permanently cured, or it may have an osteitic focus still present in the neighborhood of the joint, for instance, in the os-ilei. In that case, its recovery is very likely only transient, and it is not excluded, that it still may pass through the later stages of a destructive joint process.

Our knowledge of pathological conditions in hip-joint disease in children entitles us to some skepticisms with reference to the value of one puncture as a definite remedy against that disease. Effusion into the hip joint cannot be called a complete diagnosis. From the communication we just heard we cannot conclude that a case of what is called hip-joint disease "in the early stage," has been cured.

The following paper was read:—

FISTULA IN ANO, OF THE HORSE-SHOE SHAPE.

FISTULES DANS L'ANUS DE LA FORME DU FER DE CHEVAL.

HUFEISENFÖRMIGE MASTDARMFISTERN.

BY SAMUEL BENTON, L.R.C.P. LONDON, M.R.C.S. AND L.M. ENGLAND.,
Surgeon to the Gordon Hospital for Diseases of the Rectum, London.

The objects of this paper are:—

First.—To point out that an important cause of failure of the operative treatment of anal fissure is the presence of a coexistent horse-shoe fistula, left unrecognized and untreated.

Second.—To define what is meant by a horse-shoe fistula, and to comment upon the different kinds of horse-shoe or semicircular fistulæ met with in practice.

Fissure is such a common complaint that doubtless cases have come under the notice of all present. This malady, if taken early, is very amenable to medicinal treatment, but, in many cases, especially of females, the disease is allowed to continue for some length of time before it is brought to the notice of the surgeon. Some operative treatment then becomes necessary.

Fissure of the anus is often treated by dilatation, a method originated by Recamier, but relapses often occur. This is put forward in favor of the neuralgic nature of the complaint.

I hope, in this paper, to show that these relapses are sometimes due to errors in diagnosis, and are the result of partial operative measures. In cases of fissure or anal ulcer which have lasted for many months, it will often be found when the patient is under an anæsthetic that a blind internal fistula coexists. This latter condition is a much more frequent complication than is usually supposed.

There is a form of fistula, horse-shoe or semilunar in shape, viz., the blind internal horse-shoe fistula, which, although far more common than the complete horse-shoe fistula, is not referred to in any books that I have consulted. Some writers mention the blind internal fistula which commences in an abscess, but the fistula I am speaking of does not commence with an abscess, it is merely a subcutaneous ulceration extending between and separating the mucous from the muscular coat of the bowel just inside the anus, and usually commences in a fissure that has been of some months' duration. A fissure may be quite superficial or it may extend through the muco-cutaneous surface and expose the adjacent muscular fibres.

A fissure that has been neglected or treated alternately by caustics and emollient ointments, soon drifts on to the formation of a painful rectal ulcer, with its base of an ashy-gray appearance, and the edges of the sore well marked. While forcibly dilating the sphincter, it is quite possible that the mucous membrane may sometimes be torn from the sphincter muscle, especially round the parts near an anal ulcer, and a sinus made traumatically; therefore, when the patient is under an anæsthetic, the examination (with a blunt-pointed probe) should be made at the base and sides of the fissure before the sphincter ani is interfered with. A blind internal fistula can then sometimes be demonstrated.

It has been observed and recorded that occasionally a probe may be passed under these ulcers leading into a fistulous tract which extends some distance upward. This is true, but the direction most frequently taken is circular or semicircular.

Horse-shoe fistulæ, as at present understood, are complete fistulæ, having an external and an internal opening, the external opening being situated on the opposite side of the bowel to the internal opening. This kind of fistula is rare. Out of an examination of 750 cases of rectal disease, I have only had five cases of complete horse-shoe fistula.

Some writers on diseases of the rectum refer to the complete horse-shoe fistula, others do not. I will now read to you the brief notes of a few typical cases illustrating this form of disease and upon which cases this paper is founded. In my after remarks I will refer to them by number. (See Tables, pages 568 and 569.)

From these cases we gather that fistula in ano of the horse-shoe shape may be complete or incomplete. Horse-shoe fistulæ may be complicated with prolapsus, as in Case 5, but the most usual complication is anal ulcer. Sometimes it is also found coexisting with polypus or stricture.

I find the average age of patients is thirty-five, and the disease attacks males and females indiscriminately.

Taking out Case 6, which was a very severe one, the average number of days in bed was fourteen. The wounds, of course, are not all healed up at the end of a fortnight, but by this time patients may usually safely be treated as out-patients. If a correct diagnosis is made, the treatment is comparatively simple, and consists in laying open the sinus and paring the edges with pile scissors, dividing or paralyzing the sphincter muscle and packing the wounds systematically with small pieces of cotton wool to prevent the edges sticking together, and getting the wound to heal up from the bottom.

Anal ulcers are usually situated dorsally. Should division of the sphincter muscle be the mode of operation fixed upon, it is a golden rule in rectal surgery not to divide the muscle directly in the middle dorsal line, for these incisions always take a long time to heal. Some surgeons divide the ulcer, but draw the incision downward through the body of the sphincter muscle; others leave the ulcer alone, but are contented to divide

the belly of the muscle, so setting the part at rest by paralyzing the action of the sphincter ; others, again, recommend that the sphincter ani, after dilatation, should be divided by the knife drawn across its surface, so as to divide about one-third of the fibres of the muscle.

If it is necessary to divide the muscle at all, it is right, in my opinion, to divide it thoroughly. In a few cases disappointments occur, patients, although relieved from pain for a time, do not get well, they occasionally feel pain and discomfort at the anus, and their fears are exaggerated by noticing an occasional drop of mucus or pus on their linen (Case 8).

They are then usually treated for and told they have neuralgia or gout of the rectum. Sometimes, at all events, neuralgia is a name used synonymously with idiopathic ; it would seem to imply that the doctor does not know quite what is the matter. These are the cases which sometimes, on very careful examination, will be found to be suffering from blind, internal fistula, semicircular in shape.

Surgeons should make certain that stricture, ulceration or malignant disease does not coexist with fistula. This is a precaution which should be remembered in all cases of rectal disease.

Whether it is a case of fistula, prolapsus or any other disease, I strongly advise that the interior of the bowel be examined in every case with the finger, also with a bougie, to prevent the possibility of overlooking a stricture. I have had an olive-headed bougie made for my use : it has a soft, pliable neck, and can be made to pass up easily and painlessly. The old-fashioned, straight, unyielding bougies are very apt to catch against the promontory of the sacrum.

It is the blind, internal horse-shoe fistula, semicircular and sometimes circular in shape, to which I have thus briefly called your attention, it being one of the causes of the occasional failures which follow operative treatment for anal ulcer. Among other causes may be mentioned the too sparing use of the knife and the overlooking of a polypus.

DISCUSSION.

Dr. QUIMBY said :—

I have been much edified by Dr. Benton's paper on diseases of the rectum, especially that part relating to rodent ulcer, some forms of which are closely allied to malignancy. These rodent ulcers are usually susceptible of cure when taken in their incipiency. In reference to ordinary fissures of the rectum, I think a better plan than that suggested by Dr. Benton, or one, at least, which I have followed a number of years, and has been very successful with me, is, by stretching or dilating the rectum to nearly its full extent once or twice a week, then painting over the fissured portion of the rectum with a solution of argentum nit., from twenty to thirty grains to an ounce aqua. This stretching of the rectum beneficially modifies the action of the sphincter ani, and facilitates the cure, which is generally effected in from six weeks to three months.

In reference to Dr. Benton's treatment of fistula in ano, I think a better procedure than the one he proposes is in laying open the fistula's tract, then thoroughly scraping it with a blunt, spoon-like scraper, so as to thoroughly destroy the pus-secreting pyogenic membrane. This being done, the wound is closed by interrupted antiseptic catgut sutures. Under this plan all or a great portion of the fistula's tract heals by first intention in from ten to fifteen days.

I think this method was first suggested by Professor Van Buren, of New York. I regard this as a great improvement upon the former method of opening the fistula's tract and packing it with lint, thus necessitating the wound healing by granulation and secondary intention.

DETAILED REPORT ON FISTULA IN ANO, OF THE HORSE-SHOE SHAPE.

Reference Number.	Case.	Initials.	Age.	Sex.	Occupation.	Disease.	Date of Operation.	Date of Discharge.	No. of Days in Hospital.	Highest Temperature.	Result.	Remarks.
627	1	C. W. C.	29	Male	Cabman	Posterior fissure, with semilunar, blind, internal fistula	April 20, 1886	May 1, 1886	11	F. 99.2°	Cured	Stated to have suffered from fissure for nine months. At the time of operation, a fistula was found leading away from centre of fissure on either side, semilunar in shape, extending half-way round the sphincter ani.
679	2	R. B.	42	Female	Caretaker	Fissure, with circular, blind, internal fistulæ	May 28, 1886	June 12, 1886	19	F. 99°	Cured	Defecation for six months previous to admission was accompanied by pain, with a discharge of blood and mucus. It was found, at the time of operation, that a fistula extended from the fissure completely round the anus. It was laid open, and dressed with pieces of cotton wool in the usual way. She was examined on July 7th, and the rectum and anus found to be perfectly healthy, and there was no stricture or contraction.
733	3	A. L.	22	Female	Shop assistant	Dorsal fissure, with circular fissure	June 15, 1886	July 3, 1886	18	F. 100°	Cured	This girl had been under the care of three different doctors. For five years she had suffered from constipation and dyspepsia. Defecation was accompanied by great pain and difficulty for four years before admission to the hospital. The sphincter ani was examined on July 7th, and found to be perfectly healthy, the bowels acting daily without medicine.
748	4	H. L.	46	Male	Framemaker	Dorsal fissure, with semilunar fistula	June 15, 1886	June 26, 1886	11	F. 99.4°	Cured	Eighteen months ago he was treated as an in-patient at St. Thomas', for congestion of lungs. For the last ten months he has suffered from painful defecation, with bleeding at stool, the pain being also very severe some time after an evacuation.
672	5	T. E.	34	Male	Fireman	Rectal ulcer, horse-shoe fistula and prolapsus	May 11, 1886	May 22, 1886	11	F. 100.4°	Cured	Last seen on June 4th; he was then perfectly well.
538	6	C. L. T.	37	Male	Tea taster	Severe and complete horse-shoe fistula	Jan. 5, 1886	March 21, 1886	75	F. 101°	Good	A man of intemperate habit; for four years suffered from discharge of blood and pus from anus. Fistulous tracts extended for several inches into his right buttock, and were very deep down. There was a fistula posteriorly, and an internal opening into the bowel on his left side between the sphincters. An operation was performed on Jan. 5th; to complete it, he was put under gas and ether again on Feb. 19th; examined on May 19th, there was one place posteriorly not quite healed, also a small place on right side. He was last seen in out-patients' room on June 28th, when the wounds were all but healed. His weight on January 3d was 11 stone, 6 pounds; April 10th, 13 stone; June 28th, 14 stone. (Same scales). Therefore, five months after the fistula was treated he had increased in weight nearly three stone.

Reference Number.	Case.	Initials.	Age	Sex.	Occupation.	Disease.	Date of Operation.	Date of Discharge.	No. of Days in Hospital.	Highest Temperature.	Result.	Remarks.
758	7	H. P.	32	Male		Anal ulcer, with blind, internal, semicircular fistula	July 6, 1886	July 18, 1886	12	F. 99°	Cured	This was a typical case of a semicircular fistula, the result of a neglected fissure. For months he had been treated by a medical man for piles, but, until he came to the hospital, he stated that no rectal examination had been made or suggested. This case is also interesting because any treatment that might have been devised for the cure of the fissure must have failed, unless the fistula was diagnosed and dealt with at the same time. On the eighth day after the operation, although he had had aperient medicine and several enemas, an accumulation of fæces was discovered in the rectum. After any rectal operation it is just as well not to attend implicitly to a nurse's statement that the bowels have acted. The surgeon should make a digital examination before discharging the patient, and satisfy himself that the rectum is clear.
639	8	M. B.	39	Female	Married	Posterior anal ulcer, with blind, internal, circular fistula	April 6, 1886	April 21, 1886	15	F. 99.2°		A history of several miscarriages, 2 children living, eldest 17, youngest 4 years old. She has suffered for 14 years with pain at stool, accompanied by occasional discharges of blood and pus. 12 years ago she underwent a cutting operation for the cure of fissure, the operation being performed by Mr. T., one of the best-known surgeons of the Midland counties; he also treated her at the same time for ulceration of the womb, which was cauterised. Dr. S. operated on her a few months after Mr. T., also by cutting; he stated it was an ulcer as well as a fissure. Five years ago, Dr. S. again cut it. Not getting well, she came to London and presented herself at the Gordon Hospital last April. She was found to be suffering from a posterior anal ulcer, complicated with a blind internal fistula. This fistula, at the time of operation, was found to run completely round the anus. The operation consisted in laying the fistula freely open, cutting the base and paring the edges. At the same time the sphincter muscle was divided at the side. Last seen and examined July 21st, three months after operation; wounds all healed, perfect control over sphincter muscles, rectum and anus healthy.
776	9	W. W.	35	Male	Beer-house keeper	Fistula siniste, blind, internal	July 6, 1886	July 19, 1886	13	F. 98.4°		He was operated on in South Africa, by Dr. C., for fissure. This operation gave him temporary relief from pain, but otherwise it was not a success. The fistula, in this case, was confined to the left side of the bowel, and, as usual, separated the mucous from muscular coat. July 22d, wound healed. Recovery complete.

Dr. DURANTE, of Rome, Italy, then read the following paper :—

CONTRIBUTION TO ENDOCRANIAL SURGERY.

CONTRIBUTION À LA CHIRURGIE ENDOCRANIALE.

BEITRAG ZUR INTRACRANIELLEN CHIRURGIE.

BY F. DURANTE,

Professor of Surgery.

In May, 1884, came under my care Chiara Batistelli, a woman 35 years of age and a native of Narni. Her general appearance was good; she seemed well nourished, although not of a very robust constitution. Externally she showed no abnormality except as to her left eye, which appeared somewhat low and drawn outwardly; otherwise the movement as well as the functions of the globe were normal. This deformity had manifested itself only within the three months previous to her visit to me; for a year or more, however, she had entirely lost her sense of smell, her memory had become impaired, particularly as to remembering names, and she experienced a peculiar sensation of vacuity around her body, which caused her to feel uncertain in her movements. Motion, sense of touch and sensibility to heat and pain remained natural.

From her husband I heard that she had somewhat changed in disposition, that from being generally happy and bright she had become sad, melancholic and taciturn, although she did not seem to brood over the state of her health.

The senses of hearing and taste, the functions of the chylopoëtic viscera were perfect, and nothing abnormal was found on a close examination of the nasal and pharyngeal regions.

The course of the disease, the loss of memory and of the sense of smell, and the objective and subjective state of the patient, led me to believe in the presence of a tumor within the cranium, the pressure of which affected the anterior lobe of the brain and paralyzed or destroyed the olfactory nerve. Moreover, the displacement of the globe of the eye led me to believe also that the tumor had penetrated the inferior arch of the orbital cavity. Such being my diagnosis, I now proposed to the patient an operation that would remove the offending object, explaining to her the gravity of the operation, without reserve. She was brave and she consented.

To reach the tumor it was necessary to make a large opening in the left frontal bone; so with a cut commencing from the inner angle of the left orbit, upward *curvilinearly* to the hair line as far as the temporal region, I raised all the soft tissue from the bone in a flap; the bone being exposed, with a sharp scalpel and hammer I removed a large portion of it, commencing at the superior orbital margin. Inferiorly I found that the internal parietes of the frontal sinus had been forced outwardly. The dura mater being now exposed, I examined it and found that it had been perforated by the tumor just opposite the frontal eminence. With great care I now began to scoop out the tumor. As soon as a considerable portion of the tumor was removed, I detected that it did not adhere beyond the internal surface of the dura mater, and that therefore its enucleation was comparatively easy. I then removed it and carried with it all the adherent portion of the dura mater. The hemorrhage was slight and easily controlled by the hemostatic, a tampon treated with sublimate.

The tumor was lobular, of the size of an apple, and weighed seventy grams. It occupied the anterior fossa at the base of the left cranium, extending to the right and upon the cribriform lamina, which it destroyed. Posteriorly it extended to the clinoid

tubercle before the sella turcica. The left anterior cerebral lobe was greatly atrophied, the orbital arch much depressed, but not perforated by the tumor, as I had anticipated.

Having stopped the bleeding completely I now united the wound by first intention, leaving in the cavity occupied by the tumor a drain tube, which descended to the left nasal fossa through the opening made on the ethmoid by a prolongation of the neoplasm ; then I closed the nasal cavity with an iodoform tampon.

The operation lasted about one hour. The patient bore the chloroform very well, showing only the weakness following the use of an anæsthetic and attendant upon loss of blood. On the third day she had fairly reacted, and the wound was healing without suppuration. The drainage worked well, a large quantity of serum, tinged with blood, flowing through it.

On the fourth day, however, the patient was overtaken by sudden prostration, was inclined to sleep, exceedingly disinclined to talk, and complained of mental confusion. I then discovered that the drain had stopped during the night, so I at once removed the tampon, replacing it, however, further down the nasal cavity. The effect was good ; the serous fluid began to drop again. Not satisfied with this, I applied a gum-elastic pump to the external opening, and drew about thirty grams of liquid. The flow was thus re-established and continued all the following day and night. On the renewal of the flow the alarming symptoms disappeared, as if by magic.

On the seventh day I removed the stitches and the drain tube, and on the fifteenth day the patient returned to her home, doing very well. She had lost that sensation of "vacuity around her person" which made her uncertain in her movements, but had not regained her memory nor the sense of smell.

Three months after, I presented my patient to the Chirurgical Society at its meeting in Perugia, in 1884. She was in a happy frame of mind, and willingly stated her experience ; she stated that now all her faculties and moral conditions were normal, and that she had even regained her sense of smell. This greatly surprised me, for I felt sure that I had destroyed the left olfactory in removing the tumor, which had absorbed the cribriform lamina of the ethmoid. Upon experimenting, however, with aromatic substances, we found that she could only smell from the right, and that the left was totally insensible, its olfactory having been destroyed either by the pressure from the tumor or by the operation itself.

The part of the bone which had been removed was now partially reproduced, the cavity in the region of the operation had disappeared, and the eye had regained almost entirely its normal position. The tumor under the microscope presented a multiform cellular structure of sarcoma.

It is now four years since that operation was performed, and my patient is in perfect health. My diagnosis and the operation, apparently so hazardous at the time, are therefore justified by the result, and though such operations have generally failed, the success of mine should secure proper consideration at the hand of modern surgery.

The progress of experimental pathology and of studies of cerebral localization every day now smooth our way to the diagnosis of cerebral diseases, so that the cranial cavity may in future justly enter into the dominion of surgery. The frontal and parietal regions can now be successfully attacked by the scalpel of the surgeon, and many affections of the meninges become trophies of rational surgery.

The following paper was read :—

ANUS ARTIFICIEL COMME TRAITEMENT PRÉLIMINAIRE DES MALADIES RECTO-COLIQUES.

ARTIFICIAL ANUS AS A PRELIMINARY TREATMENT OF THE RECTO-COLIC DISEASES.

KÜNSTLICHE AFTERBILDUNG ALS PRÄLIMINÄRE BEHANDLUNG DER RECTO-COLON-KRANKHEITEN.

PAR M. LE PROF. FRANÇOIS DURANTE.

L'avoir perdu quelques uns de mes opérés d'extirpation du rectum et l'avoir rencontré des cas de recto-colite ulcéré, obstiné à chaque pansement direct ou indirect, sérieusement me font penser à la manière possible de délivrer la surface d'opération et les points de sutures, dans le premier cas, est la surface ulcérée, dans le second cas, a l'influence nuissable des matières fécales.

Convaincu qu'il n'y avait là autre moyen vraiment utile que la déviation de cours des matières fécales. Pour quelque temps je me suis arrêté à l'idée de faire un anus artificiel ; opération que jusqu'à présent on ne peut pas considérer moins dangereuse que l'extirpation du rectum lui-même, parce que la plaie, comme dans cette opération, dans l'anus artificiel, est soumise aux mêmes conditions d'infection au contact des matières fécales. Pour cela donc, de Littré, qui le premier eut l'idée de l'anus artificiel, jusqu'à présent, cette opération a été considérée comme une sorte de cure palliative, réservée seulement aux cas dans lesquels la défécation était presque impossible.

Dans le but de rendre l'anus artificiel opération préliminaire non dangereuse, dans les maladies recto-coliques, j'ai pensé de l'exécuter sur l'intestin cœcus de la façon suivante : Après avoir découvert l'intestin, de la manière connue, parmi une incision de cinq à six centimètres, je prends le cul-de-sac intestinal, je le reconnais par les caractères anatomiques et je le fixe sur le péritoine pariétale avec des points de sutures au catgut. Fermée de cette manière la séreuse pariétale à la viscérale, je fais un second étage de sutures aux catguts avec lesquels je fixe les bords de la peau aux parois de l'intestin sans intéresser la muqueuse. Il résulte de cette manière d'opérer que la perte de substance succédée à la divarication dés bords de la plaie des parois abdominals et remplacée de la parois antérieure du cœcus.

Pour empêcher la probabilité d'étranglement de la part de l'aponévrose du grand oblique, je l'ai coupé en direction de la ligne médiane. Puis j'applique le pansement antiseptique et j'envoie au lit le malade ; au sixième ou septième jour on lève le bandage et avec le couteau de Paquelin on ouvre la parois de l'intestin déjà bien adhérante aux parois abdominales. L'ouverture de l'anus est de trois à quatre centimètres de laquelle les matières fécales coulent avec une facilité extraordinaire.

Du moment que la circulation des matières fécales dans le colon a été arrêtée on commence à traiter la réctocolite ulcérée, deux fois par jour, avec irrigations d'une solution de sulphoférate de zinc, ou closure de zinc, en proportion d'un par mille, à la température de 35° à 37° C., suivis d'une irrigation, même prolongée, d'eau bouillie à la même température.

S'il s'agit d'enlever le rectum, l'anus artificiel nous permettrait d'opérer d'une façon aseptique, et en conséquence de faire les plus grandes démolitions.

Jusqu'à présent j'ai opéré avec ces règles quatre malades, deux atteints de recto-colite ulcérée, d'une durée de six à huit ans, et deux de cancer du rectum très étendu. Dans une desquelles j'ai levé le rectum, la prostate et ouvert largement la cavité de

Douglass, dans l'autre j'ai exporté le rectum, la section inférieure de la prostate avec les vexicules séminaires et ouvert largement l'espace de Douglass.

Dans les deux cas j'ai, pour me faire espace, disarticulé le coccix et j'ai pu suturer aisément le péritoïne. Tous les quatre malades sont guéris avec un cours parfaitement aseptique, mais un des malades de cancer est mort après huit mois de récidive aux organes abdominaux et l'autre après deux ans a présenté un nodule dans la cicatrice, mais il est encore vivant.

Les opérés pour rectocolite ulcérée furent guéris après trois ou quatre mois.

Pour fermer l'anus artificiel je n'ai trouvé aucune difficulté, la dissection des bords de l'intestin fixé à la paroi abdominale sans ouvrir la cavité péritonéale et les différents étages de sutures de la surface sanglante m'ont donné un bon résultat, seulement en deux cas j'ai été obligé de répéter partiellement la suture, parce que quelque point avait failli à la première intention.

Dr. L. H. SAYRE read the following paper :—

THE TREATMENT OF ABSCESSES CONNECTED WITH DISEASED VERTEBRÆ BY INCISION AND THOROUGH DRAINAGE.

LE TRAITEMENT D'ABCÈS LIÉS AUX VERTÈBRES MALADES PAR L'INCISION ET PAR L'EGAUTTAGE ENTIER.

DIE BEHANDLUNG DER MIT ERKRANKTEN WIRBELN IN VERBINDUNG STEHENDEN ABSCESSE DURCH INCISION UND VOLLSTÄNDIGE DRAINAGE.

BY LEWIS HALL SAYRE, M.D.,

Of New York City.

Great as have been the changes wrought within the past few years in Surgery by the introduction of the Antiseptic Method, in no class of cases has it been more marked than in the treatment of abscesses connected with the disease of the spinal column.

The question of early operative interference in psoas and lumbar abscesses connected with disease of the vertebræ has lately attracted much and wide-spread attention, and the object of this paper is to add another case to the number now on record, demonstrating the good results to be obtained by opening and thoroughly and completely draining these abscesses.

Before antiseptic surgery was fully understood, these cases were looked upon by the majority of the profession as not suitable for operative interference, and the unfortunate patient was allowed to gradually become more and more exhausted from the constitutional effects of absorption of the purulent contents of these abscesses, while hoping that nature would effect their evacuation spontaneously.

When this greatly desired result had taken place the condition of the sufferer was little changed, as the discharge continued in the majority of cases for so long a time, and in such quantity as to drain the vital resources to such an extent that a fatal termination was only a question of time.

Commencing as a small collection of pus, the abscess slowly but steadily increases in size, burrowing through more and more tissue, until it finally reaches the surface a long distance from its point of origin, having involved many tissues not originally connected with the disease. When they were opened by a small or valvular incision, as was the rule of practice, the contents of the abscess were not fully evacuated, and severe constitutional disturbance was the usual result of the operation. In some cases

these abscesses have been arrested by repeated aspirations, but, as a rule, the contents are too thick to be withdrawn completely through the aspirating needle, and the abscess refills and must, finally, be otherwise dealt with.

Mr. Treves, in January, 1884, read some very valuable papers before the Royal Medical and Chirurgical and other British Societies, in which he recommended the free opening of these abscesses by an anterior and posterior incision, so that the entire abscess cavity could be cleaned, any detritus removed, and thorough drainage established, and the results of the cases he had so treated caused considerable discussion, and awakened the interest of the profession in this hitherto unscientifically treated class of cases.

In an inaugural thesis, presented to Bellevue Hospital Medical College, in 1876, I urged the early and free opening of the abscesses, having been convinced by personal experience that such was the proper plan of procedure.

In that thesis I said—

"I shall endeavor to prove the incorrectness of the following almost universally believed conclusions:—

"1. That these abscesses are almost invariably due to a scrofulous or strumous diathesis.

"2. That these abscesses should not be opened, or if they are opened at all, their contents should be evacuated by a valvular puncture.

"3. That if they are the result of caries or necrosis of the sacro-iliac junction, the removal of the diseased bone is necessarily fatal."

In that thesis I also stated that: "The treatment of these abscesses should be early and complete evacuation of their contents, by means of a free incision, opening the abscess at its most dependent point and leaving no pockets for the retention of the pus.

"By opening them, you relieve the patient from the effects of retained pus and prevent pyæmia; you correct any bad influences of the entrance of air by antiseptic dressings, and if the exciting cause be necrosis or caries, you will be enabled to remove it. The abscess will eventually open spontaneously, but the patient will be greatly reduced and his system broken down by the effects of long-retained pus. By opening them you prevent these complications and avoid the disastrous results which follow when they rupture internally. If these abscesses are aspirated or punctured by a valvular incision, the severity of the symptoms will be relieved, but they will soon refill and then the operation must be repeated."

The views set forth in this thesis have been corroborated by various surgeons, both here and abroad, and with our increased knowledge of antiseptic surgery the operation offers the quickest and safest termination for these cases, and shortens by many months the time which these abscesses will continue to discharge if left to open spontaneously, and also diminishes the risk of septic infection.

By an incision along the erector spinæ muscles, and through the quadratus lumborum muscle, the lower dorsal and first lumbar vertebræ are easily reached and the abscess opened at its point of origin, any sequestræ can be removed and the cavity thoroughly drained; a counter-opening being made at the lowest part of the sac and a drainage tube inserted, the abscess can gradually diminish in size and, finally, become completely obliterated. Should a swelling be felt in the inguinal region, it can be reached by carefully dividing the muscles, and, when the abscess is reached and opened, a probe can be passed through it to its posterior wall, where the point can be felt, and a counter-opening made and a larger drainage tube introduced from one opening to the other, directly through the abscess cavity. It is best to use Thiersch's solution (acid salicylic gr. 16, acid boracic gr. 96, to aquæ Oj) for irrigation, to avoid any danger of toxic effects from the irrigating fluid.

The operation should be performed with all antiseptic precautions; the drainage

tube should be large and so placed as not to permit any accumulation and retention of pus, and the dressings should be changed as seldom as possible. Any elevation of temperature after the second day should call for re-dressing and a careful search for accumulations of pus.

At each dressing the abscess cavity should be thoroughly washed out, and as it diminishes in size the drainage tube can be gradually changed to a smaller one and, finally, a few horse hairs substituted for it. These can be allowed to remain as long as necessary.

The following case is a good illustration of the advantage of this plan of treatment :—

Mary Isabelle K., aged six and a half years, Vancouver, B. C. Only child of healthy parents. Until two years ago enjoyed good health. At this time was struck by a wardrobe door in the lumbar region, and a couple of months later began to walk stiffly, and a small swelling appeared on the right side of spine in lower dorsal region, and in May, 1886, a slight projection of lumbar vertebræ was noticed. She at this time began to complain of pain, and cried out in her sleep at night. A plaster-of-Paris jacket was applied by Dr. Davy, when the pain ceased and she seemed to be doing well. On removing the second jacket, in February, 1887, a swelling was noticed on the anterior and upper part of right thigh. This disappeared after a time, and reappeared on the abdomen low down on the right side. Walking became more and more difficult, and she was finally carried nearly all the time. First seen April 16th, 1887, when she presented slight constitutional symptoms of retained pus. On removing the jacket a small kyphosis of two lower dorsal and one lumbar vertebræ was present. A swelling the size of a large lemon was situated above Poupart's ligament on the right side. A plaster corset was applied and, on April 26th, the abscess was aspirated, removing eight ounces of thin pus, when the needle became clogged. On May 21st, 1887, the abscess was opened by an incision along the upper border of Poupart's ligament, giving exit to a large quantity of pus, which was above the abdominal muscles; the pus had come through the abdominal ring and had formed a sac extending upward as far as the umbilicus. An incision was made through the abdominal muscles, along Poupart's ligament, which gave access to the abscess proper, which was full of crumbs of bone and shreds of connective tissue. The finger could be passed so as to touch the vertebræ, two of which had been considerably eroded. A counter-opening was made opposite the last dorsal vertebra, and a drainage tube inserted directly through. After washing out the cavity very thoroughly with hot Thiersch's solution, the lower incision was partially closed by catgut suture and the wounds dressed antiseptically. The temperature at the time of operation was 103°, and, as it did not fall after twenty-four hours, the dressings were removed, when a collection of pus was discovered in the sac, external to the abdominal muscles. This was thoroughly washed out and another tube inserted to drain this cavity. After this the temperature fell. The dressings were removed about every fourth day and, on July 26th, horse hair was substituted for the drainage tube. This was found in the dressing, outside the body, on August 10th, 1887, and at the present date the wounds are entirely closed, and the child in perfect health and able to take a large amount of exercise. She still wears a plaster corset as a protection.

DISCUSSION.

Dr. Grant-Bey, of Egypt :—

I agree with Dr. Sayre that psoas abscesses are not invariably connected with a scrofulous habit of body.

Some seventeen years ago a Scotch gentleman came under my care in Cairo. He had been a great sufferer for four and a half years before I saw him, and had

consulted the first surgeons in England, Scotland, France and Italy, but, although some of them considered that the gentleman was suffering from a psoas abscess, no one dared to operate on him.

The history of the case is very simple. The gentleman was about thirty years of age and of a sound constitution, and well off in the world. When hunting one day, in the vicinity of Rome, he jerked round suddenly in his saddle and hurt his back. The pain was severe and he became thoroughly disabled. He was leeched and poulticed, and antiphlogisticated generally, and after some months he so far recovered as to be able to move about with the help of a staff, and in this state he was able to go to England and Scotland, to consult surgeons there. When he came under my care in Cairo, in 1870, he was bent like an old man, and walked lame with the help of a staff. I noticed somewhat of a fullness and even prominence a little above Poupart's ligament, with distinct fluctuation, and with the assistance of another surgeon, I operated by making an incision about two inches in length, parallel with Poupart's ligament and over this prominence and right into the cavity of the abscess, from which flowed a great quantity of pus. The incision was made after freezing the part with Richardson's spray producer, and, if I remember rightly, the wound was dressed with carbolic solution. No injections of any kind were ever made into the sac of the abscess. Pus continued to flow more or less for six weeks, then ceased and wound healed, and patient after a little became perfectly well and has been very well ever since, although he has been afraid several times of a return of the malady from having overstrained his back in driving his four-in-hand.

Mr. Edmund Owen, F. R. C. S., London, expressed his strong approval of the treatment just advocated, but it was his practice to make the anterior opening before the posterior. There was no danger of wounding the peritoneum, for the pus had pushed the serous pouch far away from the region of the anterior iliac crest. The first incision was much like that which is employed in tying the external iliac artery, but it is of less extent. The posterior opening was of great importance—an abscess cannot drain up hill—and with the abscess cavity laid open front and back thorough washing and drainage could be effected. The fluid best suited for irrigation was warm iodine lotion decolorized by a little carbolic acid. Carbolic lotion by itself, and mercuric solution, could not be employed in the usual strengh without considerable risk of carbolic or mercuric poisoning. He called attention to the fact, that sometimes after operating on the psoas abscess a second abscess quickly formed. This was shown by a rise in temperature and by, perhaps, a generally unsatisfactory condition of the patient. Such an abscess must be promptly dealt with, like the original one. In conclusion, Mr. Owen urged a thorough opening of every psoas abscess, front and back, thorough evacuation and perfect drainage. Psoas abscesses do not get well of themselves.

Dr. R. Lange, of New York: Psoas abscess and its treatment cannot be considered without reference to the ostitic, mostly tuberculous process, which has caused the formation of the abscess. It seems to me, that upon the condition of the primary tuberculous bone focus, will, as a rule, depend the success that follows surgical interference. Before all I should like to contradict Dr. Owen's statement, that psoas abscess will never undergo spontaneous absorption. About six or seven years ago, I had presented before the Surgical Society of New York three patients, who, if I am not mistaken, had outspoken Pott's disease, and in whom this occurrence had taken place. These cases are, of course, exceptional, but they happened, and I have since observed a couple of cases more. I also should like to remind you,

gentlemen, of the clinical fact, of which many of you have seen the proofs, that Pott's disease with all symptoms, pronounced angular curvature of the spine, even paraplegia, may exist and neither a microscopic nor palpable gathering of pus be present. Shall we assume that in such cases never any pus has been formed? I am rather inclined to think that in such cases the destruction of the bone in fact has been accompanied by the formation of fluid inflammatory products, but that the latter have been small, have been encysted, undergone fatty degeneration and absorption. We also know that thick cheesy deposits in such cases might eventually remain. It is to me, according to my personal experience, an undoubted fact that under proper constitutional and mechanical treatment, for which latter we shall remain principally indebted to Dr. L. A. Sayre, palpable psoas abscess, even of good size, can eventually undergo spontaneous absorption.

The surgical treatment of these abscesses will and can be followed by speedy and complete recovery in those cases only in which the tuberculous process of the vertebra is not any more in progressing activity. I myself have had a certain number of such cases, but I have never flattered myself to have cured the underlying ostitis in these cases. Most likely the latter were already healed and the abscess nothing but a comparatively harmless remnant. In other cases fistula will remain, and the patient might still be benefited. These might be cases, in which the carious process *per se* is not progressive any more, but certain products of it, say, for instance, a small piece of dead bone, keep up a slight discharge. Why should it not be possible that small pieces of dead bone, through the action of finally more healthy granulations, may gradually be absorbed? Let me remind you of the evasions of ivory pegs in the treatment of pseudosthrosis. The idea to attack all psoas abscesses as soon as they are detected, by thorough operative interference, would certainly be very rational, if only we were able to master the whole field of disease. I myself have attempted this procedure, but I am satisfied that in only quite exceptional cases such favorable condition of things will exist, that we can get at the ostitic focus in such a way as to be enabled to successfully and thoroughly attack it. As a rule, we must leave, in progressive cases of caries, a complicated wound, which is likely to keep up a large secretion and impair vitality more than does that amount of pus which is delivered into the abscess while in closed condition. And let me remind you that the existence of a cold abscess, as such, only exceptionally is the source of danger and suffering. Have you not seen cases in which, quite accidentally, the parents detected those swellings, and that the existence of the "lump" rather than the suffering connected with it, caused them to apply for your help?

The logical conclusion, derived from clinical observation, must be this: that schematical rules cannot be laid down for the treatment of a disease which varies so much in intensity and course, and of which we will often not be able to judge the degree of the primary morbid affection. In one case surgical interference might be justified, in others it might be the induced cause of a disastrous termination. I myself am rather in favor of a temporizing treatment or of that by puncture and injections; those cases excepted in which the presence of the abscess *per se* is the source of danger and suffering. In still existing tuberculous caries, the simultaneous mechanical and general treatment is of pre-eminent importance. In adults, I delay operative interference as long as possible.

Dr. SPRENGEL, Dresden, Germany, said:—

Es sind in der Behandluug der Psoas-Abscesse zwei Fragen in erster Linie zu beantworten:

1. Wann soll man die Abscesse eröffnen?

2. Wie soll man sie eröffnen?

Ad 1. Die Vorredner haben betont, man solle operiren, sobald der Abscess nachzuweisen ist. Ich bin *nicht* dieser Ansicht. Dieselbe wäre nur richtig, wenn wir den Herd selbst erreichen könnten, wie wir es bei der Mehrzahl der sonstigen skrophulösen Affektionen zu thun im Stande sind. Da wir aber den Herd an der Wirbelsäule nicht mit der nöthigen Sicherheit operativ angreifen können, so ist es gut, so lange wie möglich zu warten, um dem tuberkulösen Prozess Zeit zur Heilung zu geben. Die frühzeitige Eröffnung ist überdies deswegen nicht empfehlenswerth, weil die Abscesse in einer Reihe von Fällen zweiffellos spontan resorbirt werden und man durch die frühzeitige Incision dem Kranken eine Chance der (spontanen) Heilung nehmen würde.

Die richtige Zeit zur Eröffnung wird gegeben sein, sobald man die Progredienz des Abscesses sicher nachweisen kann und derselbe spontan zu perforiren droht. Doch soll man zweckmässiger Weise nicht warten, bis die Fascie durchbrochen ist.

Ad 2. Die Methode der Eröffnung war bis vor Kurzem auch in Deutschland die *Incision*. Neuerdings vollzieht sich indessen ein Umschwung. Derselbe ist durch die ungünstigen *definitiven* Resultate der Incisionsmethode bedingt. An den *unmittelbaren* Erfolgen der Operation stirbt zwar heutzutage wohl *Niemand* mehr. Aber die definitive Ausheilung, d. h. der *vollkommene* Verschluss der Wunde, erfolgt nur in ca. zwei Fünftel der Fälle, entweder sehr schnell oder in relativ kurzer Zeit. In drei Fünftel der Fälle bleibt eine Fistel zurück; es besteht anhaltende Eiterung fort, an deren Folgen die Kranken nach längerer Zeit (ein bis drei Jahren) zu Grunde gehen. Diesen *definitiven* Resultaten gegenüber ist es berechtigt, nach einer besseren Behandlung zu suchen. Eine solche scheint nach den bisher bekannt gewordenen Resultaten die Punktion und antiseptische Auswaschung mit nachfolgender Iodoform-Injektion zu sein. In Deutschland haben sich sehr gewichtige Stimmen für diese Behandlungsmethode erhoben und ich möchte mich nach meinen bisherigen Erfahrungen denselben anschliessen.

Remarks by L. H. SAYRE:—

I am in perfect accord with the remarks made by Mr. Edmund Owen, and agree with him as to the benefit of an anterior and posterior opening over a simple lumbar incision, as affording much more perfect drainage. In regard to the remarks of Drs. Lange and Sprengel as to the inadvisability of opening these abscesses on account of their tubercular nature, and sometimes being unable to remove all the foci of disease—it is absolutely necessary to give the patient all the benefit to be derived from the best mechanical, therapeutical and hygienic treatment possible, in addition to the operating measures. It is only too true that some of the cases operated upon do not heal by primary intention, and some not at all, but continue to discharge. Is it not better to have these abscesses freely drained through two openings, and thus prevent the danger, which is always present in these cases when left to nature, of having sinuses form one after another, due to the imperfect evacuation of the abscess, until the patient has sometimes many wounds, discharging pus and causing much annoyance? In my opinion most emphatically yes.

Dr. SPRENGEL (Dresden) nimmt nochmals Gelegenheit, seine in letzter Sitzung entwickelten Anschauungen über die Behandlung der Psoas-Abscesse kurz zu wiederholen, um das Missverständniss aufzuklären (das nach der Entgegnung des Herrn Sayre jun. zu bestehen scheint), als ob man in Deutschland die breite Eröffnung der Psoas-Abscesse für gefährlich halte, oder eine grössere Anzahl von

Kranken an den unmittelbaren Folgen der Operation verlöre. Das sei keineswegs der Fall. An den Folgen der Operation sterbe Niemand, dagegen gehe ein sehr beträchtlicher Prozentsatz der Operirten nach längerer Zeit (ein bis drei Jahren) an erschöpfender Eiterung aus zurückbleibenden Fisteln zu Grunde. Lediglich wegen des letzteren Umstandes sei es wünschenswerth, ein Operationsverfahren zu führen, welches jene Uebelstände nicht mit sich bringe.

The following paper was read :—

RODENT ULCER OF RECTUM.

ULCÈRE RONGEUR DU RECTUM.

ULCUS RODENS DES RECTUMS.

BY A. TREHERNE NORTON, F.R.C.S.,

Surgeon to St. Mary's Hospital, London.

There is an ulcer of the rectum well known to surgeons, persistent and progressive, unyielding to treatment of any and every sort, and advancing, after many years, to more or less complete stricture of the rectum. It has been said by many to be syphilitic, though no history of syphilis could be obtained, and it appears to be much more common in women than in men, and occurs generally after thirty years of age. The ulcer never seems to commence outside the anus nor at a distance from the anus, but immediately within. If, at the onset, the anus be turned out, we find inflammatory thickening of the mucous membrane, perhaps sharply excavated at the seat of ulceration but otherwise networked, more or less resembling the stomach of an ox. After a time the ulcer gradually extends and completely surrounds the rectum. It is accompanied by pain and bleeding during defecation, but the pain is not severe, and the patient may have suffered from it many years before seeking advice. As it extends up the rectum, contraction of the space takes place, and the signs and symptoms of stricture follow, chiefly constipation or difficulty in passing a motion, with flattening or shaping of the stool. In some cases, small abscesses are formed, resulting in fistulæ. The treatment by gradual dilatation is very likely employed with apparently considerable relief for a time, and thus the case goes on for a period of many years. I have known some of over fifteen years' standing.

The chief points in diagnosing this form of disease from carcinoma, are, of course, the length of time the disease has existed and the absence of any considerable quantity of growth, although at the precise seat of the stricture the quantity of new tissue may appear not inconsiderable. In some of these cases I have cut away portions from the thickest part and sent them for examination to pathologists of eminence, with the result that they were said to be sections of carcinoma. The sections contain much inflammatory tissue and scattered nests of cells of the type cylindrical epithelioma. Now, were such an ulcer, of some years' standing, to occur on the skin in a person thirty years of age and upward, it would doubtless be pronounced a rodent ulcer, but it has apparently never occurred to surgeons to look upon the same condition in the rectum as a rodent ulcer, and without making any microscopical examination, they have been pronounced to be syphilitic. Some twenty cases have come under my notice, and I have treated them as "stricture of the rectum." In several of them I ultimately performed colotomy, but they have never died under my care, and it is only recently that it occurred to me to cut away a piece for microscopical examination. I have invariably entertained the question of syphilis but in not a single instance have I been enabled to

detect any history of such, and, on the contrary, I have noticed that in cases of syphilitic ulceration affecting the rectum, there has been this distinguishing feature, that the disease has attacked the tissues outside as well as within the rectum.

It may be as well to say a few words about the pathology of rodent ulcer. There is no very precise definition of rodent ulcer. It has been said that it is the least malignant form of carcinoma, but such is rather a statement of one of its characteristics than a definition. I have many specimens of rodent ulcer in my collection of microscopical preparations, and after careful observation of these specimens compared with those of epithelioma, I should say that in rodent ulcer the columns are not so aggregated together and do not bud to such an extent as in epithelioma—that they are more widely separated by inflammatory tissue, and I am inclined to think that, owing to the formation of fibrous tissue in parts around isolated columns of epithelioma, those columns are constricted and even destroyed and cast off as small sloughs, followed by contractions of the ulcer at parts, and by apparent cicatrization of even real healing, hence, the slow progress of the disease and its very prolonged continuance without grave constitutional disturbance.

The following paper was read :—

WHEN IS COLOTOMY JUSTIFIABLE?

LA COLOTOMIE QUAND EST-ELLE JUSTIFIABLE?

WANN IST DIE COLOTOMIE GERECHTFERTIGT?

BY JOSEPH M. MATHEWS, M.D.,

Professor of Principles and Practice of Surgery, and Clinical Lecturer on Diseases of the Rectum, Kentucky School of Medicine; Visiting Surgeon, Louisville City Hospital, and Sts. Mary and Elizabeth Hospital, etc.

In considering this question the conclusions are formed after an experience of a score of years in rectal surgery, and my remarks refer especially to the diseases affecting that portion of the gut requiring surgical interference. Whereas I shall object, in the main, to views expressed by the majority of authors, and by many who are doing this special work, I wish to say that in so far as my objections refer to the dangers of the operation, I consider that they are much decreased under the present strict antiseptic precautions as practiced in opening the abdomen. However, this admission plays no part in rebutting other objections to the operation, that I shall try to produce. You will, therefore, permit me to give my conclusions first, and remarks afterwards :

1. I do not believe that Colotomy is ever justified in cases of *cancer* of the rectum.
2. In strictures, or obstructions of the rectum from whatever cause, located within 3½ inches of the external sphincter muscle, Colotomy should not be done.
3. The operation is not warranted in cases of ulceration of the rectum, unless of *specific* origin and accompanied by strictures beyond the reach of the finger.
4. Colotomy should not be performed because of the presence of tumors, or an aneurism, causing pressure upon the gut.
5. I do not believe that in cases of congenital occlusion of the rectum, the operation is justifiable.
6. In cases where the operation is looked upon as a *dernier-ressort*. I do not think colotomy should be done, save from total obstruction of benign or specific origin, and located at the distance above the sphincter as mentioned.
7. Where the rectum or sigmoid flexure is obstructed by strictures of specific or benign origin, I believe the operation to be advisable.

1. It is after a careful survey of all the reasons advanced by those authors who advocate colotomy in cancer of the rectum, that I am constrained to differ from them, and say that I do not believe the operation is ever justified in such cases. The reasons I shall state briefly. (*a*) The operation does not prolong life; (*b*) admitting that life could be prolonged, I could not admit its advisability; (*c*) contrary to prolonging life, it is the rule that surgical interference in these cases shortens life; (*d*) pain is not relieved, or indeed lessened, by the operation.

In support of the assertion that life is not prolonged by the operation I have only to mention the characteristics ascribed to malignancy, viz.: Infiltration of tissues, disposition to ulcerate, and the existence of a cachexia or dyscrasia. Surely if the cachexia be present, opening the colon will not stop it; if the processes of infiltration and ulceration are progressing, or even manifest, colotomy will in no wise affect the result. Can any one say that, because forsooth his patient lived so many weeks or months after the operation, that he would not have lived as long without it, unless total obstruction was the rule? I have seen quite as many live as long without as with this procedure. If there be any chance of prolonging life it lies in the radical extirpation of the mass, and that before constitutional infection is made manifest.

I have stated that pain, if it be a factor in the disease, is not relieved by colotomy. Pain in cancer is inherent, dependent upon nerve pressure by the offending mass, and is not due to extraneous causes. Hence of what avail is opening the gut at a distant part? If, as some would have us believe, the pain is caused by the openings made into the adjacent parts as, with the urethra, bladder, vagina, etc., I would ask how the operation of colotomy is to prevent such an occurrence? It is not the passage of the fecal mass over the affected parts that tends to this result, but rather, it is the nature of the disease to infiltrate and ulcerate. In my observation of these patients pain has not been the prominent symptom that it is said to be, especially if the growth is situated above the external sphincter muscle, and does not embrace it. The majority of cases are found above this muscle and have the natural disposition to extend upward, and severe pain is often absent, especially in the early stages of the disease. Indeed, it is not uncommon that patients come to my office to be treated for some supposed trivial rectal trouble and an examination would reveal the rectum or sigmoid flexure nearly blocked by a cancerous mass. Hence, in the main, gentlemen, these are my reasons for believing that colotomy is not justifiable in cases of cancer of the rectum, although I recognize that many have and will differ from me. Among this number is my late friend Dr. Erskine Mason, of New York, who wrote elaborately in praise of the operation on this class of patients.

2. In strictures, or obstructions of the rectum, from whatever cause, located within 3½ inches of the external sphincter muscle, colotomy should not be done.

In supporting this statement I desire to say that, in my opinion, a stricture located within this distance can be divided without risk, and the end obtained is equally satisfactory to that of so formidable an operation as colotomy. Whereas, I am not an advocate of forcible or gradual dilatation of stricture of the rectum, I would much prefer either method to colotomy. I cannot agree with those who think division of a stricture of the rectum a dangerous operation. Those accustomed to handling these parts do not dread hemorrhage, because it is easily controlled. To be more explicit, my reasons for division in preference to colotomy I would argue: If the stricture be of malignant origin the result would be that a free passage for the fæces would be had, and this is as much as we could expect from colotomy, and the patient is saved from a disgusting and dangerous operation. If the stricture, on the other hand, be of benign origin, there is some prospect of its entire eradication. It would not be by colotomy.

3. The operation is not justified in cases of ulceration of the rectum, unless of specific origin and accompanied by strictures beyond the reach of the finger.

I take it for granted that those who have spoken of colotomy, in ulceration of the rectum, mean that condition which exists when malignant or specific deposit is present, and not ulceration the result of ordinary causes. If this be true, my remarks in opposing the operation in cancer of the rectum will apply here. If it is intended to refer to syphilitic ulceration, this phase of the subject will be discussed further on. If the ulceration existing require even a consideration of so grave an operation as colotomy, it would not be the condition of ulceration that called for it, but the disease of which it is but a part. In simple ulceration of the gut I cannot imagine any one advocating the operation. In such cases the bowel can be cleaned and rendered aseptic, and all material prevented from passing over or through it, by giving perfect rest, in the use of the milk diet, as is used by Mr. Allingham, or, what is much better, the free administration of bovinine, which is a mixture of the vital principles of beef and mutton, and contains $34\frac{70}{100}\%$ of soluble albuminoids. During the taking of this preparation the patient is well nourished without any additional food, and any local treatment can be given the case without fear that the result will be disturbed.

4. Colotomy should not be performed because of the presence of tumors, or an aneurism causing pressure upon the gut.

Although we frequently read that tumors, by their pressure upon some portion of the gut, may call for the operation of colotomy, I have never seen such a case. Indeed, it is hard to imagine the reasons for such procedure, from the fact that if tumors exist, it certainly would be good surgery to remove them, rather than to resort to this operation, which would be under any circumstances but a partial relief. Neither can I believe that the presence of an aneurism would justify the operation.

5. In congenital occlusion of the rectum I do not think the operation is to be recommended.

I use the word recommended here, instead of justified, for the reason that I know many will object to the proposition. It is difficult to lay down specific rules by which we are to be controlled under circumstances like these. It was once thought the proper thing to destroy the life of the child to save the mother under certain conditions that obtain during midwifery. More modern practice would have us risk the life of the mother to save the child. In considering the question, whether or not to perform colotomy upon the newborn child, we must take into consideration not only the dangers incident to the operation upon this young subject, but to the result even if successful, which, even if it prolongs life, is most disgusting. The statistics are very much against the safety of the operation. That the perineal operation should be done I do believe, but that colotomy should be practiced I do not believe.

6. In cases where the operation is looked upon as a *dernier ressort*, I do not think it should be done, except for total obstruction located above the reach of the finger, and not of malignant origin.

In reading the literature of the subject I find that some authors advise colotomy in cases of total obstruction of the gut from malignant growths. I have said enough in the first pages of this article to evidence my objections to this practice, but in a word wish to reiterate my opinion. In cases of total obstruction within the prescribed limit for division colotomy should not be done, be it of malignant or non-malignant origin, if beyond this point. If, however, it lies beyond this point, and is not malignant, the operation should be considered. In no case, however, where the patient is dying from constitutional disease, would I resort to this operation to prolong his life a little while.

7. Where the rectum, above $3\frac{1}{2}$ inches, or the sigmoid flexure, is obstructed by syphilitic deposit, I believe the operation of colotomy should be resorted to—of course by the consent of the patient.

I beg leave here to take exception to a doctrine in regard to syphilitic diseases of the rectum as taught by many authors, viz.: That syphilitic ulceration of this part of the

gut is caused by the extension of chancrous pus into or on the part. This was the favorite belief of Dr. Erskine Mason, of New York, who wrote extensively on the subject, and Dr. Van Buren in his first edition of his work on diseases of the rectum was inclined to this view. So late an authority as Ziegler, in his text-book of Pathological Anatomy and Pathogenesis, issued this year, in treating of this subject, syphilitic ulceration of the rectum, says: "As it is met with almost exclusively in women, it is probably due to infection conveyed to the rectum by the secretions escaping from the vagina." To these views I must dissent. After a careful study of several hundred cases of syphilitic ulceration of the rectum seen in hospital, dispensary and private practice, I am convinced that syphilis invades the rectum by the deposit of gummatous material in the second stages of the disease, and that ulceration supervenes upon this, as it does elsewhere in the body. My experience has taught me that men just as often as women are affected in this manner, and not, as Ziegler says, it is met with almost exclusively in women.

In 1877 I had the honor to report to the Kentucky State Medical Society a case of syphilitic stricture of the rectum in a male patient, the constriction being near the sigmoid flexure. The whole history of the case proved it to be of syphilitic origin, and the rectum for eight inches was involved, but beyond the seat of stricture was implicated. The stricture was forcibly dissolved by the introduction of the hand into the rectum, and the case subsequently treated by anti-syphilitic medication. Decided improvement took place, the patient living for eight years and eventually dying of syphilitic brain and cord complications.

I might go on and cite many more cases in proof of the theory, but one is quite sufficient. Certainly chancrous pus cannot jump from the end of the penis to the distance of eight inches up the gut. We all have seen, in the case of prostitutes, this chancrous pus flowing over the mucous membrane of the bowel and into the rectum, but I have never observed a case of syphilitic ulceration that was caused by this flow. I have seen fit to enter thus far into the discussion of these views for the reason that my paper states that I believe colotomy is justified in cases of syphilitic ulceration of the high rectum or sigmoid flexure. This condition, if it exists at all, can only be caused in the manner that I have described, and in my opinion is as serious in its consequences as cancer, the difference being that by the operation of colotomy in cancer we cannot hope to prolong life to any definite certainty, whereas in stricture of the gut from syphilis, if there be even total obstruction, life can be not only saved, but prolonged to that extent that the patient would have lived had no obstruction existed. In other words, live until the disease of itself would kill without reference to the bowel trouble.

Remarks.—It may be asked, what is to be done in the great number of cases requiring surgical interference in order to establish a passage for the fæces, as in strictures, obstructions, etc., that are often met in the rectum? I would answer, that in every such case, of whatever origin, linear rectotomy should be done. It is free of danger, and accomplishes quite as much as colotomy, and is free from the disgusting result of the latter operation.

In many cases, where the authorities would say that colotomy should be performed, I have done rectotomy, and have witnessed the most satisfactory results.

DISCUSSION.

Mr. Samuel C. Benton, M. R. C. S., of London, entered into the discussion, which followed the reading of this paper. Mr. Benton said:—

I have listened with great interest to Dr. Mathews' paper, as I am connected with the Gordon Hospital for diseases of the rectum, in London. With reference to the remarks which Dr. Mathews has made in respect to syphilitic diseases of the

rectum, I cordially agree, but I understand Dr. Mathews to say that colotomy is not recommended for cancer of the rectum, even in cases of complete obstruction.

Firstly, because the operation does not prolong life.

Secondly, the pain is not relieved.

My experience is decidedly opposed to the above conclusions, if there is a reasonable chance of getting away all the disease. The treatment adopted in London is excision, but should the disease be so high up that it cannot be totally removed by excision, by colotomy the patient's life is prolonged, and the pain is immensely relieved. Patients suffering from cancer of the rectum with partial or complete obstruction, suffer intensely from a straining and bearing-down pain, and usually are only able to pass a little blood and mucus, with the smallest amount of liquid fæces. The pain in these cases is very severe, and almost continuous. I will only quote one of a series of cases of colotomy, which I have performed. The patient came under my care with cancer of the rectum, causing complete obstruction, and if something had not been done to relieve him, he certainly would have died within a week. I performed lumbar colotomy on the left side; the result was he lived eighteen months after the operation, and during part of that time returned to his usual avocation, his life was prolonged and the pain and suffering relieved.

Dr. Mathews includes in his paper rectal stricture of benign origin, and recommends division or rectotomy as the appropriate treatment. After division of a stricture by any cutting operation, however thoroughly it is performed, how soon does the stricture return? I had hoped to have heard Dr. Mathews' views on the treatment of stricture of the rectum by electrolysis, as I believe we are largely indebted to American surgeons for perfecting this mode of treatment. I shall be very glad, if, in replying to the remarks on his paper, Dr. Mathews will give us his experience on dissolving rectal strictures by electrolysis. I hold that benign stricture of the rectum is much more readily dealt with by electrolysis than by any cutting operation, for the following reasons: No anæsthetics are required, the patient is not compelled to keep his bed, and is able to continue his usual work during treatment; also, if a bougie is passed occasionally, there is not so great a tendency to a return of the disease; further, the operation is a painless one, and with ordinary care devoid of all danger.

Dr. J. W. Hamilton, of Columbus, dissented from the views of the author of the paper, as to the operation of colotomy in cancer of the rectum. Cases occasionally occur where it becomes an imperative duty, as in the following:—

A physician, about sixty-five year old, once reached a condition, after two years of suffering, in which the rectum was occluded as far as the ordinary means of exploration could reach. Mucus, blood, a little flatus, and only the slightest traces of fluid fæces passed in connection with and as the effect of violent and almost constant tenesmus. This continued gradually to increase for months, and finally reached a most pitiful condition, for which attempts at dilatation, whether by the fingers or soft bougies, utterly failed to give him relief. In the meantime the abdomen was distended with flatus and fæces almost to bursting. After long persuasion the patient was induced to submit to left lumbar colotomy. It gave immediate relief. Of course, it did not cure the patient's cancer. It, however, relieved him from horrible torture, and added about twenty months to his lifetime.

In such cases, he insisted not merely that the operation is admissible, but that its performance is an imperative duty.

Dr. Hingston agreed with the reader, that lumbar colotomy is not always a

satisfactory operation, but thought it advisable in cancer of the rectum, where stenosis interfered with defecation, a condition happily very rare, and in the cases of malignant disease of the rectum where defecation was so painful as to render life almost unbearable, he thought that rectotomy, though an undesirable operation, was preferable to colotomy in many cases, in which the latter is sometimes used.

Dr. DAWSON said: I have been very much instructed by Dr. Mathews' paper. Stricture of the rectum is one of the gravest diseases which we are called upon to treat, whether benign, malignant or specific. What are we to do? What am I to do in a case that I have under my control now? A young man, eighteen years old, with a large cancerous growth in the rectum, just at the end of the finger's reach—the mass is hard and gristly, and cancerous without a doubt—what can I do? A colotomy in a young man, as in one of any age, is but palliative at best.

Excision by the rectum would not, could not be complete, the entire mass could not be reached. The experience of all surgeons shows that an operation for malignant disease must be radical, every trace of the morbid growth must be removed, or our patient is damaged rather than benefited. In amputations of the breast for scirrhus, if the operation is deferred until outlines can be observed, until axillary involvement is manifest, that woman's life is abridged, is shortened by the procedure.

Can I remove this mass by an abdominal section? By removing a portion of the canal? Operations of this character have been undertaken in the stomach, the pylorus, and but recently upon the lower bowel. I intend to give this measure a trial.

In rectal strictures of syphilitic character, colotomy should be deferred, be held in abeyance. Some years ago an English lady consulted me for a fearful stricture of the rectum. It was specific without a doubt; she suffered for years the torments of the damned, but under a syphilitic course of treatment she finally recovered entirely. The deposits upon the mucous membrane of the bowels were removed, the lumen of the canal restored.

Dr. Mathews' paper is an important contribution to the literature and resources of this very important branch of our profession.

The following surgical appliances were presented by the author:—

WIRE EXTENSION SPLINTS FOR FRACTURES NEAR OR IMPLICATING ANKLE, SHOULDER AND WRIST JOINTS.

ECLISSES À EXTENSION DE FIL MÉTALLIQUE POUR DES FRACTURES PRES AU IMPLIQUANT LA CHEVILLE DU PIED, L'ÉPAULE ET LES JOINTURES DU POIGNET.

DRATHSCHIENEN ZUR EXTENSION BEI FRAKTUREN IN DER NÄHE DER FUSSSCHENKEL-, SCHULTER- UND HANDGELENKE, ODER DIESELBEN IMPLICIREND.

BY JOHN ST. PIERRE GIBSON, M. D.,

Of Staunton, Va.

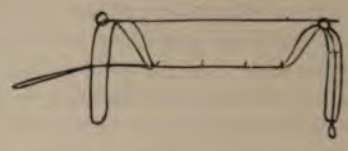
I would submit for your consideration three wire extension splints; designed for fractures near or implicating (1) the ankle joint, (2) the shoulder joint and (3) the wrist joint.

1st. The splint for fractures near and of the ankle joint combines with it suspension.

FRACTURES OF THE ANKLE JOINT.

The splint, Fig. 1, is made of No. 5 wire, of which take 159 inches and divide into two pieces, of 110 and 49 inches—or, to be sure you have enough, 110 and 52 inches. Take the 112 inches, and commencing with the part that goes above the knee, run 34 inches to ankle, bend in a line gradually upward, and curving about 7 inches,

FIG. 1.



then down about 12 inches, then at a right angle 4 inches; in the same way back to commencement:—

$$34 + 7 + 12 + 4 \text{; back} + 12 + 7 + 34 = 110 \text{;}$$

of the 52 inches, out of which is formed the counter-extension piece, measure 3 inches, then gradually curving upward about 7 inches, then downward 12 inches, then at right angle 5 inches, then backward as above:—

$$3 + 7 + 12 + 5 \text{; back} + 12 + 7 + 3 = 49.$$

The counter-extension piece is then soldered to the main splint, commencing about 18 inches from the end of the part above the knee. The splint is braced by pieces of No. 9 wire, soldered across. All the braces except those (2) above the knee, have the wire bent into rings for suspension cords. The splint in the straight part, from the ankle to the end above the knee, is braced about two inches apart, four below the knee with rings, and two above without. Each of the curved parts is braced at the top of the curve with rings five inches apart. In the centre of the foot curve is placed the extension appliance, which is made by soldering, about four inches from the top of the curve, another brace with a ring in the centre; opposite this ring, in the lower part of the foot curve, another ring is soldered. Through these rings a piece of No. 5 wire, about twelve inches long, with a ring turned on the lower end; after it has been passed through the rings, a button of solder is put on the upper end to hold it in place.

I have also found a rod, made of No. 4 or 5 wire, with a ring on one end, and a thread and nut on the other, passed through the rings at the top of the foot and extension curves, adds to the steadiness of the splint, and with it can be changed, to some extent, the amount of extension.

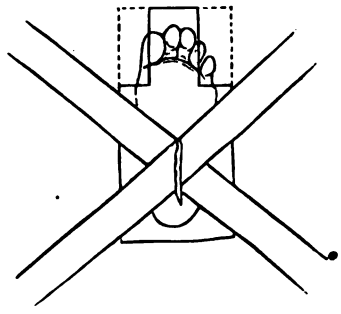
The splint is applied by first putting the adhesive plaster on the limb for extension,

FIG. 2.

Fig. 2. That for the foot is prepared like Fig. 3. It is cut sufficiently large to cover the bottom and the sides of the foot and heel, and to extend several inches above the toes; two pieces, obliquely crossing each other, are put across the centre of the

bottom of the foot, then a piece of bandage, four or five inches long and three broad, is sewed opposite the centre of the ankle joint; the whole is then applied to, and bandaged on, the foot. Two pieces of plaster (Fig. 2), three inches broad, are put on

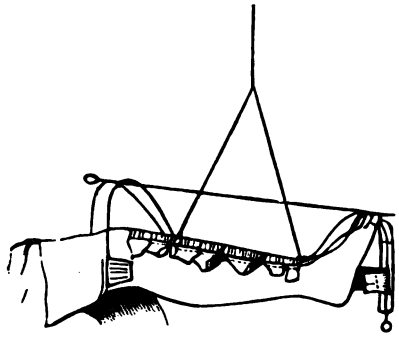
FIG. 3.



either side of the leg for counter extension. They commence at the seat of the fracture, and extend several inches above the knee.

The limb is suspended as follows: a piece of stout drilling, a yard long and about three-fourths broad, is torn into strips two inches broad, and extending into the cloth to within two inches of its middle, from the foot to the knee; that above the knee is not torn. These strips are then pinned on either side of the wire, and the cloth that goes above the knee is pinned over that part of the splint, after padding it. The cord is tied into the ring at the ankle and the one near the knee. The splint is then sus-

FIG. 4.



pended like Smith's anterior splint (Fig. 4). During forcible extension to reduce the fracture, the plasters on the sides of the leg are attached to either side of the counter-extension curve, and the piece of bandage on the bottom of the foot is attached to the wire in the middle of the foot curve, which is screwed well up, and a small piece of bandage pinned over it, to maintain extension. I do not rely on the adhesive plaster to make the extension, but it maintains it, and the springs make it continuous.

The special claims made for this splint are:—

1st. That it unites suspension with extension.

2d. That the springs make the extension continuous.

3d. That the way the plaster is applied to the foot, atmospheric pressure is taken advantage of, thus maintaining strong extension, without making sufficient pressure on the top of the foot to hurt it.

4th. That by attaching the piece of plaster which extends above the toes to the brace at the top of the foot curve, any distressing pressure is removed from the heel.

5th. The limb is open to observation.

6th. It is peculiarly adapted to the treatment of compound fractures.

7th. That the narrowness of the splint on top makes the cloth compress the muscles of the limb like a bandage.

FRACTURES OF THE HUMERUS.

The splint for fractures of the surgical neck, etc., of humerus is represented by Fig. 5, A, B ; its application by Fig. 5, C.

It is divided into two parts. The shoulder cap (Fig. 5, A) is made of tin, the arm part (Fig. 5, B) of No. 7 wire. The tin cap adapts it to either arm. It slips on the ends of the wire, which are annealed at the bend so as to facilitate the change of the

FIG. 5.

FRACTURES OF SURGICAL NECK, ETC., OF HUMERUS.

angle, which suits it to either arm. The tin cap (Fig. 5, A) is made $4\frac{1}{2}$ inches long and 4 inches broad. It is bowed to fit the shoulder. It has slots fitting the ends of the wire part (Fig. 5, B).

The wire part of the splint (Fig. 5, B) is made of 46 inches of No. 7 wire, as follows : Commencing at the shoulder end, run 4 inches, then 9 inches to above the elbow, curve over the elbow and run $8\frac{1}{2}$ inches, bend at right angle 3 inches, then same way back :—

$$4 + 9 + 8\tfrac{1}{2} + 3 + 8\tfrac{1}{2} + 9 + 4 = 46.$$

It is firmly braced, with the same sized wire, just above the elbow, three inches apart. The brace is bowed outward, and to make it take firm hold the ends are bent one and a half inches, which are attached to the splint by wrapping them to it with small

copper wire and soldering. This is important, as great stress is laid on this brace in changing the angles of the wire at the top of the shoulder, to make it fit either arm. On the lower end of the splint, on either side, rings are soldered, through which pass a piece of wire, with a ring on one end and a button of solder on the other, with which to make extension.

To apply the splint (Fig. 5, C), pass a piece of adhesive plaster, about a yard long and four inches broad, obliquely over the shoulder, and another piece, to the middle of which a piece of bandage is sewed, on either side of the arm, up to seat of fracture, with its centre at the elbow, where the piece of bandage for extension is sewed.

During forcible extension the splint is applied as follows: The shoulder cap is firmly bound to the shoulder by putting a piece of plaster over the one already applied, of the same width and about half as long; and another piece, three inches wide and a yard long, is put on just below the shoulder bend, to bind the upper part of the splint to the body. The extension is then made at the elbow, after which a piece of strong cloth, long enough to extend from the axilla to the top of the elbow and broad enough to pin around the arm and splint curve, is torn into strips, two inches broad, running into the cloth several inches on each side; it is slipped up between the arm and body and pinned over the splint. This is better than bandage, as it is easier to apply and remove. This splint being of recent invention, I have only treated with it one case of compound comminuted fracture of the upper part of the humerus, with good result.

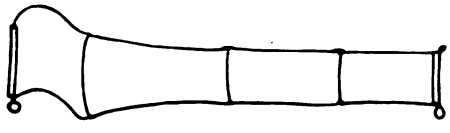
ARM EXTENSION SPLINT FOR COLLES' FRACTURE.

This splint is devised especially for treatment of fractures of and near the wrist joint.

It is easy to apply, and produces no uncomfortable pressure. Splints both bones in the semi-supine position, in which all the muscles are at rest and the bones parallel, with the idea of securing good motion, both rotative and supinative.

The splint (Fig. 6) is formed of 49 inches of No. 7 wire, commencing at the elbow,

FIG. 6.



running 12 inches to the wrist, 4 inches for hand, 5 inches for spring extension, 4 inches across, then back to elbow, with 3 inches across:—

$$12 + 4 + 5 + 4 + 5 + 4 + 12 + 3 = 49.$$

The whole length of the splint, including spring extension, is 19 inches. The length of the splint proper, that to which the bandage is applied, is 16 inches. The 12 inches from the elbow to the wrist, is braced three inches broad for the hand, four inches in length; it gradually widens to four inches and braced, then five inches are curved into a spring.

There are two braces of three inches, between the elbow and wrist, and one of four inches, below the knuckles.

For making extension, on either end of the splint on each side of the ends, rings are soldered, through which pass pieces of No. 8 wire, with ring turned on one end and a button of solder on the other.

The splint, after being bandaged, is applied as follows (Fig. 7): A piece of adhesive plaster, 2½ inches broad, is applied over both bones, commencing at seat of fracture,

and extending two or three inches beyond the elbow; the plaster is made to take firmer hold by several small pieces, partly encircling the arm. Below the fracture the plaster, three inches broad, runs over the back of the hand, extending three or four inches beyond the knuckles, and two pieces obliquely cross the back of the hand, intersecting each other.

The splint during forcible extension is attached, by adhesive plaster, to the hand and near the elbow (Fig. 8); the extension and counter-extension plasters on the hand and at the elbow are drawn tense and pinned. It is then bandaged to the arm.

Fig. 7.

It is claimed that it is the bones, and not the muscles, which are splinted their whole length, in an easy, natural position, and that the extension is made in a straight line, with the bones kept parallel. That the fingers being left free, there is sufficient motion to prevent stiffness or contraction of them.

I would again state that, while I have great faith in adhesive plaster to maintain extension, I do not rely on it to make it. If the bones are put right, I am confident this splint answers every indication to keep them right. It has done so with me.

Fig. 8.

That it does so with the least possible discomfort, as the splint proper does not touch the limb anywhere. It only keeps it in a good position with a comfortable bandage.

The lint can be inspected at any time, simply by removing the bandage without interfering with the splint, thus offering every facility for inspection and applying dressings. If found necessary, forcible extension can be made at any time, and the extension plasters readjusted.

The following paper was read by Dr. GRANT-BEY, of Cairo :—

ON THE SURGICAL TREATMENT OF LIVER ABSCESSES.

SUR LE TRAITEMENT CHIRURGICAL D'ABSCÈS DU FOIE.

ÜBER DIE CHIRURGISCHE BEHANDLUNG DER LEBERABSCESSE.

BY A. HADDAD, M. D.,

Of Alexandria, Egypt.

Professional opinion is widely divided on this subject, and to such an extent that some authorities would in no case recommend the opening of a hepatic abscess with a scalpel or even the use of a large trocar, but confine themselves in every case to the use of a small trocar, and some go so far as to employ the exploring needle attached to the aspirator; while others will not only adopt the free opening but also resect a piece of one or two ribs in every case of a liver abscess, in order to get a free access for fingers, sponges, forceps and other cleaning materials into its cavity.

There is neither need nor space here to go through all the discussions which have been recorded in support of each of these views. I shall begin by relating the facts of some selected cases from which important conclusions may be derived; for practical subjects such as this can only be decided by well collected facts derived from practice, and by fair direct inductions from them :—

CASE I.—Ali Hilal, age 25 years, a Moslem from Alexandria, Egypt, was admitted into the Deaconesses' Hospital, at Alexandria, under the care of Dr. Mackie, on October 25th, 1886. He had been a free liver, drinking often to the stage of intoxication during the three or four years previous to his illness. He had always enjoyed good health until the previous winter. While he was running after a runaway horse and carriage he felt pain in his right side as if something pierced him. He went home and was confined to bed for about ten days with fever and pain in the right side; after this he got out of bed but still suffered again from occasional attacks of fever, with rigors and pain in the side and was never able to resume his work. Ten days before admission he came for the first time to the out-patients' department of Dr. Mackie, who detected a liver abscess and punctured it with the trocar of an aspirator in the axillary line, between the eighth and ninth ribs; sixty-six ounces of laudable pus were taken away. The operation was followed by great relief.

On admission he was emaciated, temperature 37.8° C. He complained of much pain in region of the liver. Hepatic dullness in the right nipple, perpendicular line measured six inches and extended about two inches to the left of the xiphoid cartilage. There was no jaundice and no ascites, bowels were relaxed, heart and lungs normal.

November 1st. Temperature varied from 37.4° C. in the morning to 39° C. in the evening. He is weaker and has had more pain in the side. Punctured again; sixty ounces of pus, dark from admixture with blood, were removed; area of dullness decreased considerably in the perpendicular nipple line, it now measured four and a-half inches.

The operation was followed by some relief for the first two or three days, but soon afterward symptoms returned with again increased area of dullness.

November 11th. Punctured again; forty-eight ounces of clean, buff-colored pus were let out, the cavity not quite emptied, aspiration being stopped on blood beginning to appear.

November 18th. After the last puncture no relief was obtained; on the contrary, pain was much more than before, while patient had profuse night sweats and grew

daily weaker. A free incision was made between the eighth and ninth ribs in the axillary line, and nearly three pints of thick pus of brick-red color were evacuated.

Two large elastic drainage tubes were introduced and secured. On opening this abscess two clots of blood of irregular shape, about two by three inches each, were extracted; they were probably the cause of the severe pain which the patient suffered from after the last puncture.

This shows that the abscess ought not to be emptied completely; as an extravasation of blood very often occurs after a complete evacuation.

This case was only dressed with medicated cotton wool, without any washing of the cavity whatever, except occasionally with simple warm water fifteen or twenty days after the operation.

Patient made a speedy and perfect recovery, and was discharged on the 17th of December, in enjoyment of good health and strength, the opening having completely healed up.

CASE II.—Aman Guneid, a Soudanese, age twenty-three years, a servant, was admitted into the Deaconesses' Hospital, at Alexandria, under the care of Dr. Mackie, on the 25th of September, 1886.

He gave the following account of himself: About four months previous to his admission he suffered from attacks of fever coming on every afternoon, preceded by chill, and ending during the night in profuse perspiration. This state lasted about twenty days. A medical man in the village where he was gave him a bottle of mineral water and a white bitter powder; the attacks of fever subsided and never came on afterward. Patient admitted having had shivering fever attacks several times before in his life. A few days after the last attack he began to suffer from pain in the epigastrium and right side, which blisters, purges and other medicines could not alleviate. With the pain there was sleeplessness, want of appetite, and occasional sickness. A couple of weeks later he began to notice a swelling bulging from under the ribs, which grew gradually larger until it reached its present size. He never had dysentery and never used alcoholic drinks.

On admission, epigastrium presented a large rounded swelling, distinctly fluctuating, extending on the left side to the nipple line and downward as far as the umbilicus. Dullness over the right side extended right up to the nipples. Severe pain in side deprived him of all rest, and there was tenderness all over the swelling, spleen enlarged, no jaundice, bowels regular. Pulse 60, very feeble, extremities cold, temperature 36.6° C., no œdema of the feet. Heart and lungs normal. He was ordered cognac, September 27th. Temperature varied from 36° to 36.8° C. Pulse from 60 to 65. He was exceedingly weak and prostrate; cold perspiration at night; aspirated; three-quarters of a pint of brown pus mixed with blood removed. No echinococcus hooklets were detected by the microscope. Pus presented ordinary characters of a hepatic abscess in color and consistence. No relief followed the operation.

October 1st. Temperature this morning was 35.2° C. Pulse 62, very feeble. Patient is even weaker and could hardly be warmed by hot bottles, warm blankets and cognac. Pain also continued very severe. A free incision was made in the most fluctuating and pointing part of the swelling, which was about one inch below the right ribs and one and a-half inch to the right of the median line. Nearly two and a-half pints of thick pus escaped, and a piece of elastic tube was introduced into the cavity and covered with medicated cotton.

This operation relieved the pain, and the patient had more rest and could sleep much better, but all other symptoms persisted. Temperature kept very low in the morning, as will be seen by the chart hereafter given. Diarrhœa and profuse cold sweating in the night continued. Prostration increased daily until death, on October 8th, at 9.40 A.M.

Autopsy.—A large abscess occupying fully half of the right lobe of liver, and entirely confined to it, was found. The other half of right lobe was full of small abscesses of the size of a pea. Left lobe normal. Liver round the opening of abscess firmly adherent to peritoneal surface of abdominal wall. Other parts of peritoneum healthy. There was effusion into it, unaccompanied by peritonitis. Walls of abscess wholly formed of liver tissue, were rough and ragged, with numerous long bundles of sloughy tissue attached to them. No ulcers in the intestines, kidneys healthy, spleen enlarged. There was effusion into pericardium, with no signs of pericarditis. Heart and lungs normal.

One of the most interesting symptoms of this case is the low grade to which temperature sunk while extensive suppuration was going on. This may be due to the vast chemico-physiological processes which occur in the hepatic cells and make of this organ, as Dr. Harley has very ably pointed out, the greatest calorifying agent of the body, being arrested by the destruction of almost the whole right lobe, as has been shown by the post-mortem examination.

The chart of temperature in this case is as follows :—

Date.	Morning.		Evening.
Sept. 25, 1886.			37.3° C.
" 26.	36.0° C.		36.8°.
" 27.			37.0°.
Oct. 1. Day of Operation.	35.2°.	At Noon.	37.9°.
" 2.	34.6°.	35.7°.	38.7°.
" 3.	35.8°.		39.9°.
" 4.	35.8°.		39.8°.
" 5.	34.7°.		38.6°.
" 6.	37.0°.	At 10 A.M.	39.5°.
" 7.—8 A. M.	33.0°.	34.0°.	38.8°.

CASE III.—Hanipe Hassan, aged forty years, Moslem from Alexandria, admitted into the Deaconesses' Hospital, at this town, under the care of Dr. Mackie, on the 21st of April, 1884.

Previous History.—She said she had been subject to intermittent fever about four or five years ago, and once since then had had a kind of dysentery. Ten months ago she had a sort of fever, which lasted about three months, but she can give no clear account of this fever. She denies having had any pain on her right side or diarrhœa during that time, and says she had no repeated chills or periodical accesses, but a continued pyrexia ; at the end of this fever she felt pain in her knees and in her right side toward the last ribs, for which she entered the Deaconesses' Hospital and remained there ten days. She then left the hospital, the pain in her knees having subsided, but she had still pain in her right side, which continued up to this date. Patient says she first noticed a swelling on this side about two months previous to her admission. This swelling has since gradually increased until it reached its present size. Patient admits having indulged abundantly in all kinds of spirits and wines for many years.

Present Condition.—A defined tumor is detected in the right side of abdomen, extending from the ribs to the right iliac region. Dullness extends all over this part to the median line and upward to the nipples. An indistinct, resisting fluctuation is felt. Splenic dullness is normal. Patient is very weak and emaciated. She cannot stand and can hardly sit. Profuse night sweats. Tongue moist and coated white. Bowels are relaxed. No jaundice. Owing to the shaking of the carriage while she was being transported to hospital, she felt severe pain on her arrival, which necessitated an immediate tapping of the tumor, to relieve it. About two pints of chocolate-colored pus were removed by the aspirator. Under the microscope only pus corpuscles and oil globules were detected.

April 25th. Pain and general symptoms improved after operation, but yesterday she was again as bad as ever. Aspirated, and two pints of the same kind of pus let out.

April 29th. Aspirated again. One pint of pus extracted, the tube of aspirator being plugged by pus flocculi. The pus was rather thicker and more flocculent this time. On May 3d, one and one-half pints, and on May 10th, two pints were extracted by the aspirator, but these operations were followed by no improvement whatever, and patient was getting very weak.

On May 14th a free opening was made between the eighth and ninth ribs, in the axillary line, and nearly five pints of thick pus full of large fibrinous flakes were let out, and a piece of elastic tube of a large calibre was introduced into the cavity and secured. Cavity was washed out daily with boric acid lotion (gr. x–ʒj) and carb. ac. lotion. This operation gave immediate relief. Pain and fever subsided, and a few days afterward the night sweats ceased also. Her strength rallied very slowly, as the discharge was profuse, and it was not until the 1st of August that she could leave the hospital in enjoyment of fairly good health, but still with slight discharge from the opening.

About a year after the operation, on the 9th of April, 1885, she was again admitted into the hospital under the care of Dr. Mackie, with a sinus extending from the opening of the abscess, from which a few drachms of pus escaped daily. She was in pretty good health, except that she was still rather weak and anæmic.

On the 11th of April, about two inches of the eighth and ninth ribs were resected and a small cavity with several pouches and cul-de-sacs were exposed to view. Sponges on sponge holders were introduced and a good quantity of large, fibrinous flakes and ragged masses of broken-down hepatic tissue were sponged out of these pouches, and these were washed out with boric acid and carbolic acid lotions. This was done for several days after, and within a month patient left the hospital with the opening permanently cured.

Case iv.—Mohamed El Nagiar, æt. twenty-eight years. Mason, from Alexandria, admitted into the Deaconesses' Hospital, at this town, under the care of Dr. Mackie, on May 20th, 1886.

About four months previous to his admission he began to suffer from occasional attacks of shivering and fever, accompanied with pain in right side. The attack used to last three or four days, after which time patient felt relieved, but next day, two or three hours after he had recommenced his work, another attack used to set in and compel him again to give up his work and go to bed. This state lasted two months. He was then altogether laid up with fever, perspiration during sleep, loss of appetite and pain in right side. Twenty days before admission he first noticed a swelling below the right ribs, which was very tender, and had between then and admission increased considerably. Patient says he never tasted wine or spirits until six months previous to his illness, when he got into the habit of drinking to the extent of intoxication every evening. Eight days before admission he came to the out-patients' department of Dr. Mackie at the Deaconesses' Hospital, who diagnosed a liver abscess and punctured it between the eighth and ninth ribs; two and one-half pints of red, brick-colored pus were extracted by the aspirator. Great relief followed the operation.

On admission, the general symptoms and swelling had returned. Patient looked very weak, perspired profusely at night and had diarrhœa. No jaundice. On May 26th a free incision was made between the eighth and ninth ribs, and nearly three pints of thick pus, slightly reddish colored, were let out and a piece of elastic tube was inserted. The operation gave immediate relief. The fever and pain ceased on the same day; the diarrhœa stopped and the appetite returned soon after. Patient, however, improved very slowly, free discharge having continued from the opening for several

weeks. On July 27th he left the hospital in a pretty fair state of health, but the wound was still open and some discharge escaped from it daily.

On August 25th he came back to Dr. Mackie for consultation. There was a deep sinus connected with the opening of the abscess discharging a good amount of pus daily.

Patient was still very weak and anæmic, and could not attempt to do any work. He was readmitted into hospital, and on August 27th he was operated on. About two inches of the ninth rib were excised and a large cavity about the size of a man's fist, which was lying behind it, was exposed; many other smaller cavities were communicating with this, and the granulating hepatic tissue between them appeared fungous and partly overlapped some of them. They were all full of ragged masses of hepatic tissue, either still attached to their walls or detached from them.

These cavities could not be washed clean through the first opening, owing to their position. The ribs having been excised, they were now sponged clean, washed with boric acid lotion, powdered thoroughly with iodoform. This was done daily, and on Oct. 26th the wound had completely closed; patient was discharged in perfect health and ready to resume his work.

The following conclusions, it seems to me, may be fairly deduced from these collected facts:—

1. Liver abscesses are like all other abscesses in other parts of the body, with regard to the character of the pus they contain and to their form. They may contain either a laudable pus, stained or not with blood, or a curdy, flocculent matter containing flakes of curdy lymph and débris of broken-down tissue in different proportions. They may be composed of a single regular cavity with limiting membrane, or of several fissures and cul-de-sacs, communicating with an irregular sort of cavity.

2. Hence, it results naturally that different modes of treatment are required for the different sorts of abscesses, the rule evidently being *to make an outlet in the simplest and safest manner possible, free enough to allow of the complete evacuation of the contents of the abscess and to prevent any reaccumulation.*

3. There is no doubt that the simplest and safest manner to empty a liver abscess is to puncture it with a fine trocar and aspirator. But, unfortunately, this means is not applicable in every case, or rather I should say, is not enough in every case, as Cases I, III and IV have shown. In all regular abscesses with limiting membrane and laudable pus free from large flocculi, which cannot pass through the canula, this operation is likely to be successful, while in other cases it is sure to fail. But as the internal state of an abscess cannot be guessed by an external examination, and as the puncturing is very rarely, if ever, attended by danger or bad accident (on the contrary, in all cases in which I have seen it done for the first time, it has been followed by great relief. Though often temporary, still it was sometimes permanent), it would be best invariably to begin by trying to empty the abscess gradually with a fine trocar and aspirator. Then, if it is judged, by the character of the pus and the state of the patient, that this means of exit is not sufficient, some other way should be resorted to. By so doing not only is the chance of cure by the safest way given to the patient, but the danger of shock, which occurs sometimes from entirely emptying a large abscess at one time, is avoided.

4. When aspiration has failed to attain the desired result, the next step to be taken must, necessarily, be an opening free enough not only to allow the passage of thick pus and large flakes, but also, in some cases, to procure the entire exposure of the cavity in order to get to the excavations, fistulous passages and cul-de-sacs which may exist in it, and to clean them thoroughly from detained pus and broken-down tissue, which could not be washed out by simple syringing.

5. When the abscess has pointed behind the ribs, a simple free incision between them can only be wide enough to answer the first of these two purposes—the passage of

the large flocculi—but there cannot be enough room to look through or for the admission of sponges and forceps. Therefore, in those cases where these advantages are required a simple free incision is not sufficient, and in order to obtain them a piece of one or two ribs should be resected. However, this last necessity seems to be of rare occurrence, a simple free incision between the ribs having proved sufficient to attain a cure, after the failure of aspiration, in many cases of liver abscesses, of which Case I is a good example.

6. It seems not only superfluous, but also contrary to the rules of conservatism which must be observed in every operation, to begin in the first instance by resecting a piece of the rib in every case of liver abscess, since so many can be cured without it. It is a well-established principle that no more of the body should be removed than the necessities of the individual case require. Therefore, to cut away a piece of one or two ribs and thus deprive the organ behind them of its natural protection and expose it to all sorts of injuries, can only be justified by unavoidable necessity. In all the cases which have done well without this operation, it would have been in direct opposition to that established principle if it had been done; and in those which have required the operation, the necessity to perform it could only be seen after the free incision alone has been given a fair trial. Consequently, it seems logical not to resort to the resection of ribs until after the failure of the simple free incision.

In conclusion: the rule which ought to be followed in the surgical treatment of liver abscesses, (it seems to me) is to invariably begin its evacuation by means of a fine trocar and aspirator, and when this proves insufficient, to make a free incision; and if this also fails to secure the desired end, to resort to the resection of a piece of one or two ribs.

This way is followed by Dr. Mackie in his large practice, and seems to me to be theoretically and practically the best.

DISCUSSION.

Dr. Randolph Winslow, of Baltimore, said he had had occasion to make an extensive study of this subject a few years ago, and that he had certain opinions upon it which he would like to bring to the notice of the Section. In the first place, abscess of the liver is not a rare disease in this part of the United States, notwithstanding the generally accepted opinion that it seldom occurs in temperate climates. A number of cases had come under his own notice, and that of his immediate acquaintances, within a comparatively short time. In this section the abscess is frequently consecutive to dysentery, and metastatic or pyæmic in character. Pus is a foreign body, and its evacuation is demanded, whether it be seated in the liver, pelvic cavity, brain or lungs. An abscess of the liver ought to be treated in the same manner as an abscess of any other organ or tissue; if the pus is accessible, evacuate it. For diagnostic purposes the aspirator is invaluable, and in some cases the diagnosis can only be made with its aid; as when the abscess occupies the convex surface of the liver, pushing up the diaphragm, or when it points posteriorly, in which situations it may be impossible to elicit the sign of fluctuation. Aspiration is also useful as a therapeutic agency, when there is but a small quantity of pus, a certain number of cases being permanently cured under such circumstances, but in large abscesses it cannot be relied upon for a cure; the abscess cavity almost invariably refills, as is shown in the report which has just been heard. It is proper to aspirate once, perhaps twice, but if the pus reforms, make a free incision into it, and treat it as if it was an ordinary abscess. If the collection points posteriorly, it may be proper to resect a rib or two in order to allow free drainage and exploration; but it not unfrequently points in front, as has been seen by the speaker in at least two cases.

Incise at the seat of greatest convexity; cut down to the peritoneum; if adhesions have formed so much the better, if they have not, stitch the liver to the abdominal wall and incise its substance. The older surgeons were very adverse to making an incision into the liver, and even preferred to leave the case to nature; but any operative treatment is preferable to abandoning the case to nature. In conclusion, it must be insisted that the same antisepsis and cleanliness is demanded in operating upon the liver as is required in any other serious operative procedure. We cannot expect success if we use dirty instruments or have unclean fingers, but if the usual antiseptic precautions are employed, recovery will take place in the large majority of our cases.

F. PARCHAL, M.D., of Chihuahua, Mexico, said:—

Abscess of the liver is of frequent occurrence in the Mexican. This is due to the use of highly stimulating food containing a great deal of red pepper, which produces catarrhal duodenitis, then inflammation of the common duct, and finally abscesses.

The treatment by aspiration has not given me favorable results, only one case of thirty-five operated on being cured by this means alone—that a child, seven years old, who recovered after one aspiration. Six cases, by free incision, antiseptic irrigations and drainage, terminated fatally. The most favorable results that I have obtained has been by drainage in water; nine cases being cured by this method. Puncture with the aspirator needle is made twice, at intervals of five days, to insure adhesions; a large trocar and canula is introduced, and during the flow of pus, a drainage tube perforated at one end is inserted into the cavity and the opposite end immersed in aseptic water, to be frequently renewed. It is all important that *air should not* enter the cavity, and to aid this, the wound around the tube should be covered with collodion and gauze. No rule can be formulated for treatment. Drainage in water, however, seems to me of great importance.

Dr. I. N. QUIMBY said:—

I have been much interested and instructed by the paper read by Dr. Grant-Bey, on treatment of abscess of the liver. The doctor states that he only partially evacuates the pus sac, for fear that if it is entirely evacuated there might be some extravasation of blood.

There may be some reason for that method of treatment in the East which does not apply here. But our mode of treating abscesses in other parts of the body is somewhat different, as we usually completely empty the pus sac, and then cullenderize it, by injecting some antiseptic fluid, either carbolic or bichloride solution. The pus sac is hyper-distended with this solution, and washed out till the discharge runs out clear, or nearly so, after which moderate pressure is made over the region of the abscess by compress and bandage.

This method works admirably in other parts of the body, as the pus cavity thus treated becomes diminished and the quantity of pus lessened, and the abscess disappears more rapidly under this mode of treatment than any other.

As this method is so successful in removing abscesses in other parts of the body, I see no reason why it could not be applied to the treatment of abscess of the liver.

The following papers were read by the author :—

NEZ ARTIFICIEL EN CÉRAMIQUE SE FIXANT SANS LUNETTES.

SELF-RETAINED ARTIFICIAL NOSE.

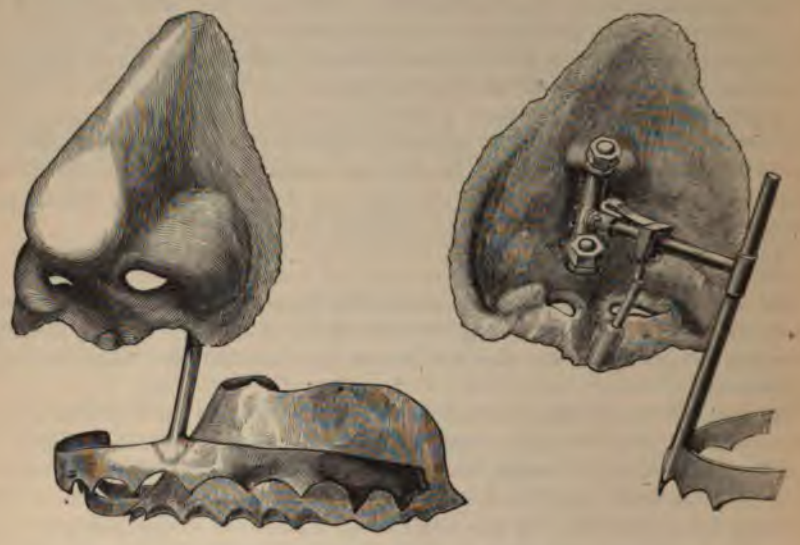
SELBSTHALTENDE KÜNSTLICHE NASE.

PAR DR. CL. MARTINY,

de Lyon, France.

Dans la séance de la Société de Chirurgie du 26 juin, 1878, M. le Professeur Verneuil voulut bien présenter un de nos malades, porteur d'un nez artificiel.

L'éminent chirurgien signalait l'heureux résultat obtenu : les difformités étaient parfaitement dissimulées ; la prothèse avait atteint un haut degré de perfection et l'emportait de beaucoup sur ce que l'autoplastie avait donné jusqu'alors, dans des cas d'égale gravité.



Et, en effet, sous le rapport de la forme, de la couleur, de la transparence, en un mot de ressemblance parfaite avec la peau, la nature avait été fidèlement imitée ; l'adaptation ne laissait rien à désirer.

Disons immédiatement que le résultat doit être attribué non seulement à la matière employée pour la fabrication du nez, mais surtout aux modifications apportées aux moyens de fixation.

On profite quelquefois de la perforation congénitale ou accidentelle de la voûte palatine pour maintenir les nez artificiels. Quand la perforation n'existe pas, c'est par l'intermédiaire de lunettes que l'appareil est assujetti. Je ne veux pas énumérer ici les inconvénients d'une méthode que j'ai totalement remplacée.

C'est à une tige fixée dans les fosses nasales que s'attachent mes nez artificiels à l'aide d'un mécanisme particulier.

La tige nasale fait partie d'un appareil qui prend son point d'appui dans la bouche. Cette pièce métallique est moulée sur la voûte palatine comme la plaque d'un dentier. Elle est munie d'une lame également métallique qui passe en avant des gencives sans les comprimer. A cette lame, un peu à côté de la ligne médiane est soudée une tige qui doit perforer le sillon labio-gingival supérieur, et en passant à côté de l'épine nasale faire saillie de un ou deux centimètres dans la fosse nasale. Cette tige perforant le cul-de-sac gingival est parfaitement tolérée ; elle ne provoque par sa présence aucune inflammation ; certains de mes malades la tolèrent admirablement depuis cinq ans. Elle présente un point d'appui absolument fixe, étant donnée la disposition de la pièce buccale. C'est à cette tige que se fixe le nez artificiel.

Celui-ci est en porcelaine ; on lui donne la forme du nez absent et aussi sa couleur ; à sa face interne et retenues dans son épaisseur sont deux vis de platine servant à fixer une petite pièce à laquelle s'adapte au moyen d'une articulation à boule un tube métallique jouissant ainsi d'une certaine mobilité dans tous les sens. Ce tube a une direction horizontale ; creux dans toute sa longueur, il renferme une tige métallique retenue au fond du tube par un ressort, mais pouvant s'allonger au-dehors de deux centimètres. Un cran d'arrêt sert à le fixer dans cette position. Pour le faire rentrer, il suffit de presser sur un bouton caché dans une échancrure de la face inférieure du nez. Ce bouton fait échapper le cran d'arrêt, et sous l'action de son ressort, la tige centrale rentre dans le tube. L'extrémité libre de cette tige porte un manchon à direction verticale dans lequel peut s'enfiler la tige fixée dans les fosses nasales.

Il est facile de mettre en place le nez artificiel. Pour cela on allonge la tige horizontale j'usqu'au cran d'arrêt. On a alors suffisamment de place pour introduire son extrémité dans les fosses nasales et enfiler son manchon sur la tige qui y fait saillie. On presse le bouton sous-nasal, le cran d'arrêt s'échappe, et le nez artificiel attiré en arrière, s'applique sur l'ouverture qu'il doit recouvrir. De même pour le retirer, il suffit d'écarter la pièce prothétique jusqu'au cran d'arrêt, et de soulever le tout pour que le manchon glisse le long de la tige nasale et devienne libre.

Ce nez qu'on peut placer et enlever facilement à l'aide d'une seule main, est solidement fixé à la face dont il suit les mouvements. Son mode de fixation par une tige mobile et pourvue d'un ressort lui permet de suivre, sans cesser d'y être appliqué, les téguments sur lesquels il s'adapte.

TRÉPANATION DES EXTRÉMITÉS RADICULAIRES DANS LA PÉRIOSTITE ALVÉOLO-DENTAIRE.

TREPANNING OF THE RADICULAR EXTREMITIES IN THE DENTAL ALVEOLAR PERIOSTEUM.

TREPANIREN DER WURZELENDEN IN DER ALVEOLO-DENTALEN PERIOSTITIS.

PAR DR. CL. MARTINY,

de Lyon, France.

En 1881, dans une communication au congrès d'Alger, je proposais une méthode nouvelle de traitement de la périostite chronique alvéolo-dentaire, consistant dans la section pratiquée directement et sur place à l'aide d'une couronne de trépan de l'extrémité radiculaire malade.

Or, cette méthode a été reproduite inexactement dans l'article dent du Dictionnaire des sciences médicales, (pp. 316 et 378,) puisqu'il y est dit que "je pratique la résection du sommet à la faveur d'une trépanation préalable de l'alvéole donnant accès dans le foyer à une pince de Liston." Il n'est rien dit de semblable dans ma communica-

tion non plus que dans un mémoire paru dans le Lyon médical (1881). La même couronne de trépan qui perfore l'alvéole au point choisi comme le plus près de l'extrémité radiculaire sectionne aussi en continuant son action la racine elle-même. L'opérateur est averti que cette section est achevée par un changement de résistance perçu par la main.

Cependant il peut arriver que le trépan animé par le tour dentaire de 3000 tours à la minute aille trop loin et ressorte du côté de la bouche. Cela m'est arrivé dans une opération démonstrative à l'école dentaire de Paris. Quoique le résultat définitif ait été excellent dans ce cas, pour éviter cet accident, j'ai ajouté au trépan un anneau mobile qu'on fixe de manière à ne laisser pénétrer l'instrument que d'une longueur déterminée.

La portion de la racine sur laquelle a agi le trépan est retirée avec lui. Si la section porte sur le sommet lui-même, la réparation est prompte. Si elle porte au-dessous, le sommet sera bientôt détaché par la suppuration et entraîné au-dehors.

M. Magitot, défendant la greffe par restitution à laquelle je proposais de substituer la trépanation, a reproché à cette dernière de produire de grands délabrements de ne pas permettre de juger de l'étendue du mal, et de n'avoir été pratiquée qu'un petit

nombre de fois et depuis trop peu de temps pour qu'on puisse juger des résultats. Je répondrai simplement que le délabrement produit par l'opération est des plus minimes, qu'on peut juger à priori du point où devra porter le trépan, et enfin que mes observations sont au nombre de 45, dont quelques-unes ont été suivies depuis six et sept ans, avec conservation de la dent, et sans changement de couleur.

Tandis qu'avec la greffe par restitution, la douleur qui suit l'avulsion et la reimplantation est vive et persiste pendant plusieurs jours en nécessitant souvent un appareil de contention, après la trépanation il ne reste que la sensibilité due au traumatisme, il n'y a pas de douleur tenant à l'ébranlement d'une dent, et l'opéré peut quelques instants après manger sur l'organe malade.

Enfin dans aucun cas on n'est exposé à la chute de la dent. Ce procédé est de plus applicable aux simples racines qui conservées serviront à soutenir une dent à pivot.

Conclusions.—La trépanation de l'extrémité radiculaire des dents est une opération simple, sans dangers, ne laissant pas de douleurs après elle, ses suites sont bénignes, (je n'ai jamais eu avec elle que des succès,) enfin ses indications sont plus étendues et ses efficacités plus grandes que celles des autres procédés employés pour la conservation des dents.

The following paper was read :—

TREPHINING IN INSANITY FOLLOWING FRACTURE OF THE SKULL—REPORT OF A SUCCESSFUL CASE.

TRÉPANATION DANS LA FOLIE RÉSULTANT DE LA FRACTURE DU CRÂNE —RAPPORT D'UN CAS QUI A REUSSI.

TREPANATION BEI WAHNSINN NACH SCHÄDELBRUCH—BERICHT EINES ERFOLGREICHEN FALLES.

BY JASPER J. GARMANY, A.M., M.D., F.R.C.S.,

Of New York.

Encouraging success has attended the use of the trephine for the cure of traumatic epilepsy. The cases of traumatic insanity in which the trephine has been resorted to, have given good results, sometimes even brilliant.

Indulge me a few minutes while I recite briefly the history of a recent case of traumatic insanity, cured by trephining.

History.—Miss H——, in August, 1868, was a girl of 18, in good health, except occasionally suffering from dysmenorrhœa. There was no history of insanity in her family.

She was struck on the top of the head by a brick falling from the cornice of a three-story house (between thirty and forty feet). The brick inflicted a compound depressed fracture in the median line at about the junction of the sagittal to the coronal suture. She was knocked down and in a few minutes was seen by Dr. Dickson, of Pittsburgh, Pa., who says she was in a "semi-unconscious" state.

Dr. Dickson trephined the skull and elevated the depressed bone. The longitudinal sinus was wounded, the hemorrhage from which was controlled by compression. The wound healed promptly without any complications. Her mind seems to have suffered no detriment, but rather to have improved subsequent to the accident. She was considered intellectual by her friends.

Ten years later (March, 1878) Dr. Cowley, her physician, noted that from overwork and worry she was in a confused state, forgetting words and becoming exhausted from slight mental effort.

In 1883 she was treated for proctitis, dependent upon a retroverted uterus. Her left ovary was sensitive, and she suffered chiefly at her menstrual periods with severe headache, referred to the occiput.

Examination.—The examination of the patient was conducted (April 30, 1887) with the aid of Drs. Jones and Pardee (Philadelphia). Patient is suffering from acute mania, which is said to have commenced three weeks ago. The mania has been of a sensuous type. A depressed white scar two inches in length extends anteriorly from a point in the median line one-half inch in front of the middle point of a line drawn from the root of the nose to the external occipital protuberance. The cicatrix appears to rest upon bone, is glistening white, and not particularly sensitive. There are no motor nor sensory symptoms.

The patient is emaciated. The uterus is retroverted and attached to the rectum. The left ovary is sensitive. Her appetite is good ; she is constipated. The relatives say that the attack was caused by worry and anxiety concerning an affair in which her emotions were enlisted.

Trephining is recommended, with the possible necessity of removal of the ovaries.

Consultation.—Dr. C. K. Mills, in consultation, said that in view of the improbability of recovery trephining was indicated.

Operation.—Dr. Howard A. Pardee assisted at the operation (May 8th, 1887), and was associated in the after-treatment of the case. The head having been shaved, ether was administered. The head was scrubbed with soap and afterward douched with a solution of bichloride of mercury 1–3000. Towels wrung out in this solution were placed over the table and around the head.

An incision was made from a point midway between the external occipital protuberance and the root of the nose, and extended anteriorly a little to the left of the median line. The incision was about four inches long, reaching almost to the forehead. Two incisions a little over three inches long were made at right angles to this incision, having its extremities as their middle points. The hemorrhage was profuse, and was arrested with difficulty.

The left flap was dissected from the skull and pierced with a ligature by which it could be retracted. The right flap was dissected from the bone, but the centre of the cicatrix was found connected to the longitudinal sinus, the bone being deficient and having a longitudinal opening three-fourths of an inch in length and one-fourth of an inch in width.

The portion of the flap attached to the dura was separated from the rest of the flap by means of scissors. This flap was likewise pierced with a ligature and the two flaps held apart by fastening the ligatures together under the occiput.

A Galt's trephine was applied posteriorly and to the right of the depression, and an uneven button of bone removed. The semi-circumference of the button on the side toward the depression was one-fourth of an inch thick, while the opposite semi-circumference was one-eighth of an inch in thickness. An artery of considerable size entered the centre of the button.

Progressing anteriorly the bone was cut away, by means of a rougeur, along the right side of the depression. The bone was thick and of flinty hardness. It was necessary, on account of the thickness of the bone, to form a groove by gnawing the bone at a right angle to the surface, in order to allow the rougeur to act.

The bone was cut away to the level of the anterior extremity of the depression. The bone was now gnawed across the longitudinal sinus, to the left of the depression anteriorly, and the section continued backward along the left side, to the level of the posterior edge of the trephine section. A horizontal cut being made with the rougeur, connecting the opening behind the depression, left an island of bone, all around which the skull had been cut away to the extent of half an inch. The bone was very vascular, hemorrhage occurring in spurts. The island of bone was gnawed away slightly on the right side, when it separated into two pieces, the left piece being much the larger.

The right portion was raised from the dura, and where its shelving edge adhered to the central cicatrix was separated by means of a scalpel. The piece of bone measures $\frac{3}{4}$ by $\frac{1}{4}$ of an inch, and was adherent to the dura throughout. The left portion was separated anteriorly by means of an elevator. It was closely adherent for one inch posteriorly, and its separation was attended with profuse hemorrhage. It likewise was closely attached, centrally, at its shelving edges. The piece of bone measures $1\frac{3}{4}$ by 1 inch, and is pierced, posteriorly, by numerous openings for blood vessels.

The opening through the skull is about $3\frac{1}{2}$ by $2\frac{1}{2}$ inches, its long axis being antero-posterior. The pulsations of the brain can be felt.

Hemorrhage having been arrested the flaps were stitched together; that portion of the scar attached to the dura being partially covered.

Drainage tubes, guarded with safety pins, were placed in the four angles, extending through the scalp and resting on the bone. An iodoform and bichloride dressing was applied. The operation was performed with all antiseptic precautions and under irrigation ($HgCl_2$ 1–3000).

Treatment.—The patient was sustained during the operation, which lasted three

hours, by a copious injection into the rectum, of warm water and brandy (water, Oij, brandy, ℥j) and by hypodermics of brandy. Sleep followed the operation with an interval, in which Miss Wadly (Bellevue trained nurse) noted "mind apparently perfectly clear during one waking interval." P. M. T. 99⅘. P. 96. R. 27. *Fluid food.*

9th. Passed comfortable night (catheterized). A. M. T. 99¾. P. 110. R. 22. Serum has oozed through the dressing twice. The dressing was soaked with bichloride solution 1–3000, and a fresh dressing applied over the old one each time. P. M. T. 99⅘. P. 100. R. 26.

10th. Passed a fair night. A. M. T. 99⅘. P. 100. R. 25. The discharge came through the dressings which were covered as before. Patient complains of headache and soreness about the wound.

11th. Had a quiet night, but awoke in a more than usually excited state. Pass urine for the first time without the aid of the catheter. A. M. T. 99$\frac{7}{10}$. P. 92. R. 22.

11th. All the dressings removed, the tubes shortened, and another antiseptic dressing applied. The wound was aseptic. P. M. T. 99⅘. P. 104. Patient is greatly excited.

12th. The excitement continued all night, necessitating the use of restraints. She talked incessantly. A. M. T. 99½. P. 100. *Allowed solid food.* P. M. T. 98$\frac{9}{10}$. P. 90. R. 18.

13th. Patient slept only four hours. A. M. T. 99⅘. P. 102. R. 22. Patient requires restraint; has a ravenous appetite. P. M. T. 99⅘. P. 102.

14th. Slept only one hour all night. A. M. T. 99⅘. P. 136. The patient is very violent. The dressings were changed and tubes removed. The wound has healed by primary union.

15th. A. M. T. 99⅕. P. 108. Patient very quiet.

18th. Sat up out of bed for the first time.

19th. Slept soundly for nine hours.

21st. Dressings removed. The wound healed perfectly.

23d. Patient extremely depressed, shedding tears for the first time.

24th. Awoke rational after a quiet night. Remained semi-rational all day.

June 3d. Improved greatly, realizing the state she has been in, and begs to be taken away from its associations.

4th. Removed from the asylum.

During July and August her improvement has been continuous.

Sept. 1st. She improved to such an extent that Dr. Talcott writes that she corresponds with her friends, her letters being well composed. Her conversation is natural, and her memory is good, except of affairs connected with her illness. She reads with pleasure. She sleeps soundly without dreaming, and has gained eighteen pounds in weight.

There are a few matters of interest to which I will now ask your attention in considering the subject of traumatic insanity.

Locality of Lesion.—From the study of the reports of a large number of cases the *locality* of the injury will be seen to be of prime importance.

Serious mental disturbance, by which is meant loss of self control, of attentiveness; of mania; dementia; idiocy occurs in 50 per cent. of those who suffer from traumatic epilepsy, following injury in the frontal region.

A marked decrease in the percentage of cases of psychical derangement is noticed in injuries to the motor and sensory zone (ascending frontal, ascending parietal and isle of Reil) followed by epilepsy. This percentage, which is not half so high, is still further decreased by the elimination of cases in which the frontal or parietal regions are also involved. The parietal region shows a percentage as great as the frontal, of cases of traumatic epilepsy exhibiting marked mental disturbance.

What has been said concerning the motor sensory zone applies equally to the occipital and temporal.

Nature of Lesion.—Again, of prime importance is the nature of the lesion. It cannot even be said that the locality of the lesion more definitely determines the development of psychical disturbance than does its nature.

The symptoms of lesions in the frontal and parietal regions will alone engage our attention. These symptoms are identical for similar lesions.

I. *Destructive Lesion.*—A destructive lesion, such as abscess, hemorrhagic softening, as after embolus, tumors, including gumma, etc., may exist without any marked psychical disturbance.

II. *Great Destruction or Atrophy.*—Great destruction may be accompanied by a change in character, such as irritability, loss of attentiveness, loss of the power of self-control, or as in cases of bilateral atrophy in dementia or idiocy.

III. *Irritative Lesion.*—When the lesion is such as might not improperly be termed an irritative lesion in contradistinction to a destructive lesion, acute mania with violent outbursts of passion complicates over fifty per cent. of the cases after the development of traumatic epilepsy. The case of a slow-growing tumor attached to the meninges, a fibroma, for example, or a gumma should be included in the category of irritative lesions.

Blindness.—The blindness which complicates lesions of the frontal region is caused by intracranial pressure. If blindness complicates lesions of the parietal region, it is dependent either upon intracranial pressure or upon the additional involvement of the occipital region. The cases of mental disturbance, consecutive to irritative lesions, are mostly caused by compound fracture of the skull.

Epilepsy is generally the precursor to the psychical derangements, although there are a few cases reported in which their order was reversed.

It is pertinent to the subject, to give a short account of the most ordinary form of irritative lesion and its consequences.

(*a*) *Scar.*—These cases being mostly the result of a compound fracture, as already stated, show a scar. This scar is generally white and glistening, sensitive, perhaps also painful, and often intolerant to pressure, which may produce dizziness or convulsions.

(*b*) *Scalp.*—The scalp about the scar is generally, if not always, congested, making it likely that the surface heat is greater at this locality than elsewhere on the scalp. This subject has received special attention in tumor of brain.

(*c*) *Bone.*—The bone is often deficient under the scar, tough fibrous tissue taking its place. The bone may be carious, with a sinus leading to it. It may be spongy and readily scraped away, like cancellous tissue, or it may be of ivory hardness, thicker or thinner than normal, and nearly always very vascular. The bone may be depressed or there may be sharp projections from its inner table.

(*d*) *Dura Mater.*—The dura mater may appear healthy. It may be adherent to the bone or scar, be congested, discolored, thickened or the seat of a new growth.

It is the involvement of the dura, so rich in its nervous supply, which causes the pain, the vasomotor disturbances and the mental derangement. The nerves of the dura involved in the cicatrix or irritated by a depression or speculum of bone, makes the irritation continuous. Dalton first (1872) showed that the reflexes from the dura occurred on the same side. The remarkable reflex irritability of the dura has also been demonstrated by the labors of Duret.

(*e*) *Cortex.*—Through the vasomotor system the irritated dura causes a congestion in the contiguous cortical substance. Perhaps the local heat may be an important factor in determining blood to the part. Increased vascular supply causes greater activity of the cells of the cortex. In this manner can be explained the cases of idiocy cured by such

a lesion, also the intellectual improvement in the case whose history we have recited, which is by no means an isolated one. The nerve cells later begin to disappear and give place to fibrous tissue. It is very evident that success is oftener to be expected from early operation.

In regard to the collection of serum so often encountered under the dura, nothing positive can be said. It seems to be confined by the agglutination of the membranes, for it often exerts pressure. Doubtless the operation soon causes it to be absorbed and partially to ooze through the perivascular spaces of the dura.

Some time must generally elapse to allow the disturbed areas to recuperate.

It cannot be the destruction of the cortex contiguous to the lesion alone, but also the impairment of all those areas reflexly stimulated, which causes the psychical derangement.

DISCUSSION.

Prof. E. P. Thwing, M.D., of Brooklyn, N. Y., records observations of a successful treatment of this malady, which he witnessed in London, by Prof. Victor Horsley. Patient twenty years of age, two years victim of epilepsy; athetosis of thumb and other signs located the lesion. First button, an inch and three-quarters in diameter, removed and tumor exposed. Two more buttons removed and tumor detached, with one-sixth of an inch of the cortex, being the epileptogenous envelope. Wound healed in a fortnight, without inflammation. Professor Thwing examined the patient six weeks later and found him in excellent health. No return of epilepsy; a year having passed. Three other patients have been successfully treated by the same.

The following paper was read:—

LE MICROORGANISME D'UNE FORME DE GANGRÈNE SÉNILE.

THE MICROÖRGANISM OF ONE FORM OF SENILE GANGRENE.

DER MICRO-ORGANISMUS EINER FORM VON GANGRENA SENILIS.

PAR LE DR. ERNEST TRICOMI.

Dans la littérature il n'y a aucun travail qui fait mention, dans l'étiologie d'une forme de gangrène ou spontané d'un microorganisme spécial à cette affection. Les chirurgiens et les bactériologistes jusqu'à présent ne se sont occupés que des microorganismes de la gangrène gazeuse consécutive aux opérations chirurgicales. Rosenbach a observé quatre cas d'enfisémie gangréneuse et a obtenu dans deux cas des streptococci et dans les autres de longs bacilles, qu'il n'a pas réussi à cultiver malgré des tentatives.

J'ai eu la bonne fortune dans un vieillard de quatre-vingts ans, à l'hôpital des Incurables de Naples, affecté de gangrène sénile typique étendue à toute la partie inférieure de la jambe gauche de trouver dans la sanie ichoreuse un bacille, qu'isolé par les cultures fractionnées et inoculées, à la dose d'une goutte, dans les animaux inférieurs, donne naissance à une plaque de gangrène momifiante, qui bientôt devient foudrouyante à la périférie. Si l'on injecte dans les lapins ou dans les cochons d'Inde dix gouttes il y a alors une gangrène rapide étendue du dos à l'abdomen si l'injection a été faite dans le connective subcutané du dos. La maladie est transmisible d'un animal à l'autre.

Pendant la vie du malade j'ai obtenu trois fois les cultures du sang du doigt et après la mort j'ai obtenu des cultures pures du sang du cœur.

L'examen immédiat du sang m'a fait voir un certain nombre de bacilles; les cultures et les expériments d'infection furent positives.

L'examen de la peau démontra un grand nombre de bacilles dans les vaisseaux connectivals, tous de la même grandeur et de la même longueur, isolés ou réunis en chaînes plus ou moins longues. Les bacilles suivent les lymphatiques interfasciculaires du derme, du connectif subcutané et des muscles. Les tissus affectés se présentent infiltrés par de nombreux éléments ronds, les capillaires, les veines et les artères d'un calibre moyen présentent une endoflébite et une endoartérite oblitréante, mais les gros vaisseaux sont tout à fait libres.

Les bacilles dans les mauvaises conditions de nutrition et de température présentent la formation de spores dans le corps ou dans une de ces extrémités, ils sont par cela presque semblables aux bacilles du charbon, mais ils sont un peu plus courts, moins longs et les spores meurent après deux mois de desséchement. On peut cultiver les bacilles dans toutes les substances de culture, dans la gélatine peptonisée, dans l'agar-agar, dans le serum du sang, sur les pommes de terre, etc. J'ai employé pour la coloration des bacilles les couleurs d'angline et pour les sections microscopiques la méthode de Gram ou la double coloration ou carmin et à la méthode de Gram.

Messieurs, l'avoir donc trouvé dans un cas de gangrène spontané un bacille, qui cultivé et inoculé dans les animaux inférieurs, reproduit les mêmes lésions, que j'ai rencontrées dans le cadavre, rend probable qu'il y a une forme de gangrène momifiante spécifique liée à la présence du microorganisme que j'ai brièvement décrit.

The following paper was read :—

CALCULUS IN SYRIA AND PALESTINE.

CALCULUS IN SYRIE ET EN PALESTINE.

CALCULUS IN SYRIEN UND PALÄSTINA.

BY GEORGE E. POST, M. A., M. D.,

Professor of Surgery in Syrian Protestant College, Beirût, Surgeon to Johanniter Hospital.

Syria and Palestine are traversed, from the latitude of Mt. Taurus to that of Mt. Sinai, by two parallel mountain ranges, running nearly due north and south. The northern Syria mountains are volcanic, and in places the igneous rocks are overlaid with limestone, as at Kessab, in the chain of Cassius, and Ahberdagh at the northern limit of Syria. The chains of the Nusairy Mountains, Lebanon and Anti-Lebanon, and the hill country of Galilee, Samaria, Judea and the Tih, are composed of limestone, with occasional outcroppings of azoic ferruginous sandstone. The ranges of hills stretching from Aleppo to Damascus are volcanic, as is the mountain system of Haurân, the ancient Bashan. The mountains of Gilead and Moab are limestone, and those of Edom lime underlaid in many places by sandstone, which sometimes forms distinct mountain masses. South and west of the Tih the sandstone forms continuous chains, which gives place in central and southern Sinai to porphyry, and diorite and other igneous rocks.

The drinking water varies in different places. In Jerusalem, and many other cities of Palestine, it is rain water, stored in underground cisterns or open tanks. Along some of the streams the running water constitutes the only supply. Damascus, Aleppo and many other cities are supplied by open aqueducts, fed by rivers or mountain torrents. In Beirût alone is the water supply brought in iron pipes, and properly guarded from impurities in the reservoirs. This water, which is brought from the Dog river, six miles away, contains only a trace of organic matter, and a very small percentage of mineral constituents. The well water, which is still used in many towns, is much contaminated by organic impurities, and is a potent means of spreading disease. The

surface drainage generally pours by visible channels into the well. The villages in Lebanon and the other mountain chains are generally supplied by fountains, with water more or less charged with mineral substances, but usually quite free from organic impurities.

Stone is a common disease in almost every part of the country, but prevails much more in some cities and villages than in others. Tagharta, a village of perhaps 2000 souls, at the foot of Lebanon, a few miles from Tripoli, furnishes a large contingent of stone cases. Four cases of children came from this village to the writer in a single day.

The proportion of large stones is relatively much greater in these lands than in Europe or America, for several reasons: (1) the prejudice, especially among Moslems, against surgical operations; (2) the small supply of surgeons; (3) the want of skill of most of those who do exist, especially in the more secluded and inaccessible country districts, and the interior towns; (4) the ignorance of the use of sounds as a means of diagnosis; (5) the poor roads and want of suitable transportation, to enable patients to reach those centres where skilled surgeons can be consulted; (6) the poverty and ignorance of the people, leading them to procrastinate and neglect the treatment of the sick; (7) a traditional faith in the solvent power of lithontriptics.

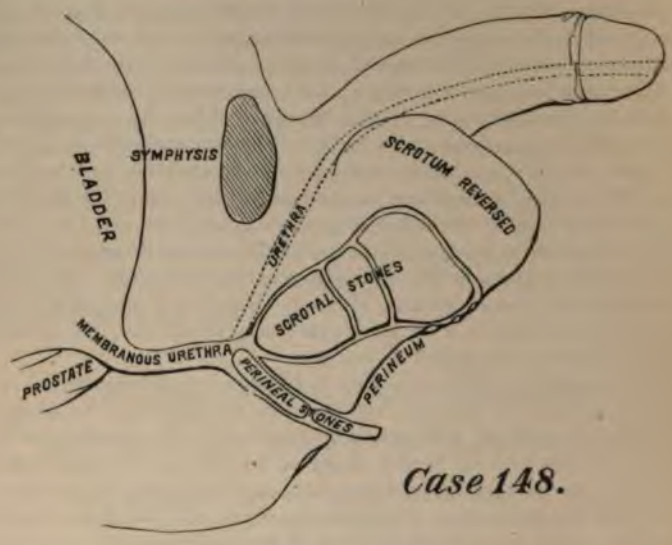
The diagnosis of stone is usually made by the dysuria, pyuria, and pain in the penis, and when the stone is large, by the digital examination per rectum. The old school of native stone-cutters have no sounds, either for purposes of diagnosis or operation. Patients often fancy that their disease is gonorrhœa, or stricture, and are surprised at the sharp click of the stone against the searcher.

The native practice is confined to median lithotomy, and is performed as follows: The patient is laid on his back, the legs flexed on the thighs and the thighs upon the trunk, and held by assistants, while others hold the body. The operator now introduces the forefinger, or the fore and middle finger of the left hand into the rectum, hooks down the stone and presses it into the perineum, and then plunging his knife, usually a razor, down to the stone where it bulges anteriorly in the perineum, cuts on it backward along the raphé, through the perineum, and the sphincter ani, and often laying open an inch of the anterior wall of the gut. The stone, if not too large, passes out of the free incision thus made. There is, of course, a considerable number of deaths from hemorrhage and septic poisoning, yet stone-cutters of long practice carry around with them a bag filled with stones which they have themselves extracted, often reinforced by specimens inherited from a stone-cutting ancestry. In those cases which escape death, there are many well-nigh incurable perineal and recto-urethral fistulæ left. The following typical cases will illustrate this class of malpractice.

Case I.—The patient, an adult, had been operated on in infancy for stone, and, on presenting himself, complained of difficulty in passing his urine, and of a hard swelling, as large as a hen's egg, in the perineum, extending from the anterior aspect of the sphincter ani well forward into the scrotal region. On passing a sound it at once encountered a large calculus, filling the membranous portion of the urethra and passing back into the bladder. A staff was now introduced into the bladder, and a median incision two inches long made. On removing the staff sixty-four cuticular calculous bodies were removed, varying in size from a grape seed to a nux vomica. The nucleus of each of these small calculi was a grape or lentil seed. They fitted into concave facets in the large stone, and in one another. The large stone weighed 68 grams and was as large as a hen's egg, but nearly of the shape of a Prince Rupert's drop, without the long tapering end. The obtuse end of this mass was covered with facets, and projected into the perineum, while the acute end projected backward into the bladder. It would seem that a fistula between the rectum and the urethra followed the first operation, and that the grape seeds, which are always swallowed by orientals, and a small number of lentil seeds, insinuated themselves into the fistulous track, and became the nucleus, first

of the larger calculus, and then of the smaller ones which continued to form from time to time.

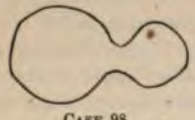
CASE II.—A young man of seventeen was operated on at the age of five years, for a stone. At the time when he presented himself there was at the posterior aspect of the scrotum, in the median line, and pointing to the left side, a hard tumor about the size of a hen's egg, which imparted to the touch the sensation of stones grating against each other. A series of fistulous openings communicated with the sac containing the calculi.



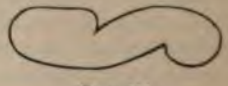
Case 148.

At a point three-fourths of an inch in front of the anus was another fistulous opening, as large as a goose quill, extending obliquely forward and upward toward the membranous portion of the urethra. From this opening the young man had, from time to time, extracted small cylindrical calculi, one-fifth of an inch in diameter, and from one-half of an inch to one inch in length. One had passed a few days before the examination of the case.

The case was operated on by connecting the fistulous openings by a single incision, dissecting out the adventitious membrane enclosing the stones, and dressing with



CASE 98.



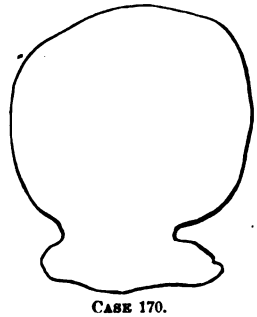
CASE 114.

compressed sponge and liquor ferri perchloridi to arrest hemorrhage, and subsequently with carbolic dressings. On the twenty-sixth day most of the urine was passed per urethram. But, as is too often the case, the patient passed from the care of the writer, who learned that through neglect another stone was allowed to form in the perineum. The annexed diagram (Case 148) will illustrate the size and relative position of these calculi.

The long duration of calculi in Syria often causes them to assume unusual forms,

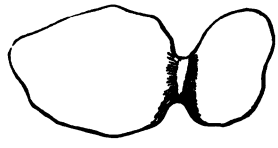
by the anterior portion of the stone becoming encysted in the prostate, and the posterior remaining in the bladder, but connected with the anterior by a neck more or less constricted. The following are some of the unusual forms, drawn the size of nature:

CASE 121.



CASE 170.

In case 170 the stone, which weighed 173 grams, was removed by the lateral operation. The neck of the stone was closely embraced by the neck of the bladder, which had to be nicked all around in order to free it. Notwithstanding the nicking, the base of the bladder was so torn that the peritoneum bulged into the wound, but was not injured. There was free hemorrhage at the time of the operation, requiring the ligature of several arteries. After the operation a pillow was placed under the pelvis, and a tube half an inch in calibre introduced into the bladder. The patient did not have an untoward symptom after the operation, and regained perfect control of the sphincter.



CASE 244.

In this case the larger segment of the stone was lodged in the prostate, and the smaller in the bladder.

The largest stone removed by the writer weighed 350 grams, and was extracted by a combination of the bilateral and lateral operations, and the prostate was incised in the axis of both lobes and in the median line. There was comparatively little hemorrhage at the time of the operation, and none subsequently, and the patient rallied well. He lived for nearly two months, and ultimately died of exhaustion from diarrhœa, brought on by imprudence in eating, after all the direct dangers of the operation had passed away. The wound had been contracting well until the diarrhœa set in. It is the writer's conviction that this stone could have been safely removed by the high operation with the modern improvements.

The greatest number of stones in any case was about two hundred, many of which, however, were merely large granules. In one case there were eleven, nearly of equal size, and weighing in the aggregate 153 grams. The patient died from intercurrent double pneumonia, three days after the operation. The calculi were wedge-shaped, with plane facets on all the sides except those toward the bladder, which were convex.

Sections of a large number of the stones removed by lithotomy show that at different stages of growth the stone has had a different chemical constitution. There are a

number with an oxalate nucleus, then uric acid and urates, and finally an external stratum of phosphates. In others the order of these various deposits is changed. Somewhat less than half of the whole number of stones are phosphatic. A little over one-third are uric acid and urates. Of the remainder the most are oxalate of lime, a few carbonate of lime, and one xanthic oxide.

The foregoing statements are based on a series of two hundred and fifty stones, treated by the writer. Of these cases ninety-seven were reported in July, 1877, in number 351 of the *New York Medical Record*, and fifty-one more in number 521 of the same periodical, in October, 1880. The remainder, one hundred and two, are recorded on pages 612–616 of this volume.

From these tables it will appear that there were 176 cases of perineal lithotomy, with a total mortality of ten, or *one in seventeen and a half*. Of these 176 cases 106 were in children of ten years or under. In these 106 cases the mortality was two, one from erysipelas, and the other from shock, the calculous mass being nearly half an ounce in weight in a child less than two years old. The total was *less than one in fifty cases*. In three patients the age was less than two years, in two one and a half. Of the remaining seventy cases, between the ages of eleven and seventy-five, eight died, or *about one in nine*. But four of these eight were in patients between sixty-five and seventy-five, and the remaining four at 13, 24, 35, and 45 respectively, the first and third being of extraordinary size, and the third died of intercurrent diarrhœa after seven weeks. Of the forty-four cases of lithotrity four were fatal. They would all without doubt have proved so with lithotomy. Of the 250 stones eleven, or about one in twenty-three, were in females.

DISCUSSION.

Dr. J. A. S. Grant (Bey) said :—

In many respects there is a similarity between Syria and Egypt. There is some aversion to consulting doctors or to having operations performed, so that in the case of calculus, when the surgeon is applied to he generally finds that it is a large stone that has been six or more years in forming. In Egypt we do not have the sect of stone-cutters, referred to by Dr. Post, as still plying their art in Syria. We are, I think, a little more advanced in Egypt, where each of the fourteen provinces is supplied with a chief doctor and a staff of doctors, midwives and barbers under him. Each province is also provided with a Government hospital. Within the last few years the Arabs have been more willing to consult the regular physician, and in this way calculus cases have come forward in time for lithotrity to be practiced on them. I have performed this operation myself, and have assisted other surgeons at the same operation, but in no cases have these operations been fatal to the patients. Stones in the urethra are also common in Egypt. I have sometimes been successful in extracting them by the urethra. At other times, however, I have had to cut down upon them, generally in front of the scrotum, and extract them from the opening thus made.

A few years ago an interesting case of scrotal calculus came under my care, and its history is as follows :—

An Arab railway employé, some six years before he consulted me, had a sudden stoppage of his urine, and this continued for ten days, so that he could only urinate drop by drop. At the end of that time he felt sudden relief and could then urinate freely ; but he felt a hard nodule in the perineum. This got larger and larger, until in six years' time it filled the scrotum and formed a fistulous opening in it, from which a portion of the urine escaped. The patient never consulted a doctor till one day, while waiting for a train to pass, some one threw from a carriage window a bottle,

that struck him on the scrotum and caused such an amount of inflammation as to render him unfit for work. He was, therefore, sent to me, and from the fistulous opening in the scrotum I could see that the swollen and tense condition of the scrotum arose from the presence of a large stone. I made a longitudinal incision through the fistulous aperture and scooped out the stone, not, however, entire, but in sufficiently large masses to see its structure. When all the pieces were put together they weighed fourteen ounces. In the centre were three or four-faceted stones as large as the kernels of chestnuts, then surrounding these were thin layers, converting the whole into the form of a cricket ball. This patient was nearly perfectly well when I left Cairo in August last, and very little urine escaped from the fistulous opening in the scrotum.

Now I should like to ask Dr. Post what is the cause of the frequency of calculus in Syria.

In Egypt, Dr. Zaucarol, of Alexandria, has made a special study of this subject, and he considers that the eggs of Bilharzia are often found forming the nuclei of the calculi, and as Bilharzia is a very common disease among the inhabitants of Egypt, he considers that the eggs of this microbe favor the formation of calculi when the diathesis is present.

The usual way of operating in Egypt is the same as that described by Dr. Post as being practiced by surgeons in Syria, only I believe suprapubic operation has been done in several cases in the Arab Hospital at Qasr el Ainee.

I. SIXTY-TWO VESICAL STONES ,

No. in Case Book.	Age.	Residence.	Date of Operation.	Duration of Symptoms.	Operation.	No. of Stones.	Weight in Grammes.	[illegible]	Results and Remarks.
149	60	Damascus	Sept. 5, 1880	4 years	Lateral	1	96		Cas[illegible] d with chronic bronchitis and fatty heart. There [illegible] oozing from the neck of the bladder at the time of [illegible] hich was controlled by liquor ferri perchloridi. [illegible] after the operation, but was checked by treatment. [illegible] t left the hospital Oct. 29, with a fistulous opening yet
152	3¼	Beirût	Dec. 2, 1880	6 months	Lateral	1	1.50	7 days	[illegible] the stone, 3¾ X 3 X 1½ inches, made it difficult [illegible] it. The prostate was incised to its full extent [illegible] s, and the capsule lacerated in extraction. The [illegible] ght, when the extent of the wound is considered, [illegible] lied well. A drainage tube was introduced. The [illegible] o contract well for a month, when the patient [illegible] atercurrent diarrhœa, from imprudence in diet, [illegible] nd of seven weeks, from the exhaustion of the [illegible] were no unfavorable vesical or renal symptoms [illegible] e operation.
154	35	Margab	May 18, 1881	Uncertain	Lateral	1	350		
155	47	Jaffa	May 25, 1881	6 years	Lateral	1	67		[illegible] he time of the operation, arrested by liquor ferri perchloridi. Recovery.
156	6	Beirût	May 28, 1881	1 year	Posterior	1		4 days	Recovery.
157	4	Hamath	July 1, 1881	1 year	Posterior	1	2	6–8 days	Recovery.
158	4	B'libba	July 5, 1881	4 months	Posterior	1	4.50	5 days	Recovery. The father says that the child passed two stones a year before the time patient presented himself, and remained free from vesical symptoms until within four months.
159	18	Rotheya	July 12, 1881	1 month	Posterior	1	3.70		Recovery. Patient had an operation, in infancy, for a large stone by a native stone-cutter. Has not had symptoms of the new stone for more than a month.
161	24	Kafroon	Sept. 7, 1881	From infancy	Lateral	1	82.5		Died of shock, 24 hours after the operation.
162	45	Hammâna	Nov. 4, 1881	2 years	Posterior	1	41		Died the following night, from exhaustion by hemorrhage from the neck of the bladder.
164	3	Beirût	Jan. 27, 1882	2 weeks	Posterior	1	0.67	1 day	Recovery. In this case, the incision into the neck of the bladder was made large enough to allow of the introduction of a dressing forceps, with which the stone was extracted.
167	13	Hamath	April 14, 1882	From infancy	Posterior	3	73.5 The stones weighed, respectively, 30.5, 22 and 21 grammes		Death. The nausea and vomiting continued from the date of the operation, and appeared to be the cause of death. There was no tympanites, and no abdominal tenderness except over the stomach.
169	14	Sidon	May 12, 1882	2 years	Posterior	1	2	3–24 days	Recovery.

No. in Case Book.	Age.	Residence.	Date of Operation.	Duration of Symptoms.	Operation.	No. of Stones.	Weight in Grammes.	Time of Return of Urine by Urethra.	Results and Remarks.
170	30	Der'ûn	June 3, 1882	2 years	Lateral	1	173	29 days	The details of the operation given in the foregoing paper.
171	19	Mardin	June 28, 1882	4 years	Italian	1	12.5	4–12 days	Recovery. In this case, hemorrhage from the neck of the bladder occurred at the time of the operation. A tube was introduced, and the wound packed with sponge moistened with liquor ferri perchloridi. The sponges and tube were removed the third day.
172	3	Beirût	July 14, 1882	1 year	Posterior	1	1	From time of operation	Recovery.
173	5	Adana	July 15, 1882	1 year	Posterior	1	5.5	4–10 days	Recovery.
176	13	Lattakia	Sept. 16, 1882	3 years	Posterior	1	23	2–7 days	Recovery. The patient was a morphine taker, and had reached a minimum of one grain a day at time of operation.
177	33	Amyûn	Sept. 27, 1882	3 years	Lateral	1	40	20 days	Recovery. Hemorrhage, at time of operation, arrested by sponges and perchloride of iron, which were removed 30 hours after operation. On the fourth day there was slight secondary hemorrhage. On the eighth, diarrhœa, which was arrested by treatment on the tenth.
178	3	'Aba	Sept. 29, 1882	1 year	Lateral	1			Recovery.
179	4	Damascus	Oct. 17, 1882	2 years	Lateral	1	1		Recovery.
181	3	Beirût	Jan. 6, 1883	5 months	Italian	1	1	10–14 days	Recovery.
182	7	Deir Mimas	March 24, 1883	3 years	Posterior	1	6	11–13 days	Recovery.
185	4	Damascus	April 10, 1883	1 year	Italian	1		1–10 days	Recovery.
187	6	Damascus	May 10, 1883	1 year	Italian	3	5.5 The weight of the stones was, respectively, 2, 2, 1.5 grammes.	6 days	Recovery. Patient had been operated on a year before by a native, and one stone removed. The parents claim that symptoms of stone existed from the time of the healing of the first wound. A sound could not be passed, owing to the contracted state of the cicatrix of the first wound. An incision was made in the median line and the urethra found, a probe insinuated into the bladder, and then a grooved director, and on it the bladder was incised.
188	4	El Huleh	June 8, 1883	2 months	Italian	1	7	From time of operation	Recovery.
189	3	Barj-el-Berajeni	June 15, 1883	1½ months	Italian	1	0.4	1 day	Recovery.
190	4	Beirût	June 14, 1883	2 months	Posterior	1	2.4	1 day	Recovery.
191	23	Antelias	June 18, 1883	20 years	Posterior	2	12	Time of operation	Recovery. Passed a small stone at the age of 3 years. Has never been free from symptoms of calculus since. Has been married a year.
192	3	Barj-el-Berajeni	June 19, 1883	2 months	Italian	1	0.15	2–6 days	Recovery.
194	1½	Safed	Sept. 15, 1883	5 months	Italian	1	4.6	8–10 days	Recovery.
196	3	En-Nebk	Oct. 9, 1883	6 months	Italian	1	2		Recovery.
198	13	Esh-Shukif	Oct. 26, 1883	5 years	Posterior	1	27	7–14 days	Recovery.
200	6	Hums	Nov. 29, 1883	7 months	Italian	1		4 days	Recovery.
201	1 1/12	Damascus	Dec. 13, 1883	Uncertain	Italian	2	13.5		Death from shock. The stones were so tightly embraced by the bladder, which contained little urine. The laceration at the base of the bladder may have allowed of some infiltration into the pelvic cellular tissue.

No. in Case Book.	Age.	Residence.	Date of Operation.	Duration of Symptoms.	Operation.	No. of Stones.	Weight in Grammes.	Time of Return of Urine by Urethra.	Results and Remarks.
203	4	Beirût	Jan., 1884	1 year	Italian	1	0.54	7 days	Recovery.
204	56	Mecca	Feb. 26, 1884	2 years	Lateral	1	72.5		Recovery. Notwithstanding the large size of the stone, lithola-paxy was attempted, but when the diameter of the stone had been reduced from 1½ to 1 inch, by the scaling off of its cortical substance, the female blade of the lithotrite broke in the attempt to crush the hard nucleus. The operation was completed by a lateral lithotomy, through which the nucleus and the beak of the lithotrite, as well as the remaining fragments, were extracted The patient, after numerous mishaps and serious symptoms, recovered from the double operation, with the drawback of a capillary fistula.
205	10	Adana	April 5, 1884	3 years	Posterior	1	12.5	5 days	Recovery.
206	3½	B'tughrîn	April 12, 1884	6 months	Posterior	1	1.5	2 days	Recovery.
208	7	Batrûn	April 15, 1884	1 year	Posterior	1	3		Recovery.
212	6	Sidon	July 29, 1884	14 months	Italian	2	5	8 days	Recovery.
213	7	Beirût	July 29, 1884	4 years	Lateral	1	8	8 days	Recovery.
215	4	Shikkah	Oct. 16, 1884	7 months	Italian	1	5	4–8 days	Recovery.
216	18	Hums	Oct. 28, 1884	5 years	Posterior	1	47	10–18 days	Recovery.
217	9	Sidon	Nov. 26, 1884	From infancy	Posterior	1	36	17 days	Recovery.
218	4	Abkar	Dec. 16, 1884	Not noted	Posterior	1	12.5	8 days	Recovery.
220	3	Beirût	April 28, 1885	Not noted	Italian	1	2		Recovery.
221	5	Kisba	May 9, 1885	Not noted	Italian	1	14		Recovery. An attempt had been made by a native to remove this stone by median lithotomy, but he had only scaled off some of the cortical substance, and had left a perineal fistula, through which a director was introduced, and used as a staff in making the incision.
227	1½	Beirût	Nov. 5, 1885	Infancy	Italian	1	2	10 days	Recovery.
228	4	Sin el Fîl	Nov. 27, 1885	3 years	Italian	1		10 days	Recovery.
231	3	Hasrûn	Dec. 10, 1885	8 months	Italian	1	0.2	12 days	Recovery.
232	4	Lonaiz	Dec. 14. 1885	17 months	Posterior	1	4	10 days	Recovery.
235	4	Beirût	April 9, 1886	Infancy	Posterior	1	5.3	12 days	Recovery.
239	2½	Aleih	June 30, 1886	10 months	Posterior	1	3	3 days	Recovery.
242	18	Beirût	July 13, 1886	8 years	Italian	1	68	3 months	Recovery.
243	3	Beirût	Aug. 3, 1886	6 months	Italian	1	4	7 days	Recovery.
244	15	Hamath	Aug. 28, 1886	Childhood	Italian	1	41	14 days	Recovery. The stone is described and figured above.
245	4	Zebedani	Sept. 3, 1886	1 month	Posterior	1	0.3	10 days	Recovery.
247	6	Beit-ed-dîn	Oct. 1, 1886	Uncertain	Italian	1	10	12 days	Recovery.
249	4	Adana	Nov. 26, 1886	6 months	Italian	1	3	7–12 days	Recovery.
250	8	Adana	Nov. 26, 1886	2 years	Posterior	1	11	7–12 days	Recovery.
253	7	Beirût	March 16, 1887	Uncertain	Posterior	1	14	12 days	Recovery.

THIRTY-FIVE CASES OF LITHOTRITY IN THE MALE.

No. in Case Book.	Age.	Residence.	Date of Operation.	Duration of Stone.	Kind of Operation.	Weight in Grammes.	Results and Remarks.
150	55	Beirût	Oct. 29, 1880	2 years	Litholapaxy	4.5	Recovery. No recurrence.
151	60	Akka	Nov. 1, 1880	1 year	"	7	Recovery. Recurred—see 175.
153	38	Beirût	March 17, 1881	1 year	"	12	Recovery. No recurrence. Two sessions were held in this case.
160	67	Damascus	July 12, 1881	2 years	"		Recovery. Recurred—see 165.
163	17	'Arzûz	Dec. 19, 1881	3 years	"		Recovery. No recurrence.
165	68	Beirût	Jan. 28, 1882	6 months	"		Recovery. Recurrent from 160. No subsequent recurrence.
168	65	Mezra'at	May 6, 1882	2 months	"		Recovery. Recurrent—see 202.
174	60	Mersina	Aug. 4, 1882	7 months	"	50	Death, thirty-two hours after the operation, with suppression of urine. It was a bad case, with tenderness over the kidneys, and fever, for which the operation was undertaken as a forlorn hope.
175	61	Akka	Aug. 15, 1882	21 months	"	4	Recovery. Recurrent from 151.
180	65	Zuk	Nov. 7, 1882	3 months	"	16	Recovery. Recurred—see 181.
181	66	Zuk	March 27, 1883	4 months	"	2	Recovery.
184	25	Deir Mimas	April 6, 1883	8 months	"	7	Recovery.
186	75	Beirût	April 25, 1883	8 months	"	11	Recovery. This man had passed 150 kidney stones per urethram. The stones in the bladder were numerous; some were crushed, others drawn out whole, in the jaws of the lithotrite.
193	55	Akkar	July 31, 1883	1 year	"	28	Recovery. Recurred—see 209.
197	53	Hums	Oct. 25, 1883	1 year	"	10	Recovery.
199	25	Zagharta	Nov. 27, 1883	Infancy	"	19	Recovery.
202	66	Mezra'at	Dec. 15, 1883	1 month?	"	12	Recovery. No recurrence.
207	80	Zuk	April 15, 1884	2 years	"	16	Recovery. Recurred—see 234.
209	54	Hums	May 15, 1884	3 months	"	2	Death, twenty hours after operation, from suppression of urine.
210	55	Mezra'at	May 31, 1884	6 months	"	5	Recovery. Recurred—see 214.
211	42	Batrûn	July 5, 1884	2 years	"	2	Recovery.
214	55	Mezra'at	Oct. 14, 1884	4½ months	"	15	Recovery. No recurrence.
219	50	Cyprus	March 4, 1885	3 years	"	36	Death. The symptoms at the first session were such that the crushing was not completed. Four days later a second session was held, but could not be carried to completion. On the sixteenth day he died of exhaustion. A fragment, as large as a hickory nut, remained in the bladder.
229	50	Beirût	May 29, 1885	1 month	"	0.5	Recovery. Has a history of the passage of kidney stones. No recurrence.
230	50	Alexandria	Nov. 5, 1885	1 year	"	1.5	Recovery. Has a history of the passage of kidney stones. No recurrence.
233	56	Beirût	Jan. 7, 1886	2 years	"	17	Recovery. In this case two sessions were held. No recurrence.
234	82	Zuk	March 12, 1886	1 year	"	3	Recovery. Recurrent from case 207. No recurrence.
236	35	Haurân	May 19, 1886		"	46	Recovery. The patient had suffered from constant stillicidium before operation. Twelve days after it, he left the hospital cured of the stone and the stillicidium. No recurrence.
237	50	El-Husu	June 14, 1886	7 years	"	42	Recovery. No recurrence.
240	30	El-Husu	July 2, 1886	1 year	"	55	Recovery. No recurrence.
246	75	Beirût	Sept. 9, 1886	8 months	"	10	Recovery. No recurrence.
248	70	Hasbeyah	Oct., 1886	2 years	"	18.5	Recovery. The patient had been cut by myself, 13 years before, by bilateral operation, and a stone removed. No recurrence.
251	65	Beirût	Jan. 3, 1887		"	23	Death, on the seventh day, of uræmic poisoning.
252	50	Beirût	Feb. 23, 1887	4 months	"	5	Recovery.
255	70	Beirût	April 4, 1887		"	26.5	Recovery.

TWO VESICAL STONES IN THE FEMALE.

No. in Case Book.	Age.	Residence.	Date of Operation.	Duration of Symptoms.	Operation.	Weight in Grammes.	Results and Remarks.
195	3	Beirût	Oct. 5, 1883	4 months	Lithotrity	2	Recovery. No loss of power over the neck of bladder.
257	11	Beirût	April 12, 1887	2 years	Lithectasy	2	Recovery. Two years ago she had the symptoms of a kidney stone, and since that time has suffered from vesical irritation. On probing, I found a stone in the right ureter. I introduced a dressing forceps by dilating the urethra, and succeeded in grasping the stone. It broke in extraction. It was 1½ inches in length and ¼ of an inch in diameter, and its form an accurate cast of the calibre of the ureter at its meatus. The patient had perfect control over the bladder from the time of the operation.

THREE URETHRAL STONES IN THE MALE.

No. in Case Book.	Age.	Residence.	Date.	History.
241	14	Togharta	July 9, 1886	The stone was removed by median incision of the prostates, in the canal of which it lay. It was as large as a date stone.
254	70	Akkar	March 17, 1887	The patient had had phimosis, for which he had been circumcised previous to his application to the writer. The meatus was exceedingly small, and an inch from the orifice a stone as large as a pea could be felt by external palpation and with a probe. Five inches back was another stone a little larger than the first. The first was extracted by incising the meatus, and seizing it with Civiale's urethral forceps. The second was then extracted with the same forceps. The man, being of the opinion that two more stones existed in the posterior portion of the urethra, was sounded, but with negative results. Five days later, I dilated the urethra to No. 20 English, and introduced a duck-billed lithotrite into the bladder, and grasped and extracted, without crushing, a stone half an inch long, one-third of an inch broad, and one-fourth of an inch thick. No other stone was found.
256	2	Beirût	April 9, 1887	Two weeks before the child was presented to me for treatment, he had complete retention of urine. A native physician had tried in vain to introduce a catheter. I succeeded in doing so, and drew off over a quart of urine. There was an obstruction at the neck of the bladder, but no distinct sensation of a stone. For several days I repeated the operation, with the same result. I had resolved that I would propose, the next time the child was brought, to make a perineal section, and explore the urethra and, if necessary, the bladder. But, at the next clinic, I found a small stone at the meatus, and, by slightly nicking the aperture, easily extracted it.

(A¹). Bony Union of Intra-capsular Fracture of the neck of the Femur (sawed).
(Carnochan).

Ununited Fracture of the neck of the Femur.

(Carnochan).

The following remarks were made by Dr. Carnochan :—

BONY UNION OF INTRACAPSULAR FRACTURE OF THE NECK OF THE FEMUR.

UNION OSSEUSE DE LA FRACTURE INTRACAPSULIÈRE DU COU DE LA FEMUR.

KNÖCHENE VERHEILUNG EINES INTRACAPSULÄREN SCHENKELHALSBRUCHES.

BY DR. JOHN MURRAY CARNOCHAN,

Of New York City.

GENTLEMEN—Through the courtesy of the President, I have been granted a few minutes in which to show you what I believe to be a rare specimen of bony union of intracapsular fracture of the neck of the femur. I have here a united fracture, (showing the specimen to the audience) where the fracture was intracapsular, and if you will compare these two specimens (showing another where there was no union) you will have an opportunity of forming an opinion whether the fracture was intracapsular or not.

This specimen was taken from a lady whom I treated for intracapsular fracture and for whom I obtained a bony union. She was about seventy years of age when she met with the accident which caused the fracture. She was thrown off the steps of an omnibus, in New York, and fractured the neck of the femur. After she was carried to her home and the family physician arrived, I was sent for and we made a diagnosis of the case. There was no doubt as to its nature or to the symptoms of intracapsular fracture being present. The question then arose as to the best mode of treatment. I followed the method which I had been in the habit of using for a number of years. This consisted chiefly in keeping the patient in a quiet position for a long time. She was kept in bed, more or less, for nine months. The fracture bed on which she was treated was made in such a way as to enable the excretions to be carried on without any disturbance to the general position of the hip joint. The patient had all the means necessary to carry out everything that would tend to the improvement of her general health. (I think I had better take the cord off so that you can remove the segments one from the other.)

This specimen was produced, in a suit at law for malpractice, in Brooklyn. A lady who had a fracture of the neck of the femur had very little use of the limb two years after the accident. She sued the medical man who attended her for a large amount of damages. The suit came on for trial. Mr. Benjamin F. Butler and Mr. Roger A. Pryor were the two prominent counsel for the plaintiff, and a distinguished lawyer, Mr. F. N. Bangs, was the counsel for the defendant. The damages claimed were $50,000, so that it was a case of some celebrity. The late lamented Dr. Frank Hamilton was on one side and I on the other. The point claimed by the defence was that it was impossible for the lady to have any better use of her limb than she had; that, from the seat of the fracture, it was impossible for union to take place. I was not very anxious to appear in the case. In fact, I refused to do so, but was over-persuaded. It so happened that I had this specimen and it was brought before the court to prove that the fracture of the neck of the femur would admit of bony union. I will pass it to you and you can judge for yourselves. (Here the specimens were passed around among the audience.) I have also drawings of the specimens (exhibiting them) made while the parts were still fresh. You will see that the line

of the fracture runs within the capsule in this direction (indicating the line); one is a drawing of the anterior aspect of the neck and the other of the posterior. (Vide Plate.) There is the line of fracture on the anterior side of the bone and here it is on the posterior. I will put the fracture that had not been united opposite. (Exhibiting another specimen.) This case, in which there was no union, I also attended, and the injury which caused the fracture was so severe that the patient died on the twenty-first day. You will observe that there is no union between the fragments. The case in which there was bony union was treated, as I have already stated, for nine months and, to my astonishment, I found the limb was so consolidated that the patient could walk well except experiencing some little difficulty from the shortness of the limb. She had about half an inch of extra heel added to her boot, on the injured side, and she walked with scarcely a halt. I may mention that the reason the bone is sawed in this manner is that Dr. Hamilton would not admit that bony union could take place in such cases, so I procured a fine saw and requested him to saw the bone vertically, which he did, and candidly admitted, in open court, that there was bony union.

This patient survived her accident, with fairly good health, for seven years and died of an internal disease.

The following paper was read :—

UNUNITED FRACTURE OF THE FEMUR SUCCESSFULLY TREATED BY DOUBLE SPLICE AND WIRED CLAMP.

FRACTURE DESUNÈE DE FEMUR TRAITE SUCCÉSSIVEMENT AVEC DOUBLE SPLICE ET DE CLAMP FILLÉ.

ERFOLGREICHE BEHANDLUNG DER PSEUDARTHROSE DES FEMUR MITTELS DOPPELTER FUGE, EINSPLISSUNG UND DRATHKLAMMER-VERNIETUNG.

BY DR. F. LEMOYNE,

Of Pittsburgh, Pa.

There can be no doubt that the cause of the vast majority of ununited fractures is the failure to procure and maintain proper coaptation of the fragments. That is usually due to the interposition of adjacent tissues, the peculiar locality of the fracture, rendering rest of the parts almost impossible, or the imperfect use of surgical appliances.

The method of treatment advocated in this essay, has been successful in three desperate cases. The first was by the author, upon the humerus (published); the second, the subject of this paper; and the third by Dr. J. McC., upon the humerus. Each of the cases had been subjected repeatedly to the ordinary operations, such as boring, drilling and wiring.

June 30th, 1883, J. K., aged 37, while working on a vessel in Chicago, was struck by a bucket of iron ore, weighing about 2500 pounds, fracturing his left femur, about the middle of its length.

The following is from the records of Mercy Hospital, Chicago, where he was first treated, in regard to his case :—

"In reduction, crepitus was obscure, leading to fear of interposed muscle; used adhesive plaster extension, with weight and pulley and coaptation splints.

"July 20th—Fragments do not seem uniting.

"August 30th—No union.

"September 1st—Etherized; cut down; found muscle between fragments, and drilled fragments.

"September 25th—No union; cut down again; sawed off ends of bone, and wired them together.

"October 27th—Removed wires.

"February 5th, 1884—No union."

About October 1st, 1884, this case having been eight months under observation, with the limb retained in an immovable splint, there was found to be no improvement. The man was in fair general health. The disabled limb was three inches shorter than the sound one. The thigh was deeply scarred anteriorly, and slightly so externally, from the effects of former operations. The knee joint was partially anchylosed. The upper fragment of the femur seemed to be carried forward, and the upper extremity of the lower fragment could be felt lapping the upper fragment about three-fourths of an inch, postero-externally. Mobility at the seat of fracture was very distinct, and the effort to rest the weight of the body upon the injured limb, produced a marked bowing of the thigh, convex-externally, with failure to sustain the weight.

On November 27th, 1884, the seat of injury was exposed by two free incisions, one on the anterior and the other on the external aspect of the thigh. The fragments of the femur were found lapped and attached by firm bands of fibrinous material, but no articular surface was discovered.

The extremities of both fragments were rounded, and the bone tissue was dark colored and soft enough to be easily penetrated by the point of an elevator.

The fragments were completely separated, and the intervening tissue cut away. In order to avoid additional shortening of the limb, a little risk was taken in the vitality of the bone preserved. The superior fragment was carried through the anterior incision. The periosteum was slit and turned back, as well as its degenerated character would admit. Two lateral slices were sawn from its extremity, in such manner as to form a wedge, of which the triangular surfaces presented forward and backward. (Figure 3, b.) The lower extremity was then brought through the external incision, prepared in a similar manner, and a wedge-shaped fragment removed from the end of it, leaving a "V"-shaped incision for the wedge of the upper fragment to fit into. (Figure 3, a.) By persistent extension, and the use of considerable force, the extremities were now brought into apposition. Holes were drilled two and one-half inches apart through the anterior surface of the shaft of the femur, one above and the other below the surfaces thus brought in contact. (Figure 3, c, d.) A steel clamp of proper size and length, bent at a right angle at each extremity (Figure 4) was then inserted into the holes, and wires passed around the entire circumference of the femur, near both ends of the clamp, and twisted down, binding it firmly to the bone. (Figure 5, p. 620.) The wires were cut long and allowed to project from the wound, which was only partially closed. The limb was placed upon a posterior inclined plane, with the foot elevated about eight inches, as the fragments were found to be in the most accurate contact in that position. Sole leather splints were also applied internally from the sole of the foot to the perineum, anteriorly from ankle to groin, and externally from the sole of the foot to the crest of the ilium, and the patient was kept as nearly as possible at rest.

High inflammatory action followed, accompanied and succeeded by very free suppuration. Ample drainage was secured by the free external incision being kept open.

On the 5th of January, 1885, the patient was in very good general health, and the condition of the limb indicated partial union. With as little disturbance as possible, the wound was opened, and the clamp and wires removed. Several small, separated spiculæ of bone were extracted. The deposit of provisional callus was very slight and

Fig. 3.

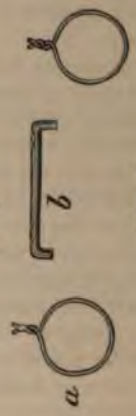


Fig. 4.

Fig. 5.

the union unsatisfactory. The leather splints were replaced, and an additional one substituted for the plane, which had gradually been brought to the level of the bed.

Tonics, consisting principally of preparations containing phosphates, were used freely; passive exercise and other measures calculated to stimulate the nutrition were practiced. Gradually the strength of the bone increased, until, one year from the date of the operation, the patient was able to bear his entire weight upon the injured limb, and assume, with ease and comfort, the position represented in the accompanying photograph.

The limb is shortened two and one-half inches, two inches of which is compensated for by the shoe.

A few days ago I received a letter from the subject of this operation, stating that he was able to walk several miles without the aid of crutch or cane.

The following paper was read:—

EXPERIMENTS ON THE REMOVAL OF SEQUESTRA FROM THE LIVING BODY, BY IRRIGATION WITH DILUTE ACID SOLVENTS.

EXPERIMENTS SUR L'ENLÈUEMENT DES SÉQUESTRES DU CORPS VIVANT PAR L'IRRIGATION AVEC DES DISSOLVANTS ACIDES ÉTENDUS.

EXPERIMENTE ÜBER DIE ENTFERNUNG VON SEQUESTREN AUS DEM LEBENDEN KÖRPER DURCH IRRIGATION MIT VERDÜNNTEN SÄUREN.

BY EDMUND ANDREWS, M.D., LL.D.,

Professor of Clinical Surgery in the Chicago Medical College, and Senior Surgeon of Mercy Hospital, Chicago.

When surgeons first learned the power of hydrochloric acid to dissolve the calcareous matter of bone, they at once conceived the hope of using it to remove the sequestrum of necrosis from the living cavity in which it lay. Some discussion arose, but many of the first experiments were so crude as to make a bad impression, and arouse opposition. Hence it happened, that a series of prominent surgeons forcibly denounced the project, and had influence enough to stop its development for more than a hundred years.

In 1775, W. Troja, in Paris, published experiments upon pigeons in this connection.

About the same time, Delpech, of Montpellier, in France, the preceptor of the celebrated military surgeon, Baron Larrey, of France, made some experiments upon man with diluted sulphuric acid, and claimed a degree of success. However, as sulphuric acid is about the worst solvent he could have selected, it is not surprising that his plan soon fell out of favor, though his pupil, Poujet, claimed to have succeeded several times.

Malgaigne discredited their reports, and probably with reason, for it is impossible to decalcify bone with dilute sulphuric acid, in any moderate length of time.

Dupuytren, of France, advocated brushing the diseased bone with strong sulphuric acid, probably with the idea of its alterant effect as a caustic on the living parts, rather than as a proper solvent of sequestra.

In 1825, Samuel Cooper, of England ("Cooper's Surg. Dictionary," Art. Necrosis), mentioned, with strong condemnation, that some injudicious surgeons used acids and other irritants upon dead bone.

In 1844, Stromeyer, of Germany, denounced the use of solvents in these words: "This removal of sequestra must take place through operative measures. Their destruction by the application of diluted mineral acids, as Delpech has done, is not justifiable. It can only be successful in superficial sequestra, and there it is unnecessary, because the operative removal presents no difficulty. In the case of deep sequestra it is not likely to succeed." ("Stromeyer, Handbuch der Chirurgie." Bd. I, S. 460.)

In 1853, Prof. James Miller, of Scotland, wrote as follows: "At one time, it was proposed to apply nitrous or other acids to the sequestrum, with a view to its becoming pliable through the loss of its earthy matter, and so capable of being gently pulled away at the cost of but little pain or blood. The impossibility of confining the acid's action to the part to be destroyed is a sufficient, because insuperable, objection to the practice." ("Miller's Prin. of Surgery," 4th American edition, p. 405.)

In 1866, Prof. Frank Hamilton, of New York, in answer to a question from Dr. Wm. Watson, of Dubuque, Iowa, responded substantially as follows: "The removal of sequestra by hydrochloric acid is impossible, because no solution strong enough to affect the bone can be tolerated by the soft parts."

In 1869, Prof. S. D. Gross, of Philadelphia, remarked, in respect to acid solvents for sequestra: "Experience has proved that they are always productive of harm from their irritating effects upon the new bone and the soft parts, while, unless they are intolerably strong, they can exert no destructive influence upon the sequestra itself." ("Gross' Syst. of Surg.," 5th edition, Vol. I, p. 871.)

In the same year, Billroth, of Vienna, uttered this vigorous condemnation of the plan: "Chemical solution of the sequestrum is not to be thought of. If you were daily to pour muriatic acid into the fistulous opening, it would affect the newly formed bone as much as or more than it would the sequestrum, which would be very unfortunate, as it must replace the latter." (Hackley's translation of "Billroth's Surg. Pathol.," p. 448.)

Still later, Mr. G. Pollock, of England, recommended to apply strong sulphuric acid to certain sequestra, in order to disintegrate the surface.

Mr. C. McNamara, of Westminster Hospital, London, commends the suggestion, but adds that great caution must be observed to prevent the acid from touching the living parts. Both writers seem ignorant of the fact that though strong sulphuric acid is a powerful caustic when applied to soft parts, it is exceedingly slow and feeble in its action upon dead bone.

The tone of the scanty literature of this subject is decidedly discouraging. However, I commenced occasional experiments upon it about fifteen years ago, but with so little hope of success that I preserved no records of my earlier efforts. However, I gradually learned the following facts:—

1. Suitable acid solutions decalcify dead bone inside the body exactly, and quite as readily, as they do in the laboratory.

2. Properly diluted solutions cause no pain, and have no perceptible effect on living bone or soft tissues. The alkaline fluids circulating in the haversian canals of the living bone and in the capillaries of the soft parts, prevent the weak solutions used from acting upon living tissues.

3. Hydrochloric acid is the most rapid decalcifier. The tissues usually tolerate, without pain, a solution containing from six to thirty per cent. of the *acidum hydrochloricum dilutum*, of the United States Pharmacopœia. The *acidum dilutum* contains ten per cent. of the absolute acid. A few patients tolerate the full strength of the *acidum dilutum* without pain.

4. Where complete and constant irrigation of the sequestrum can be had, it will be decalcified in a few days; but I proceeded at first with such extreme caution, that I unnecessarily prolonged the process.

5. After decalcification, the animal basis of the bone is rapidly absorbed, exactly like decalcified bone drainage tubes when left in wounds.

6. The acid solvent is a thorough antiseptic, and no other is required during its use.

RAPIDITY OF ACTION OF HYDROCHLORIC ACID.

To obtain an idea of the rate of action of different solutions of hydrochloric acid, of such strength as I found to be tolerated by patients, I tried the following twelve experiments outside the body, using broken fragments of actual sequestra, taken by operation from living patients.

TABLE SHOWING THE TIME REQUIRED FOR DECALCIFYING SEQUESTRA IN WEAK HYDROCHLORIC ACID SOLUTIONS OUTSIDE THE BODY.

Experiment.	Strength of Solution.	Dimensions of Sequestra.	Time required for Decalcification.
1	1-16 the strength of officinal dilute hydrochloric acid.	4½ cm. by 1 cm. by 3 mm.	142 hours.
2	1-10 do do do	3½ cm. by 14 mm. by 4 mm., very hard seq. from tibia.	120 "
3	1-10 do do do	4½ cm. by 28 mm. by 20 mm. Very spongy tuberculous trochanter.	24 "
4	⅛ do do do	9 cm. by 32 mm. by 5 mm.	117 "
5	1-12 do do do	38 mm. by 9 mm. by 4 mm.	120 "
6	⅛ do do do	2½ cm., very hard sequestr.	128 "
7	⅛ do do do	5½ cm. by 13 mm. by 1 cm.	102 "
8	⅛ do do do	8 cm. long, whole thickness of shaft of femur, diam. 2 cm.	96 "
9	¼ do do do	32 mm. by 13 mm. by 3 mm.	102 "
10	¼ do do do	64 mm. by 38 mm.	54 "
11	¼ do do do	Block of cancellated bone 16 mm. diameter.	78 "
12	¼ do do do	Sundry small splinters.	54 "

Average time of solution, about 95 hours.

COMPARATIVE EFFECT OF DIFFERENT ACIDS IN SOLUTIONS OF TOLERATED STRENGTH.

To ascertain the best material for the work, I tested ten acids in solutions of a strength easily tolerated by patients. The experiments were performed by suspending in the acid solutions small tablets of compact bone of uniform size, cut by a mechanic from a human tibia and femur. Each tablet was two centimeters long, one centimeter wide, and three millimeters thick. The results are embodied in the following table :—

TABLE SHOWING THE TIME REQUIRED BY DIFFERENT ACIDS OF TOLERATED STRENGTH FOR DECALCIFYING TABLETS OF COMPACT BONE TWO CENTIMETERS LONG, ONE CENTIMETER WIDE, AND THREE MILLIMETERS THICK.

Acid employed.	Strength of the solution.	No. of tablets tested.	Average time of decalcification.
Hydrochloric Acid.	16 per cent. of acid. dilutum of U. S. Pharmacopœia.	6	40 hours.
Nitric Acid.	do do do	6	96 "
Nitro-Mur'tic Acid.	do do do	2	78 "
Phosphoric Acid.	Full strength of acid. dilut.	2	84 "
Sulphuric Acid.	16 per cent. of acidum dilutum U. S. Pharmacopœia.	6	Failed to decalcify.
Chromic Acid.	2 per cent. of pure acid.	2	" " "
Acetic Acid.	Full str. acid. dilut. U. S. P.	2	" " "
Citric Acid.	16 per cent. of crystals.	2	" " "
Tartaric Acid.	do do do	2	" " "

The sulphuric acid is singularly inert in respect to bone. It seems to limit its own action by filling the pores with insoluble calcium sulphate, which prevents further access of the solution.

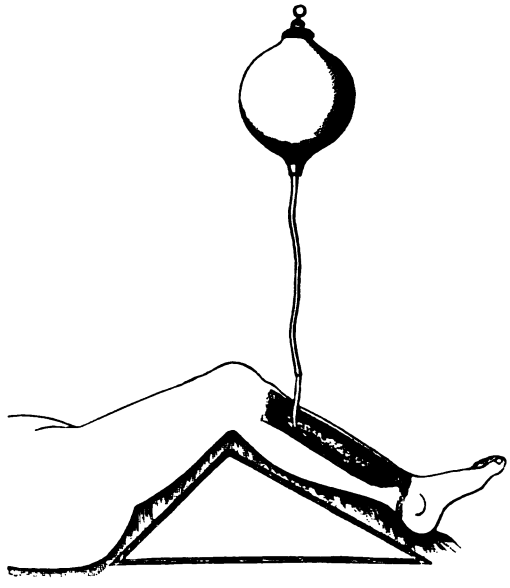
The chemical rule is this: The most rapid acid decalcifiers are those whose calcium salts are most soluble in the solution employed, such as hydrochloric, nitric, and phosphoric acids.

The experiments show that of all the acids tested, the hydrochloric is far the most rapid in action, and therefore to be decidedly preferred.

METHODS OF APPLICATION.

As before stated, I was at first too timid, using weak solutions, and applying them only once in two or three days. This prolonged some cases more than forty days, which might have been finished in ten.

FIG. 1.



THROUGH-AND-THROUGH IRRIGATION BY MEANS OF A HALF-GALLON FOUNTAIN SYRINGE.

In order to succeed, the acid solution need not be strong, but it must be steadily applied and made to bathe the whole sequestrum a considerable portion of each day. Some objectors seem to have supposed that the plan is merely to pour a little acid into the fistula once a day. Very few cases will succeed in this way, not because the acid is powerless, but because it will get no thorough contact with the bone. A little reasonable care and ingenuity must be exercised to convey the solvent into the deepest part of the cavity from whence it is to be made to plow out around the sequestrum in a frequent, copious, and prolonged irrigation.

The best arrangement, where the anatomical relations permit, is to cause the acid to enter the cavity at one end and escape at the other, thus slowly flowing along the whole length of the sequestrum, as shown in Fig. 1.

In many cases openings already exist, which admit of this method, and in many others a trifling operation will make the required orifice at a convenient spot. In case of making a new opening, the surgeon will bear in mind that fresh cuts always smart a

little if the acid is applied the first two or three days afterward, but this sensitiveness ceases as soon as the cut surface is covered with granulation. A fountain syringe is the best implement in proper cases, but a hand syringe will serve the purpose.

Where the location does not permit of two openings a single one will do almost as well, if a catheter or other tube can be made to pass alongside the sequestrum into the further part of the cavity. Fig. 2 shows this plan.

Mercier's flexible *sonde coudée*, or elbow catheter, is an old and simple instrument, but yet not well known to all the profession. It has a peculiar value in creeping into irregular channels. The abrupt curve near the tip glides over obstacles like a sleigh runner, and if it strikes an obstruction, the surgeon can, by simply turning it on its axis, give the point a new direction and thus make it pass around the resisting projection. I may be excused, therefore, for figuring it.

By selecting a pretty small one, the operator can generally insinuate it into the deepest part of the cavity. I have even passed large ones into carious cavities of the vertebræ in Pott's disease.

If the knee joint is carious several openings are necessary, with rubber tubes in them, and they should be so placed that the acid thrown into one tube will pass through the joint and escape by another. There are seven points of election where tubes may be inserted if needed, viz., two above the patella, two below it on either side of the patellar ligament, two into the sac at the posterior edge of the condyle and one in the middle of the popliteal space. I dislike the latter place, as behind a diseased knee the popliteal vessels may be crowded one way or the other by swollen tissues.

FIG. 2.

MERCIER'S FLEXIBLE ELBOW CATHETER.

In the ankle joint the points of insertion are on each side, at the junction of the anterior borders of the malleoli with the body of the tibia. In the hip joint the capsulous ligament may be tubed in front, above, and behind.

In the shoulder two points are available, one in front of the acromion process and the other behind it.

In the elbow the capsule may be tubed at several points, at the surgeon's pleasure, only not injuring the ulnar nerve. The articulations of the wrist have to be reached mostly by enlarging fistulas already existing.

Finger joints are reached at the front or the back. I have repeatedly tubed all the above joints for drainage and antiseptic injections, the knee included, with excellent results, but have as yet applied the acid solvents to only part of them.

In Pott's disease of the lumbar vertebræ, I have found the following course both safe and beneficial. The first step is to open the abscess widely wherever it points. Then explore the cavity with the finger and trace it by large probes and elbow catheters into the lumbar region. Cut down upon the instrument at the outer border of the common mass of the erector spinæ muscles, opposite the affected vertebræ. Then insert the index finger and examine all parts of the abscess cavity and the adjacent half of the body of the affected vertebræ. If a cavity is found in the bone, insert a tube well into it, and use it as the route of the future acid injections.

A scoop can sometimes be used, but not always. In one instance I found that every scrape of the instrument caused spasmodic jerks of the legs, showing that the cauda equina was disturbed. No paralysis followed, however, and I dissolved away all the carious bone by the acid injections, so that no roughness could be felt by the

probe. The patient returned home greatly improved in health and apparently on the way to recovery.

It is a matter of regret that I preserved no records of my earlier experiments, but of late I have done so and present here an abstract of them.

CASE I.—A gentleman received a penetrating wound of a joint of the index finger, causing suppuration and necrosis of the ends of the two bones constituting the articulation. A solution of hydrochloric acid was slowly injected into the joint every day. In about five days the dead bone was all decalcified, and in about fifteen days the parts were solidly healed, with anchylosis. The strength of the solution was not measured in this case, but made up extempore, mixing with water as much acid as would give a moderately pungent sensation on the tip of the tongue.

CASE II.—Caries of a vertebra and lumbar abscess. Having etherized the patient, I opened the abscess widely in the lumbar region, along the outer border of the common mass of the erector muscles. Inserting the index finger I found a carious hollow in the side of a lumbar vertebra. Into this I carried a large drainage tube and proceeded to use injections very cautiously, the hydrochloric solution containing at first only six per cent. of the *acidum dilutum*, and it was injected only three times a week. No evil followed, nor anything to show that the injection might not just as well have been used three times a day. Its strength was increased to sixteen per cent. of the *acidum dilutum*, and the carious bone was all gone in twenty-seven days. The wound granulated and healed from the bottom, and the patient was completely and permanently cured.

CASE III.—Multiple cavities containing small sequestra in both tibias. The acid was commenced at eight per cent. of the *acidum dilutum*, and slowly increased to seventeen per cent., but only used three times a week. The bone was all decalcified in forty-two days. The fistulas filled up with solid granulations, and the healing was nearly completed when the patient left the hospital.

CASE IV.—Some small sequestra in an old fracture of the radius. Solvent used at the strength of ten per cent., increasing to seventeen per cent. The injections were only three times a week. The sequestra disappeared in thirty days. The cavities filled with new flesh and the surface was nearly healed when the patient was discharged.

CASE V.—Necrosis in the lower half of the shaft of the femur. The principal sequestrum was removed by operation, but the probe felt another smaller one in a location difficult of safe access. The injections were used at ten per.cent. of the *acidum dilutum*, increasing to seventeen per cent. two or three days apart. The sequestrum was decalcified in forty-two days. The wound granulated well and the patient was discharged improving.

CASE VI.—Necrosis in radius and ulna from gunshot wound. Acid commenced at seventeen per cent. of the *acidum dilutum* three times a week. Sequestra all gone in twenty-four days. The cavities filled up with new flesh and were nearly healed when the patient was discharged.

CASE VII.—Some inaccessible sequestra left after an attempted removal. Strength of solution and frequency of injection the same as in the preceding case. Solution finished in twenty-six days. Cure complete.

CASE VIII.—Some small sequestra left after an operation. Strength and frequency of injection same as previous case, but the acid employed was the nitric. Solution finished in thirty-eight days. Discharged nearly cured.

CASE IX.—Complicated sequestrum cavity in the upper half of the femur, and a carious hole through the ala of the ilium. Great difficulty in making the solvent reach all parts of the crooked passages, but wherever it could be got in it removed the dead bone in about twenty days. The acid used was the nitric.

CASE X.—This case was in the military practice of Dr. Wm. Watson, now of

Dubuque, Iowa, and occurred about twenty-two years ago. The patient, a soldier, had a tumor—perhaps sarcomatous—on the front of the sternum. On the removal of the tumor and the scraping of the bone beneath, a necrosis took place over an area about an inch in diameter, and quite uncovered and open to easy inspection. Dr. Watson dressed it daily with pledgets of lint well saturated with hydrochloric acid of the strength of twenty-five per cent. of the *acidum dilutum*. Under this application the sequestrum visibly melted away, and had nearly disappeared when the patient was transferred to another hospital.

CASE XI.—Date not given. This was a patient treated by Dr. Watson and Dr. Hill, of Dubuque. Necrosis of the tibia existed, extending from a spot near the knee some distance downward. Hydrochloric acid of the strength of twenty-five per cent. of the *acidum dilutum* was injected daily. The sequestrum was decalcified except a few spicula washed out by the force of the injection. The cavity healed rapidly and permanently.

CASE XII.—Necrosis of lower part of shaft of femur, existing twenty-five years. Patient aged forty-three years. A sequestrum was removed by operation, leaving a large cavity wide open to sight and touch. A rough stratum of necrosed bone, adhered to the bottom of the cavity, was in full view, apparently having died more recently than the other piece, since its separation was not complete. The cavity was cup-shaped and held at least an ounce of fluid. Into this was poured daily a solution of one part of *acidum hydrochloricum dilutum* to three of water, gradually increasing to the full strength of the *acidum dilutum*. The acid did not injure the granulations, and caused no pain unless it was allowed to touch the raw edges of the skin. It did not affect the living bone, but dissolved the dead bone, while vigorous granulations grew from the place where it lay. In twenty days there remained only a very small spiculum undissolved, which will disappear in a few days. This case is of interest, as showing that the dilute acid in a cavity thoroughly open to view, plainly did no harm either to granulations or to living bone.

For the careful carrying out of my plans in most of the above cases I am indebted to the assistance of my colleague, Prof. E. Wyllys Andrews, and to Drs. Brell, Kerr and Whitfield, internes of Mercy Hospital.

Increased experience shows that my earlier way of using the acid was much too slow and cautious. There is no reason why the cavity containing the sequestrum should not be kept full of the solvent a large portion of the time, and the weeks of the treatment be reduced to days.

It is not claimed that acid irrigation can totally abolish necrotomy, but the observed facts show the following advantages :—

1. The great majority of sequestra can be removed by the acid irrigation alone.

2. We do not have to wait for the separation of the sequestrum. We can commence the solvent process at once, and often have it complete long before the lapse of the three months which must usually precede a surgical operation.

3. A large portion of carious articulations can be successfully healed without excision.

4. The acid can be safely and successfully sent into hollow vertebræ, and into carious cavities close to large joints where instruments can only be used with great caution, or not at all.

The following paper was read :—

ON THE POSSIBILITY OF OPERATIONS ON THE ŒSOPHAGUS THROUGH THE STOMACH, AS SHOWN BY DISSECTIONS—PRESENTATION OF A PATIENT FROM WHOSE ŒSOPHAGUS A SET OF TEETH WAS REMOVED BY AN OPENING THROUGH THE ABDOMINAL WALL OF THE STOMACH.

DE LA POSSIBILITÉ DES OPÉRATIONS SUR L'ŒSOPHAGE PAR L'ESTOMAC COMME LES DISSECTIONS LE DÉMONTRENT—PRÉSENTATION D'UN PATIENT DE L'ESTOMAC DUQUEL ON AVAIT RETIRÉ UN DENTIER PAR UNE OUVERTURE PRATIQUÉE DANS LA PAROI ABDOMINALE DE L'ESTOMAC.

ÜBER DIE MÖGLICHKEIT EINER OPERATION AN DER SPEISERÖHRE VOM MAGEN AUS, WIE DURCH SEKTIONEN BEWIESEN—VORSTELLUNG EINES PATIENTEN, AUS DESSEN SPEISERÖHRE EIN KÜNSTLICHES GEBISS DURCH ÖFFNUNG DER VORDEREN MAGENWAND ENTFERNT WURDE.

BY MAURICE H. RICHARDSON, M. D.,

Assistant Professor of Anatomy in Harvard University, Surgeon to the Massachusetts General Hospital.

A little more than a year ago my attention was first drawn to this subject by the patient whom I shall have the honor to present here to-day. At the time he came to the Massachusetts General Hospital for relief, his condition was so deplorable, and immediate action was so imperative, that there was no time for any preliminary work upon the cadaver for the purpose of studying the best methods of performing what has proved to be a new operation in surgery.

The details of this case have already been published, with some few observations on the operation and the anatomy of the œsophagus. (*Boston Medical and Surgical Journal*, Dec. 16th, 1886.)

Since that time I have made many dissections for the purpose of showing the best way of doing this operation, and of demonstrating the possibility of reaching any part of the œosphagus with the finger. In all the dissections the distance was measured from the upper incisors to the cardiac orifice, as well as that from the cardiac orifice to the point in the neck where external œsophagotomy is usually performed.

The patient, a man aged thirty-seven, tall and strong, weighing, when in good health, from 160 to 180 pounds, had swallowed while eating a denture of four teeth. He had presented himself at the Out-patient Department of the hospital the next day, and I examined him then. The teeth were lodged near the cardiac end of the œsophagus. He was admitted to the hospital and numerous attempts were made to dislodge them. At one time sufficient traction was used to break the coin probang which had been made fast to the plate. He was finally discharged from the hospital, having gained in weight, and it was thought that the foreign body had passed into the stomach. Eleven months after the incident, he again came to the hospital, and the foreign body was found to be in the same place as before, fourteen inches from the upper incisors. The patient was reduced to 113 pounds in weight. He had continual pain, and it was with great difficulty that he was able to swallow enough liquid food to keep him alive.

There were two methods of procedure possible, all attempts at removal through the mouth having failed, one by external œsophagotomy, in the manner described by Dr. Leroy McLean, of Troy, and so successfully performed by him (*New York Medical Record*, Vol. xxvi, p. 282), and the other through the abdominal wall and stomach. I

selected the latter method, because it had been shown that the plate was very firmly impacted indeed, and it was very probable that it would be necessary to use the fingers in dislodging it.

For two days before the operation the patient was nourished by enemata, and nothing was taken into the stomach but water and stimulants. After etherization a cut was made parallel to the lower margin of the ribs on the left side, six inches in length, through which the stomach was drawn and held upon aseptic towels by assistants. A small opening through the stomach wall was next made, midway between the curvatures, and forceps introduced for the purpose of grasping the plate. The cardiac opening could not be found. The cut was then enlarged enough to admit the right hand, which was introduced till the œsophageal opening was found. The foreign body was detected by the fingers about two inches above the diaphragm. After considerable careful manipulation, with the index and middle finger, the plate was detached and removed. The mucous membrane of the stomach was closed with a cutaneous silk suture, and the other layers with thirty-six interrupted silk sutures, after the method of Lembert. Patient made a good recovery, though complicated by slight lung symptoms, caused probably by the breaking of a small peri-œsophageal abscess into the lung by the manipulation of the fingers.

He rapidly gained in weight, from 113 to 167 pounds, and went to his work as a teamster. He is perfectly well to-day, and able to do as hard a day's work as any man.

In the selection of the above operation, and in its first performance, various questions arose, to the solution of which I devoted what little time I had. As will be seen, I was ignorant of several important points, which I have since demonstrated. I am positive that the foreign body could have been removed with instruments had I known at that time what I do to-day about the subject.

During the past winter, I have done this operation on sixty subjects, and made various observations, the general result of which I herewith submit.

The first question that naturally arose was as to the exact location of the foreign body with reference to the cardiac end of the œsophagus, and also with reference to the cricoid cartilage, in order that an intelligent choice might be made between gastrotomy and external œsophagotomy, it being taken for granted that that operation is the better in which it is possible to use the fingers if instruments fail.

The second question was: What portion of the œsophagus is accessible to the fingers after the operation of external œsophagotomy, and what after gastrotomy?

If gastrotomy is first performed, the best incision through the abdominal wall for reaching the cardiac end with the finger, and the best where instruments are to be introduced?

Finally, the best incision through the walls of the stomach, and the details of the operation as to the manipulation of the stomach and œsophagus?

Location of the Foreign Body (or condition requiring operation).—If it is known how far the obstruction is from the upper incisors when the head is thrown back, in the position for passing the exploring probang, and if the distance from the same point to the cardiac opening is determined, it is evident that we can tell immediately whether the obstruction can be reached from below or not.

The results obtained in the measurement of fifty-five subjects show that the distance from the upper incisors to the opening in the diaphragm is not constant, the average distance is fourteen and a half inches (36.8 cm.). Unfortunately, there is no constant ratio between the height of the individual and this measurement. One subject, six feet in height, measured between the points mentioned fifteen and one-quarter inches. Another, with a height of four feet ten inches, measured fourteen and a half inches.

The greatest distance was seventeen inches, and the least ten and a quarter.

The average distance from the cricoid cartilage to the cardiac opening was seven and a half inches in nineteen subjects taken at random. The greatest distance was nine and a quarter inches and the least six and a half.

From these figures, it must be said that it is not possible to locate the foreign body accurately with reference either to the cardiac opening or the cricoid cartilage. If the individual is of average height, and with a neck of ordinary length, it is safe to say that the distance from the incisors to the diaphragm is about fourteen and a half inches. If the probang is arrested at a point more than thirteen inches from the incisors, the point of obstruction is probably at or near the cardiac end of the œsophagus. Inasmuch as the œsophagus is constricted at the diaphragm, it goes without saying that foreign bodies that have passed the constriction at the upper end are at the cardiac end, on the chances.

All parts of the œsophagus are accessible to the fingers, either by gastrotomy or by external œsophagotomy. In all the subjects examined this fact was observed. In some of them it was quite difficult to reach every part; but it was, nevertheless, possible. The method of making these observations was to do first an external œsophagotomy in the usual way. Then the stomach was opened and the fingers of the left hand introduced into the cardiac opening. With the forefinger of the right hand in the upper wound, and the fore and middle fingers of the left hand in the lower end of the œsophagus it was found possible not only to make the fingers touch, but in many cases to make them overlap quite a distance. Coins and other bodies were passed from one hand to the other in both directions with great ease. In some cases it was necessary to use both the index and middle fingers of the right hand as well as the left.

It cannot be denied that these conditions of observation cannot be reproduced on the living subject, and that the results are, therefore, only approximate. It would be hardly practicable to do both operations on the same patient. If both hands are in use pushing toward each other, it is, of course, much easier to bring the fingers together, and to push the foreign body from one finger to the other waiting to receive it. Yet, even in the living, I think it is safe to say that there is no point in the œsophagus which cannot be reached by the finger either from above or below. The other source of error is in the fact that, on the cadaver, the diaphragm is relaxed. This does not make any appreciable difference, however, because the opening in the diaphragm is so fixed to the bodies of the vertebræ that it does not share to any extent in the contractions of that muscle.

It is possible to reach with the left hand three inches above the cardiac opening, that is to say, the length of the left middle finger. From above, through the wound in the neck, one cannot reach quite so far, on account of the sternum and clavicle. One can, however, easily reach the arch of the aorta, and even hook the finger under it in some cases. In a few of the dissections the measurement was made between the centre of the incision in the neck to the diaphragm. The distance was a little over six inches. Allowing in the average neck an inch and a-half to two inches from the cricoid cartilage to the lower point of the incision in the œsophagus, in external œsophagotomy we have the average distance from that incision to the cardiac opening of five and a half to six inches.

The general conclusion to which I have arrived from these measurements and digital explorations is, that the obstruction can be reached by the finger in all, or nearly all, cases when it is situated more than six inches below the cricoid cartilage. If less than six inches below the cricoid ring it can be reached best from above, though in some few cases it may be just beyond the reach of the finger. Even in these cases instruments can be easily and intelligently applied.

The measurements of the point of impaction or obstruction having been carefully made, and it having been demonstrated that the point in the œsophagus to be reached, if necessary by the finger, is not less than thirteen inches from the upper incisors or six from the cricoid cartilage, the operation of gastrotomy should be performed. Inasmuch as the best argument in favor of gastrotomy is, that the fingers may be used to detach the foreign body, if all other means fail, it is evident that the probability of the introduction of the hand into the stomach is the most important element in the selection of the incision through the abdominal wall. In the case of the patient upon whom I have done this operation it seemed to me best to make an oblique incision under the margin of the ribs on the left side, directly over the stomach. By this incision the stomach can be recognized very easily and drawn out of the wound so far that the consequent manipulations can be made without the escape of the contents into the peritoneal cavity.

From a large number of dissections for the purpose of deciding the question of incision in the abdominal wall, I have come to the conclusion that the oblique cut is the best, on the whole, for this operation, although the fingers can be introduced into the œsophagus with ease after making the median cut.

The incision through the walls of the stomach for the purpose of introducing instruments into the cardiac end of the œsophagus may be made anywhere, provided that it is far enough to the right to avoid the convexity of the lesser curvature. All that is necessary is to put the lesser curvature on the stretch, so that it makes a straight line to the diaphragmatic opening. The cut through the stomach wall must be far enough to the right to allow the passage of the instrument along the sulcus between the anterior and posterior walls of the stomach made tense as above.

If the instrument is brought obliquely to this groove and passed upward, all the time being pressed gently against the straightened lesser curvature, it will glide into the œsophagus every time with the greatest ease.

The incision in the median line was made in many subjects, and the conclusion was that it is the better method of operation where it is intended only to pass instruments into the œsophagus.

In these observations it was found practicable to pass instruments into the œsophagus, and withdraw coins with the greatest ease through an abdominal cut in the median line, only two and a-half inches in length. By enlarging the incision it was found to be quite practicable to introduce the hand into the stomach and pass the fingers into the œsophagus, but not so easy as by the oblique incision. It was also much more difficult to draw the stomach out of the peritoneal cavity, enough to avoid the escape of the contents of the stomach into the abdominal cavity.

The stomach having been exposed by the lateral cut through the abdominal walls, and having been drawn out of the wound, it must be held by an assistant in such a way that the lesser curvature is put on the stretch. It is better for the assistant to hold the stomach in both hands so that the whole viscus is flattened out. He should stand on the left of the patient and grasp the greater curvature. The operator standing on the right of the patient holds the lesser curvature between the left thumb and forefinger, thereby making tense the lesser curvature and assisting in the passage of the instrument. Before doing this, it is best to introduce the hand into the peritoneal cavity and examine the diaphragmatic opening externally. With the stomach held as described, the opening through its walls may be made anywhere in the flattened surface so as to avoid the large vessels.

It goes without saying that the incisions should first be just large enough to get hold of the stomach with the thumb and fingers, and draw it out so as to stretch the lesser curvature. An incision through the stomach wall can then be made large enough to

introduce the instrument by which I believe the foreign body can be removed with ease in the majority of cases.

In cases of stricture of the œsophagus the instrument can be passed with very small openings, both in the abdominal walls and in the stomach itself. For this operation the median incision is the better.

The wounds in the stomach and abdominal wall should then be closed in the usual manner.

In the selection of the operation, whether by external œsophagotomy or by gastrotomy, another reason for the choice of the latter operation is, that there is less danger of wounding important structures in the thorax where the traction is downward, than when it is upward. If the body is at the orifice, it is not in direct relation with either pleura, heart or aorta. There is much less danger of lacerating the œsophageal wall in pulling down the foreign body a short distance than pulling it up a considerable one.

Finally, the prognosis after laparotomy and gastrotomy, where the opening in the stomach is small, I believe to be very good indeed.

As to the frequency of the conditions requiring this operation, with the exception of impacted foreign body and stricture of the œsophagus, I know of no indication for the operation. It is certainly within the experience of nearly every surgeon that foreign bodies do become impacted low down in the œsophagus, and that the dangers which arise therefrom are very great. It is in the hope that, in such cases, this method which I have described will be tried that I submit these observations.

Since L'abbè's successful case, in 1875, to the present time, there has been no reported fatal case of laparotomy and gastrotomy, for foreign body in the stomach. In the two fatal cases one had been preceded by an external œsophagotomy, and in the other the stomach wall had been perforated by the foreign body. The list comprises the cases of L'abbè, Kocher, Schönborn, Thornton, Billroth, Credé, Bernays and Polaillon.

The following address was delivered by Dr. Carnochan :—

CONGENITAL DISLOCATION OF THE HIP JOINT.

DISLOCATION CONGÉNITALE DE LA JOINTURE DE HANCHE.

ANGEBORNE LUXATION DES HÜFTGELENKES.

BY JOHN MURRAY CARNOCHAN, M. D.,

Of New York.

Mr. President and Gentlemen—In addressing you on the subject before us, I beg to show you a specimen of interest in this connection, chiefly on account of the difficulty of obtaining post-mortem illustrations of the disease.

This specimen represents a double dislocation of the head of the femur, called a congenital dislocation. It is asserted by many surgeons who have given attention to the matter that the dislocation or misplacement in such cases takes place in utero. There is no doubt that similar conditions have existed in this disease, as in other maladies, in past times, but until about 1820 the profession generally had no cognizance of it. An Italian surgeon, Palleta, was probably the first to write of it, but until 1826, when Dupuytren was presiding at the Hôtel Dieu, and inculcating

lessons of experience to his pupils, from every quarter of the globe, it was not understood at all. In 1826, Dupuytren gave, in one of his lectures, an illustrated description of the disease, calling it *luxation originelle de la tête du fémur*, and in concluding the subject, it being, as he believed, unknown, said . . . "mon but n'a point été de grossier le catalogue déjà trop nombreux des misères humaines, mais d'éviter aux practiciens de graves erreurs de jugement, et aux malades, des traitemens aussi inutiles qu'ils sont dangereux."

Since his time some of the French surgeons have taken the matter up, but still the affection remains unknown to a great degree; even Sir Astley Cooper, the highest authority on fractures and dislocations, does not mention it in his work.

In 1845, I met Mr. Wm. Adams, Curator of the Museum of St. Thomas' Hospital, London, then a young man, but since risen to great eminence, with whom I conversed much on this subject. He read an article before the British Medical Association, at Cardiff, in 1885, in which he gives me the credit of introducing the knowledge of this malady in England. If you will pardon the apparent egotism, I will read what he said before the British Medical Association, as published in the *British Medical Journal* of November 7th, 1885: "My attention has, however, always been directed to this affection, since it was first pointed out to me, in the year 1845, by Dr. Carnochan, of New York, who was then visiting England, having recently come from Paris, where he had carefully studied this affection from the specimen in Dupuytren's museum; also from his published works. Dr. Carnochan brought a case of so-called congenital dislocation of both hips, in a boy aged 18, to St. Thomas' Hospital, when I was curator of the museum, for the purpose of demonstrating this affection; and, probably, this was the first case noticed in England. The external character of the dislocation, and its effects in altering the conformation of the chest and abdomen, in consequence of the tilting forward of the pelvis, and the production of lordosis to an extreme degree in the lumbar region, were so strikingly illustrated in this case that, under the direction of Mr. South, an entire model of the boy was made by Mr. Kearney, and is now in the museum of the hospital."

I make this quotation, to show that even now the subject is one of great obscurity. It has been under discussion for the last two or three months in the Royal Medical Society of London. It is not an uncommon malady, for I have been told by a gentleman who practices in Washington, that he has seen about fifty cases, chiefly among the colored population, and some cases among the whites. In New York I have seen about seventy-five cases, in the course of twenty-five years. Mr. Adams, in his paper, mentions that he has seen and taken note of no less than sixty cases within twenty-five years, which shows that the disease is not so unusual as the general lack of knowledge concerning it would indicate. Let me call your attention to the appearance of the patient while in a standing position. The plates show the extreme development of the disease or affection, running from birth up to seventy years of age. They present three views, viz., anterior, posterior and lateral. (See p. 634.) In the anterior aspect the most marked points are the apparent elongation of the arms, the tips of the fingers dropping down below the level of the upper margin of the patella. The abdomen presents a protuberant appearance, the pelvis is tilted forward, the folds of the groin between the abdomen and upper part of the thigh are deeper, less transverse and more vertical than in the healthy subject. The legs appear proportionately shorter and more slender, and while standing the feet are usually pointed forward in a straight direction. Time will not allow me to describe the various soft tissues between the cutaneous surface and the ligaments and osseous structures,

but it may be stated, that all the tissues, including the pelvis itself, are modified in their nutrition. You will observe that the heads of the bones, which escape from the acetabula in utero, pass to the dorsa of the ilii. The head of each femur has lost its spherical appearance, and is changed in its dimensions and texture. The articular cartilage has disappeared, the head of the femur having come in direct contact with the osseous tissue of the ilium. Where the round ligament should be inserted, the head represents a flattened surface, denuded of its articular cartilage, a thin brittle shell of bone only covering the deteriorated, cancellated interior structure of the head. The neck of the femur is small, short and stunted, and assumes a more horizontal direction to the axis of the femur. The head and neck of the femur may assume in this affection various shapes and degrees of deformity, from the simple

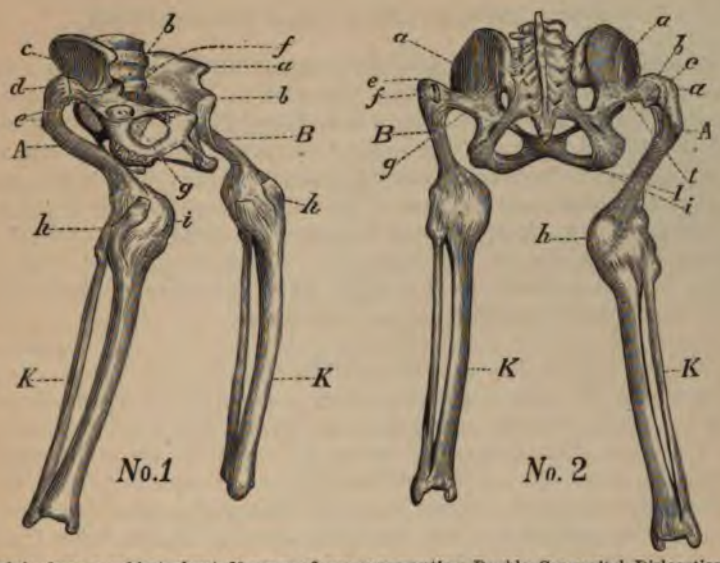
Double Congenital Dislocation.

Double Congenital Dislocation of the Head of the Femur on the Dorsum ilii—age 70.

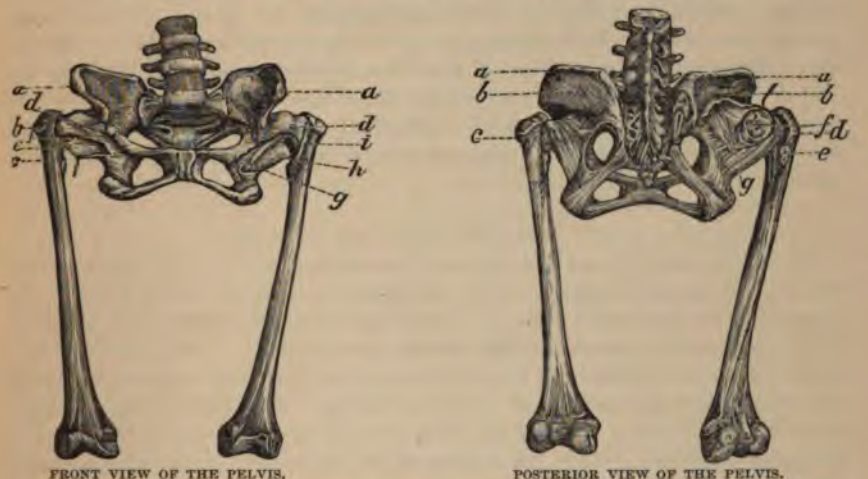
changes which take place in early life up to their complete atrophy and disappearance in old age. The primitive acetabula, long in disuse, are changed entirely in shape, size and position. They present a shallow, contracted, triangular form, more or less changed in position, and totally unfit for the reception of the head of the femurs. They present no articular cartilage or remnant of ligamentum teres, the bottom of the cavity presenting at the time of dissection only a loose tissue, somewhat resembling the haversian glands.

The synovial membrane, and the articular cartilage of the primitive cotyloid cavity, like the same structures of the head of the femurs, have disappeared, leaving the osseous tissue denuded.

The capsular ligament and the ligamentum teres are found also to present various phases and alterations; as a consequence of the ascent of the head of the femur upon the dorsum ilii, the capsule must become elongated and stretched beyond its normal dimensions, the round ligament also becomes stretched and more slender,



Male Pelvis, from a subject about 60 years of age, representing Double Congenital Dislocation at the Hip. This caused great alteration of the femur at the femoro-tibial articulation.



Dr. Carnochan's case of Female Pelvis, representing the Double Congenital Dislocation of the Heads of the Femurs upon the Dorsa of the Iliac Bones.

On the Left—
(*a*) Anterior and Superior Spinous Processes of the Ilium.
(*b*) Trochanter Major.
(*c*) Trochanter Minor.
(*d*) Anterior part of the original Capsular Ligament.
(*e*) The original Capsule laid open.
(*f*) The annular opening by which the head of the Femur escaped upon the Dorsum Ilii.
(*g*) The original Acetabulum become now triangular.
(*h*) The cavity of the original Capsule laid open by removal of its anterior wall.
(*i*) The Neck of the Femur grasped by the annular opening in the capsule through which the head passed.

On the Right—
(*a*) The Crest of the Ilium.
(*b*) The Dorsum Ilii.
(*c*) Posterior surface of the New Capsule, entire.
(*d*) Trochanter Major.
(*e*) Trochanter Minor.
(*f*) Head of the Femur lying on the New Capsule.
(*g*) New Capsule laid open, showing its interior.

and is embraced by the central contracted portion of the capsule, through which it must extend while it remains unbroken. The original, articular capsule may retain for many years its integrity, the head of the femur playing upon the dorsum ilii during progression, a layer of the capsule intervening between the head and the external surface of the ilium. At length, from continual friction and pressure, absorption takes place, and a portion of the wall of the capsule gives way so as to allow the head of the femur to escape from its cavity, and to come in direct contact with the osseous structure of the dorsum ilii. This state of things may exist for a longer or shorter period, the head of the femur passing to and fro between its original capsule and the external surface of the ilium.

NEW CAPSULE.

When the head of the femur has escaped from its natural capsule and becomes placed in contact with the surface of the os ilium, a new set of phenomena take place, reparatory efforts are made to restore the head of the bone to something like its former condition, and nature attempts to form a new capsular ligament which, on the one hand, is attached to the dorsum and contiguous portion of the ilium, and, on the other, to the outer and posterior surface of the old capsule, and to the margin of the perforation through which the head of the femur had made its escape. Or, the head may never have escaped upon the ilium, and, in that case, in proportion as the head becomes atrophied and disappears, the sides of the old capsule become coalesced and glued together, as previously remarked. A set of radiating fibres, springing from the dorsum of the ilium, becomes attached to the posterior part of the cord, thus formed, and fortifies it. These fibres probably represent, in another form, a recent capsule which, under other circumstances, as when the head has pierced the old capsule and rests upon the dorsum ilii, would assume a regular capsular form.

THE NEW ACETABULUM OR SOCKET.

By the formation of the new capsule, a false articulation is partly accomplished, which, to be complete, requires a new arrangement upon the dorsum of the ilium, to represent a new acetabulum. This end is attained in two different ways: in one there is a simple glenoid depression scooped out, as it were, upon the dorsum of the ilium; in the other, nature increases her efforts, and, as often happens after traumatic dislocations, new osseous matter is thrown out upon the ilium, which, at times, assumes to some extent the cup-like form of an acetabulum. In the first case the new capsule attaches itself to the margin of the depression, in the second to the osseous border of the new socket.

GENERAL CHANGES WHICH TAKE PLACE IN THE OSSEOUS TISSUE.

The alterations which occur in the parts of the skeleton in proximity to the morbid articulation, or, even at a considerable distance from it, merit consideration. From my own observation, the diameters and the several measurements of the pelvis are invariably changed, and the tissue of the bones, not only of the fossa femora, but also of those more remote, undergo alterations in their texture, density and volume. With so great a deviation from the natural condition of the parts, an interesting question arises as to the causation of the initial steps of the pathological changes. Is it an original malformation? an arrest of development (*un arrêt de développement*)? Can it be the result of traction, badly applied during the *accouchement?*—or, may it not be the result of morbid contraction of the muscles about the hip

joint, as in club foot, dragging the head of the femur out of the shallow acetabulum favored by the position of the thigh bone of the fœtus flexed to its utmost upon the abdomen? Each of these suppositions has had its supporters. Dupuytren was in favor of the theory of the *arrêt de développement*, as also Mr. Adams. I am disposed to favor the theory of muscular retraction acting upon the upper portion of the femur, dragging the head of the bone by degrees out of its natural receptacle. The theory of Farrier, localizing the motor power in certain parts of the cortical substance of the brain, permits of an easy inference that the muscles attached to the trochanter major and upper part of the femur may be struck by morbid contraction under the influence of the particular part of the gray substance, while in a condition of irritability. My own opinion goes still further, and, in consideration of the diversified pathological conditions or malformations met with in early and in mature life in connection with the hip joint, I refer the etiology to a diversity of causes, particularly to that of arrest of development, and to continued muscular retraction.

DIAGNOSIS.

The pathognomonic signs in adult and mature life are so marked that little doubt is left in designating the nature of this affection. In early life, however, where the full symptoms are not developed, and where there is excess of infantile fat, and where the patient has not reached the age of locomotion, the diagnosis is often difficult. From birth to two or three years of age the shortening is slight, not more than half an inch, and all movements are difficult, from the abundance of fat and the small size of the bones; but from that time the diagnosis is more easily made. Mr. Adams in his able paper, above referred to, giving the diagnostic points, states: "1st, We are able to decide that the shortening of the limbs is above, and not below the knee. 2d, From the absence of symptoms of hip-joint disease, and free mobility of the joint without pain, congenital dislocation may be allowed to be the cause of the shortening. 3d, The crucial test of measurement of the ilio-femoral triangle of Bryant, and also by Nélaton's line, can now be more readily made, and the top of the great trochanter will always be found to be on a level, or nearly so, with the anterior superior spinous process of the ilium. These measurements, together with the other symptoms, will at once decide the case. From five to ten years of age, the head of the femur can generally be felt rolling under the fingers when pressure is made. As has already been stated, as age advances, say from ten to twenty years, all the diagnostic symptoms become exaggerated, especially in the case of so-called double dislocation. When one hip joint only is affected, tilting of the pelvis and lateral curvature of the spine are certain to result in the inequality of the length of the leg." (Vide *British Medical Journal*, article by William Adams, November 7th, 1885.)

PROGNOSIS AND TREATMENT.

It is not possible to pass in review all the details which might be embraced in a special monograph on congenital dislocations; I shall therefore be obliged to finish the subject of this article by a few condensed remarks on the prognosis and treatment of congenital hip-joint disease.

The pathological condition of the parts, as heretofore stated, do not afford a favorable prognosis regarding the ultimate curability of this affection. Dupuytren gives the following opinion: "On se consolerait aisément de ne pas connaître de ces déplacements, si l'on connaissait les moyens de les faire cesser, ou du moins de pallier leur mauvais effets; malheureusement il n'eu est pas ainsi, et ces déplacements ne comportent ni remède curatif ni même palliatif bien efficace." Yet, in

more recent times, attempts have been made to bring about permanent reduction of the head of the femur, which, under favorable circumstances, may have been attained. As to the mode of treatment, the general principles to be observed are carried out by the application of prolonged extension and counter-extension, by means of apparatus devised for the purpose, and certain gymnastic exercises to bring into operation the more normal action of the muscles connected with the ilio-femoral articulation. In addition to this it has been attempted to bring about artificially the process which nature performs in cases of traumatic dislocations, by inducing the formation of a new osseous cotyloid cavity, by producing artificial irritation upon the dorsum of the ilium by subcutaneous scarification of the periosteum at a suitable place on the dorsum of the ilium. In addition to this effort, and based upon the theory of muscular retraction acting as the original cause of the displacement, the muscles at fault have been divided subcutaneously, as in club foot and similar affections.

The following paper was read :—

AN ORIGINAL METHOD OF TREATMENT OF TALIPES VARUS WITHOUT TENOTOMY, OR THE DIVISION OF ANY TENDON OR FASCIA.

MÉTHODE ORIGINALE DU TRAITEMENT DES PIEDS-BOT VARUS SANS TÉNOTOMIE OU DIVISION DES TENDONS OU "FASCIA."

NEUE BEHANDLUNG DES TALIPES VARUS OHNE TENOTOMIE ODER DURCHSCHNEIDUNG IRGEND EINER SEHNE ODER FASCIE.

BY ISAAC N. QUIMBY, M.D.,

Surgeon of Jersey City Hospital; formerly Surgeon of Hudson County Hospital, N. J., etc.

It is not the purpose of this paper to discuss the etiology or literature of congenital varus—whether it is caused by maternal impression, pressure of the uterus, arrest of nervous development, nervous irritability, contraction or paralysis of the muscles. Permit me to say, however, that I do not believe that this deformity is due to the arrest of development, for it is rare to find any deficiency in the tissue formation of bones, muscles or ligaments. But, on the contrary, it appears to be due to a combination of muscular contraction and motor paralysis. Moreover, the practice still followed by some surgeons, of deferring treatment for a year and then dividing the tendons and fasciæ, and then confining the limb in hard and unyielding mechanical appliances, preventive of growth and development, is as unphilosophical as it is unreasonable ; sometimes resulting in no benefit to the patient, or even aggravating the deformity.

There is no better way of destroying bony and muscular development or nervous energy than in forcing a young and growing limb into a fixed position.

Therefore, being dissatisfied with the results of the treatment of talipes as it was practiced, as long ago as 1863 I devised the method which it is the object of this paper to describe. It is scarcely necessary for me to say, in this presence, that this was before the publication of the very excellent and practical work of Mr. R. Barwell, of London, or the very instructive papers of Prof. Lewis A. Sayre, of New York. And if any publications of similar practice were made at an earlier date, they escaped my observation.

This method I have termed "*The Stretching and Stimulating Process*," on account of the systematic application of adhesive plaster and bandages applied to the foot and leg soon after birth.

This method of treatment was first described by me before the New York Academy of Medicine. Subsequently, in 1868, I read a paper, partially describing it, before the American Medical Association. Since that time I have treated in my own practice, and in the clinic of the late Prof. A. C. Post, of New York, some thirty-eight cases, with uniform success. As the treatment of these cases was nearly uniform, it is unnecessary to give them in detail, but, instead, the following two cases will illustrate the general treatment :—

As a preliminary step, the nurse is directed to wash and rub the deformed foot and leg twice a day, for a week or ten days, with a one-pint solution of one drachm of alum in two parts of water and one of alcohol, for the purpose of hardening the skin, to prevent excoriation, which, otherwise, sometimes follows the traction of plaster. Meanwhile, the nurse is also instructed to occasionally hold the foot in position. After the hardening process has been thus effectually pursued, the adhesive plaster, preferably that which is spread on India rubber, is applied, in the following manner :—

Take two pieces (sometimes one will do), cut one and so apply it as to cover the

FIG. 1.

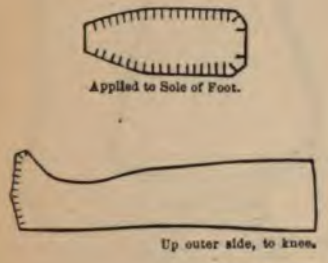


FIG. 2.

entire sole of the foot, leaving the toes free. The second piece is then cut in the form of a parallelogram (see Fig. 1), one and a half to two and a half inches wide, and long enough to extend about an inch over the inner side of the foot, across the sole, and up the outer side of the leg to the knee. On the foot end of this piece there is an expansion, sufficient to cover the entire sole of the foot to the heel, the foot being brought into proper position and held by an assistant. A roller bandage is now applied, commencing at the toe, and extending up to the knee. This simply serves to keep the plaster adherent. (Fig. 2.)

During the first eight or twelve weeks of this treatment, the bandages are changed, and the plaster reapplied two or three times a week, according to circumstances ; at the same time *thorough passive motion* is made.

After ten or fifteen weeks of this treatment, unless the case is one of great obstinacy and rigidity, the bandages are changed but once a week and the plasters renewed whenever they become displaced or relaxed.

For example : CASE I.—I was called January 8th, 1864, to attend Mrs. T., who gave birth to a child with talipes varus of an aggravated form in both feet. After tanning the

feet, so to speak, with the lotion above described, for about a week, the plasters and bandages were applied. During the first eight weeks, these were changed about twice a week, when the improvement was so great that it was unnecessary to renew them only as they became relaxed or displaced. The feet at this period (four months) became so nearly normal in their position, that the mother was able to continue this treatment until the child was one year old, since which time she has worn nothing but the ordinary shoes, there not being even a turning in of the toes, so common with children who suffer from this malformation. (See Fig. 3.)

CASE II.—May 10th, 1882. Child aged thirteen months, with marked and obstinate type of talipes varus of left foot, and equino varus of the right foot. (Fig. 4.)

Notwithstanding the advanced age of this child the treatment was commenced and pursued as in the case related, and in about eight months the feet were almost wholly reduced to a normal condition without any arrest of development. (Fig. 3.). The treatment was continued by the mother, under my direction, until the child was able to walk squarely upon the soles of his feet, which he did at the age of about twenty-four months, using both feet equally well. The subsequent treatment was by the attachment of a chain and rubber tubing to an eyelet in the sole of a common shoe, by means of hook and chain, about four inches long, fastened in a leather band encircling the leg, just

FIG. 3.

FIG. 4.

above the knee. (Fig. 5.) This simple contrivance was worn five or six months, when the cure was complete. (Fig. 6.) As soon as the child can stand squarely upon *the soles of the foot*, the cure may be considered almost complete, as the progress of growth and development, when thus fairly started, requires but little additional aid from art or science.

This case, however, selected from about forty others as a typical one, is especially worthy of attention on account of its aggravated type and the advanced age of the child (thirteen months) before treatment was commenced, and the length of time occupied in effecting the cure; showing clearly that if a cure can be accomplished in a child of the age of this one *without tenotomy*, it can, with increased certainty, be accomplished in a child when treatment is commenced soon after birth.

Attention is invited to a few important points deduced from this method of treatment, as follows:—

1. The opinion held by many writers of distinction, that this deformity is due to contraction of one set of muscles, or the paralysis of their opponents, and that, therefore, it cannot be cured without tenotomy, *is erroneous*.

2. The rule as formerly practiced, of waiting from six months to one year, and the rule as now generally followed, of waiting from six weeks to six months, before treatment

is commenced, thereby allowing the tendons and ligaments to become more firm and unyielding, and the deformity more marked, is a waste of the most valuable time for the cure of this malformation, and may make all the difference between a perfect and a *partial* cure.

3. That, where there is no lack of growth, the deformity can be completely and perfectly corrected, however aggravated, when taken at the early period of from two weeks to six months after birth, without tenotomy.

4. The time required for cure is not so long when the treatment is commenced soon after birth; and, besides, all the cumbersome paraphernalia of stiff shoes, steel splints, elastic bands and the like, may be dispensed with.

5. The simplicity and utility of this treatment is such that it need not be confined to a few expert surgeons, but every medical practitioner of ordinary competency is able

FIG. 5.

FIG. 6.

to practice it; and if the deformity is, as seems probable, due to defective innervation of the muscles or to motor paralysis, the adhesive plaster will act in a double capacity, by giving mechanical support and by stimulating the cutaneous surface.

6. This treatment is almost wholly devoid of suffering to the patient, is not subject to ulceration or sloughing of the integument, as is not unfrequently the case with other methods.

7. During the time of treatment by this method, the foot and limb are very little confined; and if the deformity is in one foot only, after the plasters and bandages are applied, the mother may have the satisfaction of dressing one foot the same as the other; or, if both feet are deformed, shoes or socks adapted to normal feet may be worn while the child is under treatment, and the deformity be concealed.

8. With this method the foot and limb are not confined so as to retard (as is usually the case with other mechanical appliances) *normal development while under treatment.*

The following paper was read :—

GALVANO-CAUTERY SOUND AND ITS APPLICATION, ESPECIALLY IN HYPERTROPHIED PROSTATE, WITH REPORT OF CASES.

GALVANO-CAUTÈRE SONDE ET SON APPLICATION, SPÉCIALEMENT DANS LA PROSTATE HYPERTROPHIÉE, AVEC RAPPORT DE CAS.

DIE GALVANO-CAUSTISCHE SONDE UND IHRE ANWENDUNG, BESONDERS BEI DER PROSTATAHYPERTROPHIE; MIT FÄLLEN.

BY ROBERT NEWMAN, M. D.,

Of New York.

The galvano-cautery sound, originally devised by me for treatment of the hypertrophied prostate gland, has also been applied to other localities, and proven beneficial in kindred maladies.

To show the benefit of its use, as originally intended, and to elicit some interest in the instrument, I will first report one successful case as a typical illustration.

CASE I.—HYPERTROPHY OF THE PROSTATE—CURE.

Dr. T. F. H., æt. 60 years, has been a regular practitioner in New Jersey for the last thirty-five years. He suffered greatly from enlarged prostate, frequent and painful micturition, and from cystitis. He was obliged to pass urine seven or eight times during the night. Seeking relief, he presented himself to me May 25th, 1886. Suffering by day and deprived of rest at night, he was no longer equal to the performance of his professional duties.

The history of the case shows that he had violent cystitis, as early as 1874, attended with painful micturition. His urine was dark and blended with pus and blood. He had a renewed and aggravated attack in 1879. The present attack has been marked since 1884, and authorities have made the diagnosis hypertrophy of the prostate, and declared there was no cure. One specialist recently said he had an enlarged prostate, cystitis and contracted urethra, and proposed prostotomy. At times he had retention of urine, and was obliged to draw it off with a catheter.

Examination.—Digital examination per rectum disclosed a hypertrophied prostate, enlarged in its entirety, with a preponderance of pars intermedia. There was a slight discharge from the urethra.

Examination of the urethra with the bougie à boule showed at 2¾ and 5¼ inches, respectively, from the meatus, two strictures, easily passed with a No. 18 French sound. At these points the walls of the urethra were somewhat indurated. The ejaculatory ducts were 7½ inches from the meatus; indicating that the galvano-caustic applications must be directed to that point as a central region. Otherwise, the patient was in perfect health.

May 26th. The fenestrum of the galvano-cautery sound (in which the platinum wire is) was brought against the hypertrophied prostate at 7½ inches from the meatus, then the cautery was applied by two instantaneous flashes. The operation occupied but a moment and was painless, the patient scarcely feeling it, and without discharge or loss of blood. The patient immediately left for home, by rail, eighty miles distant.

June 7th. The patient reports that he has suffered no inconvenience from last operation. Galvano-cautery repeated in same manner as on first operation, with same result.

June 21st. To make more room for the instrument and enlarge the calibre of the urethra, electrolysis was applied with an egg-shaped electrode (No. 23 French) as the negative pole, against the first stricture in the urethra. The positive sponge electrode was held in the palm of the hand. Only three milliampères were used for five minutes, when the electrode passed through both strictures and into the bladder.

Oct. 5th. A No. 23 French sound passed very easily through the entire length of the urethra, proving that during an interval of three and one-half months the calibre of the urethra had suffered no contraction.

Oct. 12th. Galvano-cautery to prostate at 7½ inches. Three effective flashes were given.

Oct. 19th. Patient feels better; has suffered no pain or unpleasant effects from the cautery, which was repeated to-day.

Nov. 9th, 15th and 22d. Galvano-cautery applied as before. Patient is better; the urine is now clear, without sediment; he voids water easier, and any residue is voluntarily passed a few minutes after. He has ceased to use the catheter.

Dec. 21st and 30th. Slight flashes of galvano-cautery to prostate at 7¾ inches.

Jan. 24th, 1887. Galvano-cautery applied to prostate at 8 inches.

March 9th. Stronger applications are made at 8 inches. Patient has no untoward symptoms or pain; but several days after the application passed with his urine, occasionally, small coagula of blood, without unpleasant feeling.

March 21st. Light applications at 7¾ inches. Digital examination disclosed that the gland has been reduced almost to its normal size, the left lobe only appearing a little large; the other side and the pars intermedia were normal.

May 18th. Patient reports continued improvement, and that he considers himself well; he passes urine voluntarily, easily, at regular intervals; sleeps undisturbed the whole night, and can retain the water for eight hours. The urine is clear, and there is no residuum. His general health is in every respect improved. He attends to a large country practice without fatigue and is in excellent spirits. Galvano-cautery applied at 8 inches, over left lobe.

May 30th. Application at 8 inches repeated.

June 14th and 28th. Galvano-cautery applications.

July 12th. A No. 24 sound passed easily into the bladder without irritation or inconvenience. The meatus urinarius is small and will admit no larger size; there is, however, no valid reason for cutting it. Prostate is normal. The patient is well.

In confirmation of this statement, and in order to have a full and unquestionable record, I subjoin the patient's letter.

———, WARREN COUNTY, N. J., July 11th, 1887.

"DEAR DOCTOR—I send you statement of my malady from its inception to present time.

"In June or July, 1874, I first noticed urinary trouble, frequent and painful micturition; urine mixed with pus and blood. The late Dr. Van Buren diagnosed my case as cystitis. Prognosis unfavorable. With care and suitable treatment, I almost fully recovered in a few weeks, though at intervals feeling more or less uneasiness, but at no time experiencing material uneasiness of the rectum. In 1879 an aggravated attack followed, with constant discharge of pus from the urethra, with all attending symptoms of cystitis, though with but little uneasiness of the rectum. Again with care and usual treatment of hip bath, etc., I measurably recovered.

"I had for some time suspected hypertrophied prostate, and in 1879 a surgeon, upon examination, had confirmed this view.

"In 1886, with all my previous troubles coming on, I consulted an acknowledged specialist, who diagnosed enlarged prostate with cystitis and contracted urethra, and

advised enlarging the meatus. Before determining to submit to the operation, I was looking over a report of the proceedings of the American Medical Association, held last year at St. Louis, and was struck with your report on treatment of hypertrophied prostate and cystitis by galvano-cautery. I consulted you and was encouraged to be so treated. I submitted at once. The application for the first few months was weekly and then bi-weekly. The impression is that the application is painful; on the contrary, it is painless.

"I now feel much improved.

"Not two weeks before you commenced treatment, I was assured of cystitis with enlarged prostate by a specialist, and told by him that I knew the result, and that he knew of no remedy for my trouble.

"What has been the effect of these applications upon the enlarged prostate, I can only judge of from my condition at the time of their commencement and at present. All symptoms have improved.

"Writers on the subject, after giving the treatment of the disease, reflect on the impotency of our art to combat it. What will be the outcome of the galvano-cautery the future must decide. Judging from my case, I feel we have at last a remedy by which we can assure our patients of a cure.

"After application I traveled over eighty miles by rail and foot without inconvenience. Micturition is without excitement or unusual sensation, and urine is clear.

"P. F. H."

Comments.—I submit this record as a typical case, so perfect in its simplicity that the facts must stand unquestioned and undisputed. My diagnosis of hypertrophy of the prostate had been confirmed by three surgeons (of New York and Philadelphia) of the highest standing as operators, teachers and authors, whose works are recommended by colleges as text-books, and are in almost every medical man's library. The patient is a physician, and gives, in writing, his diagnosis of the case, its treatment and results. He feels improved—nay, well; and is so jubilant over his cure, that he is ready, in person, to give any information about his case, even to submit to an examination to prove that his prostate is now normal. He testifies that treatment by galvano-cautery is painless and without inconvenience. He has had eighteen séances. The course of treatment was under unusual difficulties; the patient traveling before and after each operation eighty miles, therefore coming very irregularly, sometimes at intervals of more than three months. It is expected that better results will be obtained where the patient is near the operator's office and comes regularly every three or five days, as directed.

Whether or not a relapse will occur, and what the post-mortem appearance will be, the future will disclose. At present it must stand as a successful case.

THE GALVANO-CAUTERY SOUND

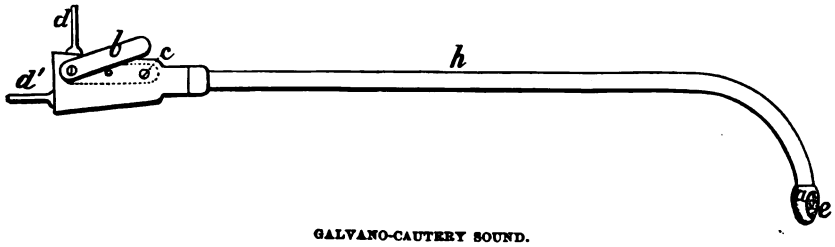
as devised by me, was first introduced to the profession at a meeting of the American Medical Association, held at St. Louis, when I had the pleasure of demonstrating its working at the Surgical Section, June 5th, 1886. Since that time I have constantly labored to improve the instrument.

For many years I have endeavored to apply galvano-cautery directly to narrow cavities, like the deep urethra, bladder, etc., but for want of a suitable instrument was unable. The stumbling block was, that instrument makers would not carry out my plans, declaring them impossible; or when my proposed instrument was nearly completed, the instrument maker improved according to his idea, producing a miscarried monster unrecognizable as my offspring, and better adapted for a dry-dock than for my purposes.

Many difficulties had to be surmounted.

The desired instrument was a catheter-shaped, smooth-surfaced sound, of small size, and so easy of introduction as to glide to the spot to be cauterized. Both poles thoroughly insulated, running side by side, without touching, must be placed within the small tube of this instrument, and the mechanism so arranged that the platinum burner could be heated to the desired degree instantaneously, with certainty, and beyond possibility of failure. The cautery, its beginning, duration and ending, as well as the quality used, must be under the absolute control of the operator. The platinum must not, in its entire length, touch anything; the heat must be concentrated, and not approach the surrounding parts of the instrument; the connections must be perfect and act promptly. The instrument must be light, small, handy, and have the correct curve, and so arranged that the operator can manage the entire procedure without an assistant. The entire mechanism must be placed within the limited space of a No. 18 French scale-sized tube. Next we must have a battery, so constructed as to give a certain quantity of electricity of a fixed potency, suited to the work to be done and the instrument; too high a potency will melt the platinum wire or cut the tissues like a razor; on the other hand, too low potency will fail to heat the wires or not be effective. Therefore, it is imperative to adjust the electricity necessary for our work and instrument, for the same quantity of electricity under the same circumstances will always do the same work.

It is not practicable to measure the heat for galvano-cautery, or say how many degrees we need. Sufficient electricity, and no more, must be generated, to heat the platinum burner to the desired degree. According to the length and size of the burner this heat must be adjusted; a fluid battery changes it almost every instant by polarization. The fulfillment of these requirements required constant hard work, drawings, models, trials, vexations and experiments with mechanics; and while the instrument shown in St. Louis worked well, it needed many improvements. Now I am pleased to exhibit it to you altered and improved.



GALVANO-CAUTERY SOUND.

a, Fenestrum; inside of which is the burner, *e*, a platinum wire; *h*, tube; *d* and *d'*, pins to be connected with the electrode cords; *b*, current breaker; *c*, screw, to be connected with current breaker *b*.

The instrument is catheter-shaped, of smooth, polished metal, with a short curve at one end, at this end is a fenestrum (*a*) in which is placed a platinum wire (*e*)—the burner to be heated. A serpentine form is best for this wire; each end is firmly fastened to one of the two copper rods inside the tube (*h*) and represent, respectively, the positive and negative poles. The other end of the instrument is straight and forms the handle, in which commence the above-mentioned copper rods, each of which is fastened to one of the pins or heat conductors (*d*) and (*d'*). These two pins are connected to the electrode cords by binding screws. The other ends of the two electrode cords are fastened respectively to the positive and negative poles of the battery. The current breaker (*b*) is movable, and when set straight and pressed firmly down on the screw (*c*) electricity is evolved and the burner (*e*) instantaneously heated.

The recent improvements consist in (1), having the handle in a light, easily managed, convenient piece ; (2) having the current breaker under the immediate control of the index finger ; (3) having the fenestrum filled up, whereby the instrument is more thoroughly insulated and less liable to become heated ; (4) having the tube filled up, thus preventing it from getting wet or blocked with débris inside.

THE BATTERY.

The instrument may be heated by different machinery. Any good galvano-cautery battery may be used with the instrument, but it is necessary, as before stated, to so regulate the battery that it yields the exact electrical potency to be used for the operation. I use a Dawson battery, which works to my entire satisfaction. Experiments are necessary to establish the standard.

The heat must be of a high red color, just short of white heat. The instant the current breaker is touched, this heat must be kept while the wire is in contact with the mucous lining. Less electricity is required to heat a free wire in dry air than to heat a wire held against a moist surface. The strength of the fluid is adjusted according to these requirements ; the elements are immersed in the fluid to a certain depth, the electrode wires are regulated with regard to their size, length, etc.

Having determined these requirements for the operation, there will be no further trouble. It is a certainty that, in the near future, every scientific instrument maker will construct his apparatus with the graduated measure needed, attached and regulated, so that the operator can use any measure desired. We can also use the storage battery, consisting of a series of cells, which answers our purpose as well as for electrical illumination. This battery, once adjusted with reference to the quantity needed, works with equal power and steadiness till the stored electricity is exhausted. The last of its electricity has the same effect as the first. This instrument, though portable, is rather heavy. We may use the dynamo machine, which can be operated by hand, foot, hydraulic pressure or steam according to construction and desire. The machine above described, I have seen in St. Louis at the store of Mr. A. S. Aloe (corner Fourth and Olive streets).

I repeat, no matter what kind of a machine is used, a fixed measure of electricity is necessary. You will see its action in some of the experiments ; beginning with one flash of light, to be followed by several quick flashes. If the wire is heated slowly, becoming warm and gradually hotter, till the desired heat is obtained, it shows that the instrument is faulty in its construction, consequently must be imperfect in its results. In experimenting with the instrument on mucous linings, we find that a galvano-caustic application of the same power acts differently according to the length of contact with the tissues. Thus the effects can be regulated from a light blush to the total destruction, or even amputation of the tissues.

It is a misconceived idea of many, that the galvano-cautery necessarily burns, destroys, and is followed by cicatricial tissues. Nevertheless, this is a favorite objection of some ignorant persons and enemies of electricity. If the operator bungles, or wishes to destroy, he can, but the expert will not. It is well known that eminent neurologists apply galvano-caustic directly to the faces of young ladies, without ever causing marks. All depends upon the manner of application. Even deeper applications on mucous linings may cauterize without destroying. Voltolini, Carl Michel, Shurly, and Yeamans of Detroit, and many others, have applied the cautery to the nasal and pharyngeal cavities with great success. Therefore it is evident that different methods can be instituted with the instrument, and applied for various purposes to different parts.

Each of these batteries has its advantages and disadvantages. At present none is perfect. The operator must adjust the quantity he needs to use, in each instance.

We will now demonstrate the practical workings of the instrument by experiments.

Please note the instantaneous heating of the wire, which I show, holding the instrument free in the air, by making—

First. One short flash.

Second. One long flash.

Third. Several flashes in succession.

The physiological effect on mucous lining direct, you can see by the specimens. No. 1, is made by one flash. No. 2, is made by several successive flashes. No. 3, by longer contact or deeper cauterization. No. 4, by still stronger application, for destruction of tissue.

HYPERTROPHY OF THE PROSTATE.

We will now consider the application to the enlarged prostate, as the instrument was devised mainly for that purpose. I omit any consideration of the anatomy, physiological relation, pathology, etc., of the prostate in health and disease, as it is well known to you all, and not within the scope of this article, though of late valuable additions have been made by Mr. Reginald Harrison (*Liverpool Med.-Chirurg. Journal*, July, 1885), and by A. H. Wilson, M. D., of Boston, at the last meeting of the American Medical Association (not yet published). It is sufficient to say, that hypertrophy of the prostate is of frequent occurrence, causing much suffering and death. While there are some isolated happy results by treatment, it must be admitted that no satisfactory treatment for the cure has been established. Hence this field in surgery needs improvement.

I have proposed, in the treatment of prostate enlargement, three methods of galvano-cautery—

(1) The regular (slow) method by the galvano-cautery sound.

(2) The rapid method in one séance.

(3) The operation for radical cure by the removal of the hypertrophy.

I. THE REGULAR (SLOW) METHOD BY THE GALVANO-CAUTERY SOUND

will principally occupy our attention. This address is chiefly to bring its utility to your attention.

I prefer and recommend this treatment, as it has done good service. It consists in giving to the substance of the enlarged gland a short application, from an instant to a few seconds in duration ; this produces not more than a white film similar to the effect of nitrate of silver, in the treatment by Desormeaux.

Modus Operandi.—The galvano-cautery sound is connected with the two electrode cords, which are then attached to the two binding posts of the battery, each respectively to one pole, or, in case of the Dawson battery, to the zinc and platinum poles. The fluid in the cells must be of the right standard, and all the machinery in perfect order. When all is in readiness, I invariably let the elements down and try my instrument with a short flash. No matter what assurance I have of the perfection of the appliances, this little precautional trial excludes any failure. The prostatic portion to which the cautery is to be applied must have been ascertained, and the distance from the meatus measured. This distance is then marked on the instrument by a small rubber band. The patient, according to his preference, may stand erect, be on an operating table, or in bed. The instrument is then introduced so that the fenestrum with its platinum wire is in contact with the part to be cauterized. The operator will know by touch when the instrument is in the right place, and the measure will corroborate the correctness of the situation. One hand holds the instrument in this place firmly, while the other hand sets the battery in motion, and then the current breaker (*b*) is placed in a straight line and pressed firmly upon the screw (*c*), a flash follows, and the raising of the finger from (*b*) disconnects the current. In one moment the operation is finished and the instrument withdrawn. It causes no pain, and in some instances the patient

scarcely believes that anything has been done. He is able to walk about and is not detained from his business. In cases of very sensitive patients, I have used cocaine injections, but it was scarcely necessary. The séance should be repeated in about three days, or even in two. The instrument must be kept scrupulously clean, as the cautery will fail if there is dirt between the connections.

The question now arises, How does this method bring about a cure? The end sought is, first to remove the obstruction, so that the bladder can discharge all the urine, and at regular intervals; and then, to reduce the prostate to its normal size. The theory is that the cautery first acts as a tonic, and next as an astringent; the mucous lining shrivels up, the glandular tissue contracts, and by shrinkage the size is diminished. The stimulation gives new life and healthy action. Each repetition of the operation acts similarly, and perhaps on another part of the hypertrophy. The operation must be continued till the cure is effected. Care must be taken not to over-stimulate, and cause prostatorrhœa, prostatitis, etc., thereby creating or aggravating the very ailment we seek to cure. The cautery must be given just severely enough to accomplish the object and no more. If the cauterization is too prolonged and too deep, the glandular action is overtaxed and weakened, and will be followed by a terrible prostatorrhœa, which takes a long time to cure. At the same time an inflammation is created, which causes pain and swelling, and at last, the too greatly cauterized tissue will slough away and may cause septicæmia.

For these reasons, I prefer the slow method described, and am opposed to rapid methods, or too deep cauterizations.

The practical workings I judge by analogy, from observations of the cautery in hypertrophied tonsils. Great similarity of structure exists between the tonsils and the prostatic gland—both are glandular organs, covered by mucous lining, having epithelium; both are secretory organs, having ducts, follicles, canals, and one twelve to fifteen orifices, and the other twelve to twenty small excretory ducts. In hypertrophy of the tonsils, after other remedies had failed, I succeeded by using galvano-cautery with this instrument. The application was made in the cases of children of very tender age, who stood free before me, and without any aid or force opened the mouth, and went through the operation without flinching. Not one complained of pain, all came back to have the cautery repeated, and then stood still with more confidence than at first. In these cases the immediate effect was a splendid illumination of the whole buccal cavity, and a white film was seen on the tonsil after withdrawing the instrument. The cautery was repeated in two or three days; in one case the next day. Sometimes the cautery was repeated in the same place, at other times, from preference, an adjoining place on the tonsils was selected. Almost daily observations of this series of cases convinced me that the galvano-cautery acted practically just as I theoretically described. The patients were benefited, the tonsils diminished in size, and a cure effected. It was remarkable how soon the mucous lining regained its normal color, and when a deeper cauterization was used, there was no unpleasant slough, only a patch was observable, like in appearance to follicular tonsillitis. The instrument was well adapted for the tonsils, the curve suited exactly, the fenestrum could be held against the exact place to be cauterized, without possibility of accidentally burning any other part.

In treatment of the enlarged prostate by galvano-cautery, it is absolutely necessary to pay attention to other symptoms and troubles of the patient, according to established principles. Pain must at all hazards be allayed; this I generally accomplish by rectal suppositories. Chas. Mitchell, of Philadelphia, prepared for me some gelatine articles, which act very well. In medication I rely mostly on belladonna. The bowels must be kept regular, as constipation adds considerably to the inflammation, and by pressure causes pain. While the galvano-cautery is used, it is of the greatest importance to attend to the state of the bladder, by drawing off the urine and washing the bladder out.

This treatment is indicated in all cases of enlarged prostate, where urgent necessity for *immediate* relief does not exist, and particularly, in such cases, where the patient is perambulant. It is useless if the patient is in the last stages of albuminuria, where uræmic poisoning may carry him off at any moment. The earlier the treatment is instituted, the better results may be expected. Will this treatment cure always and in all cases? No.

From present experience, our treatment will improve and cure only when the hypertrophy has not been allowed to become too great and other complicating troubles have not advanced to a dangerous condition. It is impossible to state the exact size of the enlargement which can always be cured, or the limit of the enlargement beyond which this treatment would be useless. The general state of the constitution and health, habits, business relations, temperament, the home and its surroundings and attention to details of orders in treatment, all have an influence on the progress of the treatment and its results. The time needed to effect an improvement, or a cure, depends principally on the size of the enlargement and frequency of the applications, also on the habits of the patients.

The aim of this treatment is to remove the obstruction to a free discharge of urine, thus relieve the bladder from distention, inflammation and degeneration; finally, to reduce the hypertrophy of the prostate, thereby preventing a recurrence of this painful bladder trouble and its consequences.

Theories are subject to doubt and discussion. I have given not only a theory, but have shown that a desired object has been accomplished, that hypertrophy has been reduced and bladder troubles terminated. I am sustained by patients who testify to the efficacy of the galvano-cautery sound and by the report of cases.

In conclusion, I again deny that the treatment, properly administered, will so cauterize or burn as to destroy tissues. But slight flashes, which are harmless, must be used frequently and carefully, to effect the object contemplated.

For which reasons I practice and recommend this first method.

The question may be raised, whether or not good results in hypertrophy of the prostate cannot be obtained by electrolysis rather than by my method of galvano-cautery. Electrolysis has been used and some cases reported with favorable results. I have tried electrolysis in various ways. Sometimes progress was made; but I never could establish a method which cured, or could be recommended. In my report of "Electrolysis in Surgery" (*Journal American Medical Association*, April 25th, 1885, page 452) I have mentioned the subject. The drawback appears to be, that the application to the prostate acts too quickly as an over stimulant, causing greater inflammation, instead of diminishing the hypertrophy. While I am writing this, a favorable result in one case comes to my notice. It is reported by Dr. Bryce, the genial editor of *Southern Clinic* (*Southern Clinic*, August, 1887, page 232). The report is graphic and concise, showing the suffering and misery in this malady; corollary, the necessity of a certain method of benefit to such patients. For these reasons I quote Dr. Bryce's case in his own words:

"The subject to whom I refer was an old gentleman, 74 years of age, of fairly good general health, and whose circumstances in life no longer necessitated any exposure or business worry. For ten years he had suffered with severe cystitis, due to the enlargement of the prostate. His attacks of pain, worry, burning and general discomfort would come and go, but never left him entirely, and when he came under my care he had gradually reached a state of complete wretchedness. His urine was voided every half hour, with sudden painful spasms of the bladder, in small quantities, burning like fire and loaded with fetid pus. His general health had suffered terribly, with loss of sleep and constant pain by day; his life had become a burden to him. Utterly unable to control his frequent desire to urinate, he wore an urinal constantly, in order that he might be able

to leave his house and return in any condition of comfort or cleanliness. In this condition he sought our assistance, and with a hope of palliating the trouble we undertook the case.

"Hygienic conditions were observed, local treatment by counter-irritation, attention to the evacuation of the rectum, general remedies, and everything I could think of was given a fair and honest trial. The condition of the urine was treated, the bladder irrigated, the catheter used, but all to no purpose: my poor old friend went from bad to worse. I finally told his family that I could do but little more for him and suggested the last remedy which surgery offers these unfortunates, viz., to open the bladder and give it rest, as in the case of lithotomy. But before resorting to this, I proposed the use of galvanism to the prostate through the urethra. The patient and family readily consented to anything I proposed that could possibly offer even temporary relief. Inviting my friend, Dr. R. A. Lewis, to assist me, I passed an insulated sound down to and upon the prostate, leaving a certain metallic portion in close proximity to the hypertrophied gland. I then used a current of eight cells (McIntosh) for five or ten minutes, when the old gentleman became very faint, and I withdrew the electrode and put him to bed.

"In a few hours he had a violent urethral chill, which lasted an hour or more, and was followed by a fever, which lasted for twelve hours. After this a sudden and marked improvement set in, all his symptoms were better, and he enjoyed relief he had not obtained for years. At the end of one month his trouble began to return. I again used galvanism, as before, which was followed by chill and fever, with steady improvement, and I have never given him a dose of medicine or used anything else for him since. His trouble is gone, and has been gone for a year; he has thrown away his urinal, has grown fat, is healthy, hale and happy, and I believe is cured, for the time at least, by two applications of galvanism."

While I have had in some cases good results, similar to Dr. Bryce's, I have failed in others, and in some the galvanism has made matters worse. One difficulty is to regulate the current to each case. Then these cures are isolated and do not definitely establish anything. Besides, fainting, chills, fever, etc., in old men, are not pleasant after symptoms.

My method of treatment of hypertrophied prostate with the galvano-cautery sound, I prefer for the following reasons: It has benefited or cured all cases under observation; a reasonable time has passed without any relapse; it has caused no pain, no detention from business or pleasure; no untoward after symptoms or circumstances have ever occurred.

Though the first case reported in this article is a typical case, I will mention briefly a few other cases.

HYPERTROPHY OF PROSTATE, STRICTURE URETHRÆ, CYSTITIS.

CASE II.—B. S. J., æt. 63 years, New York, a business man, came to be treated for stricture of the urethra. His family physician was unable to pass any instrument through his urethra, and after many weeks of hard work daily, gave it up.

December, 1885. The patient came under my observation suffering from the effects of the stricture, cystitis and an enlarged prostate. He could not pass any stream of water; it only dribbled away; sometimes with severe pain and tinged with blood and pus. He was treated for the stricture, first, until the calibre of the urethra was enlarged sufficiently for the introduction of the galvano-cautery sound.

March, 1886. Before the galvano-cautery sound reached the prostate region, at 6½ inches from the meatus a sore, congested place was found, which caused an unpleasant hemorrhage on the slightest touch. At short intervals two applications of the cautery were made, each by a long flash.

April 29th. The last application was so successful that the slightest hemorrhage has not since occurred, even on the passage of the instrument. Patient felt well and went South.

1887, February. Patient has felt comparatively well, and desires to be treated for enlarged prostate.

From February till June 1st, six galvano-cautery applications were given to the prostate at 7½ to 8 inches from the meatus. Each séance consisted of two light flashes, one toward the right, the other toward the left of the prostate.

June. The hypertrophy has been reduced, the strictures have not reappeared; he emits normal clear urine, in a good steady stream, without any trouble. He considers himself well, and left for the country.

CASE III.—J. C. A., æt. 66 years, Vermont.

June 4th, 1886. Had a bad stricture, nearly fifty years old. Found on digital examination per rectum a general enlargement of the prostate. The stricture was four inches from the meatus, and admitted only a No. 12 bougie, French scale. The stricture was treated successfully by electrolysis till a No. 28 French was admitted.

The prostate was treated by the galvano-cautery sound (as above described) with light flashes. Only three séances were held during the month of July, when the patient felt so much improved that he discontinued further treatment, because he considered himself sufficiently comfortable, and the trips between his home and my office were too troublesome for him.

CYSTITIS-HYPERTROPHY.

CASE IV.—B. H. G., æt. 63 years, New York City.

Has an aggravated cystitis, with violent spasms and pain in consequence. The prostate is very moderately enlarged. In this case it is hard to decide whether the prostate hypertrophy was the cause or the result of the cystitis. He had been treated by two physicians for strictures, who had done their work so well, that on examination I found a healthy urethra of good size. But the pars intermedia was pushed upward by its hypertrophy, so that the whole urethra was thereby elongated, and it took nine inches to reach that part with the galvano-cautery burner.

August, 1887. One month passed in treating the cystitis, particularly in allaying the spasm, washing out and dilating the bladder.

During September, four galvano-cautery applications finished the treatment, and the patient left apparently well.

CASE V.—W. S. S., æt. 61 years, New York.

During four years has been unable to void urine voluntarily, and had to use the catheter frequently, on account of constant desire to micturate, accompanied with pain. He had been under treatment for an enlarged prostate with the usual result. The urine is loaded with pus and mucus, appearing in thick, ropy masses, and sometimes blocking up the catheter, causing more spasm and pain.

October 13th, 1886. On digital examination found the prostate hypertrophy general and of the size of a hen's egg. The patient suffers greatly and his general condition is pitiful. He has lost flesh, is very anæmic, in constant pain, unable to do any work, scarcely able to move, and his countenance indicating approaching dissolution. There is no stricture; the urethra is of good size; but a metallic catheter meets the obstructing prostate, and is grasped tightly by the bladder, so that washing out the bladder is almost impossible. The history of this case shows that hypertrophy of the prostate was the primary cause of the obstruction, using the catheter acted as an irritant, and was followed by severe cystitis with spasm and loss of power of micturition.

October 18th. Soft rubber catheter does a little better, and was used in washing out the bladder.

During the balance of October and in November systematic washing out of the bladder was continued, the spasms allayed, and by degrees the bladder was dilated by injecting hot water, sometimes medicated, as a sedative.

October 19th. Patient is improved, the bladder is quiet, without spasms or pain. The urine contains no sediment and has assumed a normal color. He can hold the urine two and a half hours, when the use of the catheter is necessary. He feels easier; the bladder tolerates one-half pint of urine. Bowels are better, but still act sluggishly; appetite is improved.

November 24th. Galvano-cautery sound applications were made, at 8¾ inches from the meatus, to a part of the hypertrophy; a few slight flashes were given.

November 30th. Galvano-cautery repeated.

December. Electricity was applied externally faradic as well as galvanic. Patient is much improved, no pain in bladder, no contractions, but a sensation that water should be voided when he uses the catheter. Urine is now clear, without any sediment.

1887, January. Five galvano-cautery applications were given during this month, generally two flashes, one to the right and the other to the left side, without pain or loss of blood. Patient is much improved. The prostatic enlargement has materially decreased. The sound passes very easily, without impediment, causing no pain or inconvenience to the patient.

March. The galvano-cautery flashes have been continued at intervals of two weeks with good success. Once with stools a natural micturition occurred, and almost one and a half ounces of urine passed in a good stream per urethram, for the first time in four and a half years.

I continued treatment in the same manner.

Urine now passes regularly with stools, but bladder will not always act at will.

June. Passage of the urethra is free, sound passes easily.

July. The hypertrophy is so reduced that it is scarcely felt. In fact, it may be considered cured. The treatment is continued only by external electric applications, with a desire to regain voluntary action of the bladder, which has been inactive for nearly five years.

The patient has received twenty-four galvano-cautery applications.

CASE VI.—W. F. B., æt. 59 years, of Brooklyn, was brought to me by Dr. Russell, to be treated for stricture. On examination with bougie à boule a stricture was found, but on subsequent electrolysis, while the electrode passed that stricture, it would not enter the bladder, nor would any other small instrument.

1886, December 14th. On examination, the prostate was found to be greatly hypertrophied. Patient feels uneasy, and has a desire to micturate frequently. Galvano-cautery was applied to the prostate, and repeated on December 28th.

1887, January. Prostatic hypertrophy has diminished, patient feels much better. All the former obstructions were due to the enlarged prostate, and now a sound, No. 28, passes easily. Galvano-cautery repeated at 7¾ inches from meatus.

Patient went West.

April. On returning from the West reports great improvement, feels almost well.

During April and May four more galvano-cauteries were made and patient dismissed, being entirely well. Seven applications had been made.

CASE VII.—D. H. M., æt. 75 years, Westchester County, N. Y.

1886, December 1st. So much troubled with a desire to micturate frequently that he is obliged to empty the bladder four or five times during the night.

While there is a constant desire to urinate, he feels an obstruction, and oftentimes the water only dribbles away. There is sometimes hæmaturia, and the urine is always

loaded with pus and mucus. On examination found hypertrophy of the prostate, more enlarged in the lateral lobes. He also complains of hemorrhoids and costiveness.

Rectal suppositories were ordered, of iodide of potassium, hyoscyamus and nux vomica.

Five galvano-cautery applications were made direct to the prostate, once a week for five successive weeks, which caused neither pain nor uneasiness.

The old gentleman traveled, each time a good distance on railroad, and five miles in wagon, in very inclement weather, during the winter.

He made a good recovery, the prostate was diminished in size, and he discontinued further treatment; feeling well.

I did not consider it a perfect cure, but learned in August from his family physician that he has continued well.

I have used the galvano-cautery sound with perfect satisfaction since October, 1885. It has answered its purpose, and never caused pain or uneasiness, and no untoward after symptoms ever occurred when it was applied with care.

The cases are all so similar that the foregoing will suffice.

The galvano-cautery sound has also been employed in other diseases, with good results.

I mention briefly some of its uses, without record of cases.

SPERMATORRHŒA.

This disease is rare, but one genuine case was treated by applying the cautery to the ejaculatory ducts at 6¾ inches from the meatus. The application was repeated once a week.

IMPOTENCE.

In several cases the galvano-cautery was applied to different places, such as prostate, Cowper's glands and ejaculatory ducts. The treatment was aided by other means.

IN DISEASES OF THE BLADDER

the instrument has worked admirably, particularly in villous tumor with hæmaturia, and in traumatic ulcer of the bladder. The patient had been injured, and a ragged wound near the neck of the bladder was transformed into a chronic ulcer. The place could be felt by the introduction of the instrument, the patient himself could give the best information when any instrument came in contact with the ulcer. One patient, weakened by constant hæmaturia for years, passed no bloody urine after the first application of the cautery.

URETHRAL GRANULATIONS, DENUDED SURFACES, AND ULCERS

readily yield to the galvano-caustic treatment. Frequently patients present themselves to be treated for a chronic discharge, some call it leakage. I consider it error to assert that all chronic discharges of the urethra spring from strictures. On the contrary, I often find that when strictures are radically cured, the old troublesome discharge remains.

For twenty years I have treated such cases by local applications through the endoscope. Generally we find chronic granulations, which yield to local circumscribed applications of nitrate of silver, repeated at intervals. Sometimes we find denuded surfaces, which bleed at touch, sometimes chronic congestions, and even ulcers. With these affections the galvano-cautery has done better and cured quicker than the old method. The endoscope is needed to diagnosticate and locate the diseased spot.

II. THE RAPID METHOD BY THE GALVANO-CAUTERY IN ONE SÉANCE

is plausible, but at the present time not practicable. It consists in passing an instrument by galvano-cautery through the obstructing prostate and establishing a new

free passage in one séance. Prof. Bottini, of Padua, practiced a similar operation with success. He constructed his own instrument, and used galvano-cautery against the offending portion of the prostate for forty-five seconds.

The patient was kept in bed afterward, and on the twenty-fourth day, for the first time, passed water voluntarily. It took six months before he was cured. While I admire the zeal of Bottini, I scarcely think this method will become popular. His instrument is very clumsy, unhandy, and needs so large a galvano-cautery battery as to be too heavy to be moved about. His instrument is shaped like Heurteloup's lithotrite, without any curve, the end only having a short coudée, like Desormeaux's fenestrated male endoscopic tube. Such an instrument is exceedingly difficult to introduce, and in many cases of hypertrophy unintroducible. The intention is to push this instrument into the bladder, over and beyond the enlargement of the prostate, then to reverse it inside, so that the beak is turned downward. The galvano-cautery knife is (a la caché) inside the bëak, and moves outward by turning a dial on the handle, while the battery heats it, thereby making a central cut and division in the obstructing prostate. While the idea of the operation is excellent, I cannot approve of the instrument.

There are many objections to this method. It is a very severe and uncertain operation, with an immediate shock, followed by pain, much suffering and inflammation, which may cause a new obstruction, partly by spasm of the bladder and partly by the débris of the destroyed tissue, which may also cause a septicæmia. If some of my friends object to this method, saying that it is effected by burning a hole through the tissues, they have a good cause for discussion. It is a dangerous and uncertain operation, and at best the patient is kept in bed for a long time, in pain and anxiety.

At the commencement of my remarks, under the second division of my subject, it was stated that this operation is plausible. I have devised an instrument for performing it. But I have been disappointed by instrument makers in the satisfactory presentation of my idea. The perfection of the instrument belongs to the future.

III. THE OPERATION FOR RADICAL CURE BY GALVANO-CAUTERY.

This consists in the removal of the hypertrophy by galvano-cautery *in situ*, and in one operation, access being gained by either perineal section or laparotomy.

This removal may be partial or entire.

It can be done with the galvano-cautery burner or wire sling.

This operation is indicated, in fact we may say peremptorily demanded, when the patient is in immediate danger of succumbing, and no time is left for a slower method of procedure. This state has arrived when the hypertrophy causes absolute retention of urine, and there is no possibility of gaining an entrance through the obstruction so as to evacuate the bladder. Complications have generally taken place and the fatal end is within a few hours, either by rupture of the bladder or uræmia, as even aspiration of the bladder would only give temporary relief.

My radical operation, proposed for such a state, is not free from danger, but, as the patient without such severe means will succumb in a short time, the operation cannot decrease his chances, on the contrary, can only increase them.

Besides, through recent inventions and improvements in like surgical cases, and antiseptic precautions, the mortality has been reduced to a very small percentage, so that such patients have fair chances of a full recovery.

The rationale of the operation proposed will be seen better by a retrospective of recent doings in this line of surgery. Different operations have been reported, successful cases, by excision of the hypertrophied prostate with the knife after perineal section. As such reports are on record and well known, I cite but one case, which I give in detail.

CASE VIII.—HYPERTROPHY OF THE PROSTATE—SUPRA-PUBIC OPERATION—GALVANO-CAUTERY—PERINEAL SECTION—PROSTATOTOMY—RECOVERY.

H. M., æt. 66, English, widower, has suffered from an enlarged prostate for ten years past, which gradually prevented voluntary micturition. Three years ago he began to use a catheter. One year ago the catheter broke, and part was left in the urethra. All efforts to remove this part failed. He went to the Hahnemann Hospital in New York, where the piece of the catheter was removed by the supra-pubic operation on May 20th, 1885. The broken piece had remained in the bladder two weeks. A catheter was left in the opening of the wound through the abdomen, as an outlet for the urine. This outlet was left open, so that the urine was emptied through a continuous rubber tube into a vessel as soon as it entered the bladder, leaving the bladder in a constant collapse.

Feb. 11th, 1886. I saw him for the first time. He was in a tolerable condition, in bed at his home in Brooklyn, with his harness of rubber tubes and catheter in abdomen attached. He was troubled very much with spasm of the bladder. A metal catheter, No. 21 French, passed per urethram into the bladder with less difficulty than expected. The water injected into opening passed out through the other outlet pretty clear, and in a full stream. Only spasm of the bladder troubled him. Such water will run well at eight inches from meatus. Urine contains pus, some phosphatic deposits, and is cloudy. Examination per rectum found a slightly hypertrophied prostate. Urethral suppositories were ordered, containing ext. belladonnæ and opii. Bowels are nearly regular.

Feb. 12th. Bladder was washed out and gradually dilated. It would not tolerate more than 1½ ounces of urine. The urine was allowed to accumulate in the bladder by stopping the drainage tube with a cork ; when enough had accumulated, it was let out. During the night the water was let off four or five times, between which the patient enjoyed a good sleep.

Feb. 13th. Had some voluntary micturition per urethram. This is the first voluntary micturition in a year. Can hold in bladder 3½ ounces. Catheter No. 23 passed easily over prostate into bladder. Urine ran out at eight inches, but at 7¾ the water stops.

Feb. 16th. Galvano-cautery applications were made to the prostate at different places ; four flashes were given without pain.

Feb. 18th. There was improvement. Endoscope was used and showed an almost healthy bladder.

Feb. 20th. Galvano-cautery to prostate—three prolonged flashes. Patient felt the cautery but no pain or inconvenience therefrom

March 2d. Had consultation with Dr. Hutchison, who approves the treatment and maps out the boundaries of an enlarged prostate. Galvano-cautery repeated.

March 9th and 13th. Two more galvano-cautery applications to prostate.

Patient discontinued the treatment. He was improving steadily, and know of no reason for the discontinuance.

April 3d. He went to Brooklyn Hospital, where Dr. Hutchison performed perineal section and prostatotomy. Patient was on the operating table about one-half hour. Two hours after operation began to bleed quite freely and was in great pain, and required morphia.

April 4th. Passed a very restless night, having a great many spasmodic contractions of the bladder. Bladder washed with borax solution and a large rubber tube inserted, but it does not seem to work very well, much of the urine coming through the old opening.

April 5th. Still continues to have very violent contractions, occurring every half hour, and not controlled by morphia. Temperature, 103°. Appetite poor.

April 6th. Spasms still continue; tried silver catheter but it worked no better. General condition not so good as yesterday.

April 7th. Continues in about the same condition. Had a tube made especially for him; it seems to work much better than anything used before. Spasms less frequent.

April 8th. Slight improvement, but spasms still frequent and patient very weak.

April 10th. Not much change; spasms continue. Washed out bladder with solution sodi bicarb., ʒ ij–Oj.

April 12th. Continues about the same; gave tinct. hyoscyami in aq. camphoræ. Urine somewhat increased.

April 14th. No change for the better, growing weaker if anything.

April 16th. Continues in same condition.

April 19th. Patient much worse; delirious during night, face flushed, perspires very freely, pulse weak and rapid; stopped use of hyoscyamus.

April 20th. Not quite so delirious; spasms less frequent, but general condition not so good.

April 22d. Seems improved in every way.

April 24th. Continues to improve; spasms very infrequent and not severe. Most of the urine passes through tube.

April 26th. Continues to improve.

May 4th–7th. Improving.

May 10th. Allowed to sit up for a short while.

May 20th. The abdominal fistula has opened again by suppuration and the urine is discharged freely from it. The spasms are less in frequency and severity. Given suppository of iodoform every night.

May 24th. Removed tube altogether, and allowed urine to drain away through the lower opening. Spasms less and patient feels much better.

June 1st. Commenced to-day to draw the urine every two or three hours through the lower opening. Suppositories continued.

June 2d. The above plan works admirably; patient retains all the urine and nothing drains away, either above or below. The catheter has to be passed every two and a-half to three hours. Suppositories continued. Patient takes but little opium now, perhaps ♏xx deod. tinct. during the night.

June 4th. The improvement continues. No urine has passed through the upper opening since the last plan was adopted. The urine which is drawn is very clear, and the cystitis has entirely subsided. The spasms have left altogether and patient feels markedly improved in mind and body.

June 5th. Patient was up to-day, for the first time, and feels first-rate in every way.

From this time patient continued to improve steadily. Passed urine through the natural channels on June 17th, without inconvenience, some escaping through the perineal incision. July 1st, the wound had entirely closed. He remained here until July 13th, when he took his discharge.

For the above record from the Brooklyn Hospital, I am indebted to Dr. Raynor, the house surgeon of the hospital. August 19th, 1887, Dr. Bierwith, who assisted Dr. J. C. Hutchison in the operation, kindly informed me that it consisted in external median prostatotomy. The prostate was incised in the median line, but no portion of it was removed. The patient wore a large silver tube for several weeks, until the upper opening left from the supra-pubic operation was closed.

I have further information up to date from the son of the patient, as well as from his family physician, that he has enjoyed pretty good health since.

This case illustrates three points: (1) That prostatotomy by perineal section was a

success, the patient remaining well; (2) That my first method of treatment by the galvano-cautery sound is a success, and devoid of any danger, as was proven by the subsequent operation, and (3) That the supra-pubic operation is not dangerous, as was formerly believed.

Innovations and improvements in these methods have been practiced recently. To illustrate this progress, I can do no better than to follow Dr. Belfield's cases.

In a paper read before the surgical section of the American Medical Association, held in St. Louis, 1886 (Digital Exploration of the Bladder, etc., including two prostatotomies, by W. T. Belfield, M. D., of Chicago, *Journal American Medical Association*, Sept. 4th, 1886), one case commends itself to our attention.

CASE IX.—In hypertrophy of the prostate, etc., perineal section was performed. Two weeks later, the perineal wound was opened, and a channel made by galvano-cautery through the prostate, sufficiently large to admit a lead pencil. The patient made a good recovery. Seven months later the patient died, of acute uræmia. Autopsy showed contracted kidneys, and the specimen of bladder and prostate was presented at that meeting.

Later, in another case, Dr. Belfield removed a prostatic middle lobe (*Journal of American Medical Association*, March 12th, 1887, p. 303), a hypertrophy, by supra-pubic prostatotomy. Recovery was uninterrupted, the fistula closing entirely on the seventeenth day. Patient has since urinated freely without a catheter; cystitis has subsided. Patient was 73 years old.

The next question to be considered is, which operation is better for our purpose, perineal section or the supra-pubic. Several contributions, considering these points, have lately been added to our literature; and the latter operation has been much improved and simplified. According to Dr. Dennis' statistics (Exploration of the Bladder by the supra-pubic method, by F. S. Dennis, M. D., *Journal American Medical Association*, May 28th, 1887, p. 604), the mortality since 1879 has been reduced from 30 to 9 per cent., in cases of the largest stones. There is less danger if the operation be simply prostatotomy, than for large stones, and with antiseptic precautions there is every reason to believe that the danger may be still further reduced by galvano-caustic prostatotomy.

The foregoing shows a decided progress in prostatotomy, and I believe I am justified in regarding galvano-caustic prostatotomy by the supra-pubic method a further advance, for which I propose the following *modus operandi*:—

Prepare the rectum and introduce the rubber water bag, which is dilated with sufficient water to bring the bladder upward and forward. The bladder may be injected, but, in most cases, this will be impossible, as the operation is performed when the urethra is impassable. The opening in the abdomen is made in the usual way, through the linea alba and avoiding the peritoneum. When the bladder is reached, sutures should be introduced, to be used as retractors, to avoid inflammation. As the bladder could not have been emptied before, the urine is now drawn off by a trocar and canula, and then through the canula the bladder is well washed out and disinfected. Then the bladder is opened, and the prostatotomy performed, either by the galvano-cautery sling or burner. Drainage is established, and a tube introduced for after treatment and washing out of the bladder, to combat cystitis. Further details of the operation are omitted, as such can be found in recent articles on the subject of supra-pubic cystotomy, etc.

The advantages of this operation must be considered in two parts: first, supra-pubic method over perineal section, and, second, galvano-cautery in preference to the knife (or scissors).

I. *Advantages* of the supra-pubic operation above the perineal section are:—

(1) The ducts and ureters are not wounded.

(2) Perineal fistula is prevented.

(3) The operator sees what he is doing, and does not work in the dark.

(4) The after treatment is easier, and cystitis can be better combated.

The former drawbacks in the supra-pubic operation are now much reduced, and, with care, the peritoneum will not be wounded, nor infiltration allowed, and antiseptic precautions will prevent sepsis.

II. The advantages of galvano-cautery above the knife are :—

(1) That it avoids hemorrhage, also secondary hemorrhage.

(2) Leaves no raw surface exposed.

(3) Heals better, and

(4) Avoids septicæmia.

The statement of some reporters, that in prostatotomy with the knife hemorrhage does not take place, cannot be accepted, as the history of cases shows that primary, as well as secondary, does occur, both of which are entirely avoided with galvano-cautery.

Such an operation may be decided upon according to circumstances, but is imperative when the patient's death is certain without it. This operation gives him at least ninety-one per cent. of chances for a new lease of life.

ABDOMINAL SUPPORTER OF DR. DEBACKER, ROUBAIX, FRANCE.

SUPPORTEUR ABDOMINAL DU DOCTEUR DEBACKER, ROUBAIX, FRANCE.

ABDOMINAL-BRUCHBAND VON DR. DEBACKER, ROUBAIX, FRANKREICH.

IN THE ABSENCE OF ITS AUTHOR, PRESENTED BY THE SECRETARY.

The supporter differs from all others in that it is very thickly padded. It terminates inferiorly in an angle and at either side by a projection similar to an ordinary truss.

The effect of the supporter is to sustain the intestines which tend to press upon and bear down the uterus. It acts similarly in relieving the lateral ligaments of the uterus, distended during pregnancy, thus enabling them to regain their normal elasticity. It also supports the muscles and skin over-distended during pregnancy.

The supporter has met a favorable reception at the hands of many Parisian surgeons, who recommend it to their clients.

The superiority of the supporter depends upon its simplicity, its receiving its support from a point behind higher than usual, and its grasping the abdomen like a hand just above the pubes.

It may be worn under all clothing.

We recommend it in all cases where support of the uterus is required, either on account of need of rest, or from anteversion, chronic metritis or constipation of mechanical origin.

We also recommend the supporter to all women compelled to stand much of the time, to carry heavy weights or to make long journeys on foot.

A FEW POINTS IN THE PATHOLOGY OF THE PROSTATE GLAND—ILLUSTRATED.

QUELQUES POINTS ILLUSTRES DANS LA PATHOLOGIE DE LA GLANDE PROSTATE.

EINIGE PUNKTE IN DER PATHOLOGIE DER PROSTATA.—ILLUSTRIRT.

BY A. H. WILSON, M. D., M. R. C. S. ENG.,

Of Boston, Mass.

MR. PRESIDENT AND GENTLEMEN OF THE SURGICAL SECTION :—

Before presenting these illustrations of the Prostate Gland, as we find it after it has undergone pathological changes, which I am enabled to do by the kindness of my friend, Dr. S. M. Nelson, of Boston, who has made and mounted the sections from which these photographs have been taken, a brief glance at the normal structure will, I think, be profitable. Let us then examine the floor of the prostatic urethra. (Illustration No. 1.) We find it beginning posteriorly at its vesical boundary, in a rounded prominence in the floor of the neck of the bladder, called the uvula vesicæ ; from this point forward the floor expands laterally and presents a hollowed-out appearance ; this hollow being

FIG. 1.

FIG. 2.

divided into two parts by an elevation in the centre running longitudinally forward, called the Veru Montanum. This elevation gradually rises until it reaches its highest point (about one-eighth of an inch) near the anterior boundary of the prostatic urethra, when it somewhat suddenly slopes down, and leaving the prostatic, enters the membranous urethra. On the anterior slope of this eminence we see the opening of the utricle, or uterus masculinus, and opening on the lateral walls of the utricle we see the mouths of the ejaculatory ducts. Along the floor of the prostatic urethra, upon each side of the veru montanum, we see the mouths of the ducts of the lobules of the glandular element of the prostatic body. Turning to the next illustration (No. 2),

we have a longitudinal section showing the relative amount and position of the glandular structure, together with the manner in which the glandular lobules are separated from each other and surrounded by an inter-glandular stroma of unstriped muscular, vascular, nerve and connective tissue. On looking at that part of the prostate lying in front and above the urethra, we notice an entire absence of the glandular structure, this portion of the organ being entirely muscular. We get also a lateral view of the ejaculatory duct, lying in a somewhat wider band of inter-glandular stroma, and terminating by running forward to the utricle, which is also very well shown in lateral section. The component structures of the prostate may be traced from within outward, except that the two layers of unstriped muscular tissue underlying the mucous membrane are not to be distinguished from each other, thus reducing the number of structures to four; namely, mucous membrane, submucous muscular tissue, glandular tissue and inter-glandular stroma, and finally the capsule proper.

Illustration No. 3 shows a longitudinal section of the right lobe of an hypertrophied prostate magnified about ten diameters. This illustration shows an hypertrophy of the glandular element at the expense of the inter-glandular or stromal portion of the pros-

Fig. 3.

tatic body, and would be diagnosticated during life by an increase in size together with diminished resistance on making an examination per rectum, with or without a sound in the bladder.

Illustration No 4 shows the same characteristics, but being magnified to a higher degree (about fifty diameters) shows the histological changes in the glandular structures more markedly.

Illustration No. 5 shows the changes that have taken place in a carcinomatous prostate. Here we see the whole section of the prostate infiltrated with cancer cells, the glandular and stromal elements being alike almost undistinguishable.

Illustration No. 6 shows a section of the prostate which is completely infiltrated with pus cells.

My purpose in presenting these illustrations is to contribute in some slight degree, if possible, to the more exact diagnostication of the actual change in the structure of the prostatic body which may have taken place in a given case, to the end that our plan of treatment may be founded upon a somewhat more exact and definite idea of what tissue we desire to modify and how we propose to change it. My friend, Dr. Robert

FIG. 4.

FIG. 5.

FIG. 6.

Newman, of New York City, has presented to this Congress a valuable paper giving a résumé of his treatment of several cases of enlargement of the prostate with the galvano-cautery sound, and the results he reports would seem to indicate that in all his cases the treatment was efficient, and that at last he had discovered a method of dealing with that until now universally acknowledged incurable condition. I am, however, desirous of learning whether the galvano-cautery sound produces a shrinking of glandular, connective, and muscular tissue alike, so that for all practical purposes it is immaterial whether we are dealing with a case of glandular or of muscular hyperplasia. Without definite proof, however, I think we should be unwilling to assume that a galvano-cautery which would produce a shrinking of the soft cellular tissue would be likely, when applied in the same manner and of the same strength, to be followed by the same changes in the case of firm stromal growth, and where the glandular structure was to a greater or less degree obliterated by pressure. Harrison, of Liverpool, has found atrophy of the prostate to follow tapping of the bladder through that body, a procedure which he recommends in certain conditions of urinary retention. He thinks a new action is set up in the gland, similar, perhaps, to that produced by a seton, which results in its atrophy. He has also noticed atrophy to follow the incision of the prostate in lithotomy. This has been the experience of other surgeons, and it would seem that in those cases of prostatic enlargement dependent upon hyperplasia of the unstriped muscular element of the organ, an incision dividing deeply these muscular fibres and placing them absolutely at rest would have the usual effect of disuse of muscular fibre, namely atrophy. On the other hand, where the enlargement is caused by an increase of the glandular constituent, I have no doubt the galvano-cautery sound would operate efficiently.

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